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ORIGINAL ARTICLES.

PAGE

Ashby, Henry, Note on the Use and Abuse of Chlorate of Potash in the Treatment of Disease 25

Ashby, Henry, On the Value of Milk Whey in Infant Feeding . . 389

Balfour, Andrew, and Porter, Charles, A Research into the Bacteriology of Typhus Fever: Preliminary Notice (two figures in text) . . 141

Beevor, C. E., A Case of Abdominal Aneurysm bursting into the Psoas Muscle, and subsequently into the Left Pleural Cavity . . . 42

Berry, G. A., On the Treatment of Some of the More Common Eye Affections 333

Brockbank, E. M., The Causation and Rhythm of the Crescendo Murmur of Mitral Stenosis (four figures in text) 236, 341

Caton, Richard, On the Prevention of Valvular Disease of the Heart . 329

Crookshank, F. G., Asthma Sexualis and its Occurrence in the Insane . 588

Croom, J. Halliday, Vaginal Hysterectomy for Cancer . . . 249

Croom, J. Halliday, On Acute Mania following Simple Ovariotomy . 470

Ferguson, James Haig, Repeated Ectopic Pregnancy in the same Patient, with Account of a Case where two Gestation Sacs (one a Lithopædion) were successfully removed from either side at one operation. (Plate I. and one figure in text) 145

Fothergill, W. E., The Function of the Decidual Cell (seven figures in text) 265

Gillespie, A. Lockhart, A Visit to a German Institution for the Treatment of Pulmonary Disease 484

Greenfield, W. S., Clinical Lecture on a Case of Typhus, and Some Lessons to be Drawn from it: being especially a Plea for the Increased Application of Bacteriological Science to Practical Medicine . 378, 492

Haultain, F. W. N., A Retrospect of a First Series of One Hundred and Ten Abdominal Sections for Pelvo-Abdominal Disease (four figures in text) . 105

Jones, R. Llewelyn, and Clinch, T. Aldous, Suppurative Cholangitis (three figures and one chart in text) 363

Lackie, J. Lamond, Puerperal Hyperpyrexia, with an Illustrative Case . 585

PAGE
LITTLEJOHN, HARVEY, Medico-Legal Cases 473
LITTLEJOHN, HARVEY, and KER, CLAUDE B., An Outbreak of Typhus Fever (chart in text) 555
LUCY, R. H., The After-Treatment of Cases of Gastrostomy . . . 46
M'BRIDE, P., The Treatment of Ozæna, with special reference to Cupric Electrolysis 217
MACKENZIE, W. LESLIE, The Hygienics of Milk . . . 372, 563
MAGUIRE, ROBERT, The Involution of Life—A Scheme of Pathology . 1, 119
MAYLARD, A. ERNEST, Erasion *versus* Excision in Childhood ; or, the Remote Effects of Excising or Erasing Tuberculous Joints in Children . . 577
MORISON, ALEXANDER, On the Relation of the Nervous System to Disease and Disorder of the Viscera (fourteen figures in text) . . 28, 151, 224
OSLER, WILLIAM, Chronic Splenic Enlargement, with Recurring Gastro-Intestinal Hæmorrhages 441
RANSOM, W. B., The Vis Medicatrix Naturæ . . . 12, 129
ROSS, T. A., Chloroform Sickness 591
STEWART, J. PURVES, Clinical Considerations in Chorea . . . 257
TIRARD, NESTOR, The Treatment of Renal Dropsy 545
WARDEN, A. A., Instillations and their Value in the Treatment of Cystitis . 50
WARDEN, A. A., Urinary Antisepsis (two figures in text) . . . 273
WILSON, G. R., The Inebriates Act of 1898 254
WOODHEAD, G. SIMS, Tuberculosis and its Prevention. (Plate II.) . . 453
YULE, GEORGE P., Perforation in Enteric Fever, with Notes of Ten Cases . 358

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ORIGINAL COMMUNICATIONS.

THE INVOLUTION OF LIFE—A SCHEME OF PATHOLOGY.[1]

By ROBERT MAGUIRE, M.D., F.R.C.P., *Physician to Out-patients and Joint Lecturer on Pathology, St. Mary's Hospital; Physician to the Hospital for Consumption, Brompton, London.*

THE science of pathology at the present time is scarcely anything more than a huge mass of details, with little to bind them together, and yet there must be some general law which governs all diseased processes, and the changes which result from them. To lay down the terms of such a law is a work which can be approached in only a diffident spirit—first, because of the importance of the task; and, secondly, because of the difficulty in knowing, in the present rapid progress of research, whether the facts at our disposal are as yet finally and perfectly observed. Yet it is permissible to make such an attempt, if for no other purpose than to serve as a means of arranging our knowledge.

We must, in the first place, recognise a universal error of physicians and pathologists. At the bedside the physician uses every endeavour to discover the actual state of each organ, and the manner in which it is doing its work. He names the morbid states and actions, calls this his diagnosis, and says and thinks that this is the disease from which his patient is suffering. But it is not the disease at all; it is only the effect of the disease. Similarly, the pathologist names as the disease the macroscopic and microscopic changes which he sees, and the morbid processes of which he has obtained evidence; and again these are not the disease, but its effects. If we are to formulate a general view of pathology we must look further, and ask ourselves what more remote cause might have led to these effects; and a consideration of this is likely to lead us to a primary process, which, varying in its methods and ways of action, will produce so many different states.

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[1] An Introduction to the Course of Special Pathology at St. Mary's Hospital.

In the case of healthy organs and functions, this has already been done; and widely differing as are the anatomical and physiological conditions of the individuals constituting the animal and vegetable world, yet the law of evolution has now, I think, been universally accepted as an explanation of how these differences are due simply to the working of one common influence, under varying surroundings and in varying strength. It is the presence of that influence which makes the difference between an inert mass of protoplasm and the cytode, and the respective amount of that influence which separates the cytode from the most highly developed man. We must have a name for the influence, and at present we can find no better than vitality. In using the word for our present purpose, we must divest it of all the mysticism with which it, or its equivalent term vital essence, was enveloped by the astrologers, alchemists, and charlatans generally. It is unnecessary for us to inquire into the nature of vitality, or the manner in which it got its origin. We know that heat and light are forms of motion, but we study heat and light as separate energies, knowing well that they are not elemental. We study electricity as if it were an elemental energy, though it is certain that some day we shall know its true nature and be able to analyse it as now we can synthesize it. Similarly, it is allowable to consider vitality as an entity—there are objections to calling it an energy—knowing nothing of its true nature, and unable even to guess its relation to the known forms of energy. Yet there it is, a distinct and very active influence, possibly compound in its nature, which is, at least, an agent for the transmutation of the known forms of energy, which requires the presence of those energies for its existence and increase, but into which, so far, those energies cannot be artificially transmuted. Moreover, while its quantity differentiates the highest from the lowest organism, we must recognise its agency in the development of the various parts of the highest organism, the presence of more and more vitality changing the simple forms of embryonic life into the elaborate structures of the adult, and the general reflex or imitative functions of the infant into the specialised efforts of the skilled artist or great thinker. We must consider the most highly developed organ, the most complex action, and the greatest idea, as consisting merely of the simplest form of each, plus vitality. The manner in which all this has been arranged by Nature has been worked out in detail by many writers, but described as a whole by Herbert Spencer, who stated, on the publication a few years ago of the last volume of his "System," that, after thirty years of labour, he had finished his work. Yet he had dealt only with Nature in its progressive or normal aspect, and had left almost untouched the retrogressive or abnormal processes of nature with which we physicians have elected to spend our lives. Just as the repeated addition of vitality to the simplest forms leads to their highest development, so the subtraction of vitality from the highest forms brings forth others of lower development, and causes what we name disease.

It would be easy to reduce this to an absurdity, and say that it simply amounted to the statement that a part is diseased because it is not healthy. But this would be a misunderstanding of what is meant by vitality. Let us use the word exactly in the sense stated above, as naming a definite influence, and try to formulate a law of pathogenesis from a consideration of the occurrences of disease as they are known to us. We may then state a law of pathogenesis as follows:—

Disease is caused by a lack of vitality, general or partial, sudden or gradual, stationary or progressive, having as its effect on tissues the substitution of the lowly organised for the highly organised; on functions, the substitution of the less specialised for the highly specialised, the general for the less specialised, and the automatic for the general, until absolute loss of vitality leads to death of either a part or the whole, and total abolition of function.

Before proceeding to illustrate the working of the law, it will be necessary to explain it somewhat. The first portion explains itself. The second portion, that relating to the effects, is a corollary to the theory of evolution, as already stated. Since the higher is developed from the lower, by the addition of vitality, it would follow that the subtraction of vitality from the higher should reduce it to the lower, and this is practically what is above stated. The first formed tissues of the embryo are lowly organised, and very simple in structure. With the addition of vitality, they become more and more highly organised. If by chance the supply of vitality be insufficient, they do not attain the high grade of development proper for the individual of that species, but will nevertheless develop to a certain degree, and form a less highly organised structure. If the vitality of the highest organised tissue be simply diminished, the tissue need not revert to a purely embryonic structure, but may assume the character of such a more lowly organised tissue as can be produced by the supply of vitality at its disposal. Similarly, the first acquired functions are automatic, and require little vitality for their production; more vitality produces general functions, still more the specialised, and again more the highly specialised. Take away a portion of vitality from the highly specialised, and there is no need to revert to the automatic at once. The quantity of vitality remaining is sufficient to produce the less specialised; and so on, by further and further subtraction of vitality, we reach the general and the automatic. By absolute removal of vitality we reach death, and the whole or its parts undergo further changes, according to the laws of another science than that under discussion. In the case of animal bodies, bacteria then play their part unhampered by the vitality of the tissues, and indeed, as will be shown later, exert a similar action when the protective vitality is simply diminished in amount, and before entire somatic death has occurred.

Again, we must have a clear understanding of what is meant by disease. Clinicians include in this term everything which is a departure from the normal, and probably for practical purposes this is the best position to take. But to do so would not agree with the views of the most modern pathologists, who would, for instance, consider inflammation, in its numerous aspects, as not a diseased condition, but an example of the process of chemiotaxis, and an active effort to protect the organism from injury. This is a very new theory, not yet fully established, and possibly to be overthrown in the future; but the authority of those who hold it is of such great weight, and the facts discovered by them of such prime importance, that the opinion must be considered in any general discussion. Further, there are certain congenital malformations which are due to intra-uterine disease, and others, again, which are caused by lack of developmental power, and which are not generally considered to be diseased states. These latter conditions may be classed apart, but they nevertheless only differ from ordinary diseased conditions in the fact that they are caused, not by a subtraction of vitality from an already perfect body, but by a primary lack of vitality causing imperfect development.

Also, we must differentiate clearly the direct and the indirect effects of disease. In the nervous system, Hughlings Jackson, especially, has shown that the higher portions of the system, "higher levels," as he calls them, have not only the functions peculiar to their own anatomical and physiological positions in the scale, but exert a control over the lower portions, proportionate to their higher development. Disease of these "higher levels," not only causes symptoms due to lack of their own inherent functions, but also others due to the withdrawal of control over the "lower levels," and consequent irregular action of the latter. There can be little doubt that similar control action is exerted in other parts whose connection with the nervous system is more supposititious than proven. We might then, to meet these different views, classify departures from the normal as follows:—

(1) Imperfect developments, without a noxious agent; (2) the direct effects of a noxious agent; (3) the indirect effects of a noxious agent; (4) the effects of attempted removal of a noxious agent.

Of these, (1), (2), and (3) are the results of an initial or acquired lack of vitality, while (4), inserted to meet the views of the most modern pathologists, would by them be considered not disease at all, but a reaction of the tissues, and the result of high vitality.

Let us now examine examples of morbid changes, selected as widely as possible from the processes of general pathology, and most of the various systems into which we divide the body for physiological and clinical purposes, and see how they agree with the law of pathogenesis, expressed above.

General pathology.—If we recognise the principle of control over the lower organised structures, exerted by the highly organised, as shown clearly in the nervous system, the application of the law to general pathology is free from much difficulty. Yet there *is* a difficulty when we consider inflammation from the standpoint of recent research. But let us first take the older view, as expressed, for instance, by Burdon Sanderson's definition, "The process of inflammation is the succession of changes which occurs in a living tissue when it is injured, provided that the injury is not of such a degree as at once to destroy its structure and vitality,"—a definition, however, which was admittedly not perfect. Now "the succession of changes" therein referred to has as its result the return to an embryonic state, in many respects closely resembling the tissue of a sarcoma, and the cause of this return is a diminution of vitality, short of the absolute destruction of vitality mentioned in the definition. (It will be observed, of course, that in speaking of "an embryonic state," I ignore those qualities of the young cells found in an inflamed part, which have been the subject of investigation in recent years. It may well be that the inflammatory cells, viewed in the light of their being coarsely or finely granular or hyaline, oxyphile, eosophile, neutrophile, or amphophile, macrophage or microphage, and so on, will be found to differ from embryonic cells, though having their origin in the latter. On this point we have as yet but little precise knowledge. Taking, however, the general character of an inflamed part, its resemblances to an embryonic tissue are obvious.)

But it will be asked, why does an injury causing lack of vitality produce so apparently active a process as that of inflammation? The answer is found in what occurs in the case of the nervous system, as detailed below. The inflammation, in this case, corresponds to the excitement and excess of animal functions seen in mania, and to the delusions of grandeur in general paralysis, when the higher control is destroyed. The fully developed, highly organised tissues keep the development and functions of the lowly organised cells under control. The former can resist a certain amount of injury, whether mechanical, chemical, or bacterial; but their vitality will at last give way, and as the first sign of this we have the riot—certainly an organised and purposeful riot—of the lowly organised cells and functions, shown in the form of inflammation. Development from that embryonic state may once more take place even to the reproduction of the original tissue (we need not here enter into the question of whether such reproduction is only from remaining cells of such tissue or from young indifferent cells—the point is not yet entirely settled); but more commonly the vitality necessary for this complete process is wanting, and the part must be content with a development into the more lowly organised fibrous tissue, in the form of a cicatrix. This is especially seen by the surgeon in the case of ununited fractures. The bones being properly set, the embryonic tissue produced by the first irritation should give place in time to true bone, uniting the fragments; but if the patient be in bad general health—a condition of general lack of vitality—or if constant movement and irritation cause further local lack of vitality, only fibrous union will ensue.

But, as you have heard in the course on General Pathology, the researches of Metchnikoff and his followers have thrown an entirely new light upon this subject, especially when the noxious agent is bacterial. It has been shown that there is an attraction of cells of various characters and functions to the noxious agent by a process called chemiotaxis, for which a much more widely spread influence is sought. Some of these cells have the power of merely shutting off the noxious agent; others of weakening it by extracellular action; others, the phagocytes, of enveloping and digesting it. Thus, it is asserted that inflammation is not a disease at all, but entirely a protective process. It is impossible to follow out this subject practically when one is engaged also in clinical work, and it would be impertinence to criticise it with imperfect knowledge. Still, it is allowable to say that we have certainly not yet had put before us such complete proof of chemiotaxis and its associated theories as will warrant their universal acceptance, and it is very difficult to apply the principle of chemiotaxis to slight and even subcutaneous injuries equally with the virulent injury caused by bacteria. And, even accepting the principle, it is not impossible to apply the law stated above to the condition produced by inflammation. That condition, whether caused by chemiotaxis or not, is a departure from the normal to a state which is not so highly organised; and the process itself, apart from the injury which causes it, is capable of doing damage to the fully organised normal tissue. In the same way, too, I think it could fairly be contended that any tissue or process presenting chemiotaxis is not in so highly organised a state as the original tissue or process, perfectly suited as it is for its normal work. We must beware, however, of ignoring facts in order to support a theory, and the advocates of the chemiotaxis view of inflammation remove difficulties from our way by asserting that the process is not a disease at all. Hence we may include it under the fourth head of departures from the normal, namely, "effects of attempted removal of a noxious agent."

The various processes known as degenerations obviously conform to the law. They, as their name imports, consist in the substitution of a more lowly organised substance for a more highly organised, the intimate processes by which this is brought about being a matter of research for the histochemist, and as yet almost unknown. But whatever may be the method by which mucin, fat, glycogen, or amyloid substance is substituted for healthy proteid protoplasm, the essential of the process is a lack of the vital energy necessary to retain the protoplasm in its healthy state, or to renew it in its perfect form when exhausted by its work. There have been fitful attempts at times by a few pathologists to obtain recognition for a fibroid degeneration—attempts, which I think have not been sufficiently sustained, although the word degeneration is here mal-applied. It is still considered by many, and I think erroneously, that fibrous tissue morbidly placed is always the direct result of inflammation. Whether it is really ever so, is, I think, a matter of doubt; but there are certainly instances in which the deposit of fibrous tissue in abnormal amount is quite independent of inflammation. Most markedly is this seen in the pathological histology of the nervous system. In locomotor ataxy we find fibrosis of the posterior columns of the spinal cord, a growth of fibrous tissue, which is probably a hyperplasia of the neuroglia, at the expense of the special nervous elements. But even at the spreading border of such a sclerosis, we see no evidence of inflammation, but only the gradual increase of the fibrous tissue. The actual process is due really to the diminution of vitality of the whole part, the result, in this case, usually of syphilis; growth still goes on, but there is not enough vitality to produce the highly specialised normal tissue, but only the lowly organised fibrous tissue.

Tumours again, and especially those of a malignant nature, are results of the same process. In the carcinomata we have a growth of lowly organised epithelioid cells at the expense of the more highly organised special tissue, and in the epitheliomata we often find what I think is evidence of the giving way of the vitality of the latter tissue, in the small-celled infiltration which underlies the growing mass of epithelial cells. Such a reversion of development has a special tendency to occur where the character of a mucous membrane is changing, and where, we may assume, the cells are indeterminate as to the character they are to adopt. Thus we find carcinomata, commonly, at the edge of the lips, at the anus, at the pylorus, and in the œsophagus, especially near the bifurcation of the bronchi, the spot at which the separation of the œsophagus from the trachea in embryonic life is last effected. In the sarcomata we have a transformation of the tissues into an embryonic type, an embryonic tissue incapable of further development. The process is active enough, and causes increase of bulk of the part; but the whole tumour is of the lowest organisation, and has not even sufficient vitality to maintain its own cells in their perfect form, hence the mucoid degeneration which is so common.

The confused facts and theories accumulated on the subject of fever during the last half century have recently been admirably criticised and arranged for English readers by Burdon Sanderson in a short and masterly article.[1] M'Alister has urged that we have thermogenic, thermolytic, and thermotaxic functions, and some say there are nervous centres for each. By the disturbance of one, other, or all of these, increased body heat and its accompaniments are produced. Practically, only two main theories exist as to how this change occurs, namely, that which asserts that the nervous system is primarily affected by the pyogenic agent, and that which supposes that the same agent causes a condition of the tissues which renders them more easily metabolised. The latter was formerly put forward by Burdon Sanderson himself, but now he does not so strongly support it. Again, he shows that the greater part of febrile phenomena are probably caused by disorder of thermolysis. But at the bottom of all these analyses, whichever view we adopt, there is a destroying of control, or, as one may put it, a diminution of the vitality of the higher controlling power, and therefore ascendancy of the lower functions in greater or less degree.

Nervous system.—The simplest example in the nervous system of the working of the law is so-called serous apoplexy, an incident in Bright's disease, and often considered to be of uræmic origin. Nevertheless, it is an organic disease, an exudation of serum into the cavities and interstices of the brain, and the result of high arterial tension. The disease affects all parts of the brain, and apparently equally, as regards its anatomy, and causes coma and death; but before the coma is perfect it has occurred to me on several occasions to find a right-sided hemiplegia. In one case only, after observing the hemiplegia, I was able to make a post-mortem examination, and then found general serous effusion, but no special affection of the left side of the brain. The disease had affected the brain generally; but, in obedience to the law, the more highly specialised left side of the brain had given up its functional power, while the less specialised right side had still been able to continue its work for a time.

We must now consider that most important feature in the nervous system, insisted upon by Hughlings Jackson and referred to above, that the more highly specialised parts—the "higher levels"—not only have their own peculiar functions, but exert control over the less specialised parts. Therefore, when the higher parts fail from lack of vitality, not only do we lose their own special functions, but the lower parts run riot, and we have an apparent excess of functional activity caused by lack of vitality. It is made clear, however, by Hughlings Jackson, that we must not be deceived by such over action. The total result of the action of the nervous system in such conditions is, as it were, a loss of quality, with an apparent increase of quantity, and, on the whole, a decrease of functional activity.

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[1] Allbutt's "System," vol. i.

The most typical example of this we find in acute mania, a disease in which the higher functions of the brain are in abeyance, but where the lower functions are most active. Even without apparent delusions, the patient is noisy, violent, extremely restless, the animal propensities are unchecked, and the control of the organic powers is lost, as evidenced by the frequent pulse, the high fever, and the ultimate loss of strength. Yet the prognosis in such cases is not bad; under treatment, the activity of the lower functions abates, and the higher functions gradually resume their power. Bad results do, however, occur, either from the violence of the lower functions causing asthenia and death, or from the lack of vitality extending from the higher functions to the lower functions which were at first so active, and causing dementia, in which the patient simply lives and no more, possessing merely automatic actions. Observance of such cases shows, in the most interesting manner, the decline of the functions, in the order named in the law.

What is known as general paralysis of the insane, is not really a definite disease. It is only an assemblage of symptoms which, as seen in asylums, progresses and ends in death. It is not an organic disease, though organic changes may be found post-mortem as the result of the morbid processes which have persisted for so long, and the alienists are now almost agreed that its almost invariable cause is syphilis. In this condition, as in mania, there is deterioration of the mental powers, leading finally to dementia; but the progress of symptoms illustrates a portion of the law, in that even almost to the end the failure of vitality in the mental powers is often only partial, and such ideas as the patient may have are happy and exaggerated. But the "delusion of grandeur," so typical of the condition in its earlier stages, is an excellent illustration of the law, and of the effects produced by the lower functions running riot when the higher functions are inactive. Such delusions are not confined to general paralysis of the insane, as known by the alienists. We all of us have them at times in our dreams. We may express the highest function of the intellect as the correct appreciation of the correlation of facts, and in our dreams, as well as in the delusions of the insane, this highest function is lost. One of the commonest of dreams, is that in which a man imagines that he is taking a jump, but in the actual process of jumping his powers seem to be ever and ever renewed, until he has covered an impossible distance in his jump —or wakes, to recognise the absurdity. Here, light sleep has paralysed the highest functions, allowing the lower functions to act unchecked, and the same process exists in the general paralytic, with the exception that here, according to the alienists, the highest functions never resume their power. We see the same thing too in the horse, who loses his highest power of judgment, when excited. Thus, whilst cantering, he will gather himself and jump a crack in the ground, which while walking he will merely step over. He has a delusion of grandeur, and "schooling a horse at his jumps" as it is called, is merely teaching him to observe his "correlation of facts," even when excited—a very desirable thing for other persons. Man, in his otherwise sane state and in his waking moments, especially if he be a vocalist, is subject to a similar delusion, the result of conceit; he will attempt and believe he has performed feats of which he is quite incapable. So, the general paralytic, having lost his highest power of correctly appreciating the "correlation of facts," and believing that he can jump over the moon, will actually jump a foot high for a distance of a yard and assert that he *has* jumped over the moon.

The melancholia, which is often a marked symptom of general paralysis of the insane, is similarly explained, and similarly is present from the same cause, as an accompaniment of less seriously disturbed states of the system. Again, we have to deal with a loss of the highest power, "the correct appreciation of the correlation of facts"; and, consequently, facts or thoughts of facts, which, viewed in their correct correlation, would have no such effect on the mind, now produce melancholia.

In another matter, also, a comparison of general paralysis with acute mania illustrates the selection which the lack of vitality may make in its choice of parts. In general paralysis the whole of the higher mental powers is not affected, as in mania; while on the other hand the bodily powers, really of low development, are at first untouched in mania but very soon affected in general paralysis.

But in dreams we have a reproduction of only one symptom of general paralysis, while in medical practice we find the whole of the symptoms so produced that I think we ought to take a broader view of the disorder than is taught by the asylum physicians. Thus we have at times general paralysis in its typical form, and progressing to its fatal end, caused by lead poisoning in the absence of syphilis. I have seen two cases in which no other diagnosis than general paralysis could be made, so well marked were the symptoms, where the cause was influenza, and the patients completely recovered under the administration of phosphorus. One hears at times of the difficulty in diagnosis between alcoholism and general paralysis, but I think this is misdirected thought, for the disease, or rather collection of symptoms, is the same, the cause only being different. In all these conditions we have simply a poisoning of the highest cerebral functions by syphilis, lead, influenza, or alcohol respectively. The effects of the latter two poisons can be removed, but those of the former two are permanent and progressive, and lead to death. Again, I have met with two cases—and those engaged in work amongst the insane must have seen more—in which the symptoms of general paralysis were distinctly intermittent. I do not allude to the well-known tendency of general paralysis to improve for a time and then relapse, but to distinct recovery lasting for a long period, and this repeated several times. Here the highest functions recovered their vitality, at least temporarily.

If we consider now some of the nervous diseases characterised by motor disorders, we shall find that they are easily explained by the supposition of lack of vitality of the higher functions and loss of control. This, in epilepsy, has been made clear by Hughlings Jackson. The bromide treatment of epilepsy is anything but rational, and seems to me only a compromise when the actual disease of the highest centres is beyond treatment, and the riot of the lower centres so pronounced and habituated, that we are obliged to ignore the seat of the real disease, and merely attempt to quiet its symptoms. In epilepsy, occurring for the first time late in life, we have a chance of using a better treatment with effect, and of really curing the lack of vitality of the higher controlling centres. Thus I have two patients under my care now in the out-patient department, whose epilepsy is better checked by phosphorus and quinine, respectively, than by bromides.

Chorea is a disease which has resisted all attempts to demonstrate its organic origin. Is it not a functional disorder due to destruction of the vitality of the higher control centres by the rheumatic poison, and cured by arsenic, a well-known nerve tonic? The picking out of certain portions of the control mechanism will cause variations of the irregular movements, both as regards their localisation and their character, and thus we have post-hemiplegic chorea, athetosis, and the condition known as pseudo-sclerosis, of which I have met with two examples, in one of which typical symptoms of disseminated sclerosis were present, but a careful post-mortem examination made by modern methods failed to find any organic disease of the nervous system.

Lastly, I may mention the Cheyne-Stokes respiration, which elsewhere[1] I have endeavoured to show is due to a "stormy unbalanced action of the respiratory centre, due to a depressed vitality of itself and of its higher cerebral controlling mechanism." I need not here again review at length the arguments which point to this conclusion. Shortly, they are as follows:—The respiratory centre has an inherent tendency to rhythmic action, probably assisted by inhibitory impulses coming from the distended lung, and carried principally by the left vagus. It is controlled also by impulses from the brain cortex, and experiments show that these also sustain the rhythmic action, and without them the cells of the centre would act tumultuously and irregularly. In the frog, with low cerebral development, there is normally a periodic grouping of the respiratory movements, and Sherrington has shown that removal of the hemispheres and thalami produces Cheyne-Stokes respiration, while the results of other experiments tend similarly. Yawning is an irregular action of the centre, brought about by fatigue of the higher centres, and involuntary sighing has the same causation. In sleep, the higher cerebral functions are in abeyance, and sometimes in adults, frequently in children whose higher control is not yet perfectly developed, and also, as I have observed, in an idiot, there is a periodic breathing during sleep, which resembles the Cheyne-Stokes phenomenon. Finally, Cheyne-Stokes respiration only occurs in disease, when there is great depression of the higher cerebral faculties and very frequently unconsciousness. For these reasons we are led to the conclusion that the mechanism of the Cheyne-Stokes phenomenon is as stated.

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[1] *Clin. Journ.*, London, 19th Jan. 1898.

(*To be continued.*)

THE VIS MEDICATRIX NATURÆ.[1]

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Gentlemen,—It cannot have escaped your observation, for indeed it is plain to the man in the street, that the numbers of the medical profession have grown and are still growing to a large and perhaps an alarming extent. Yet, in spite of this, you will note the remarkable fact that the population of our earth still grows larger, and by its irrepressible increase causes still more alarm in the prophetic political breast.

If we glance from the ages of Hippocrates and Galen, through the days of Paracelsus and Harvey, to those of Jenner, Virchow, and Pasteur, we cannot but be struck by the apparent incompatibility of these two parallel facts.

We find how, in the elder days of the healing art, if there were some who accounted it a branch of philosophy, there were others of no mean weight who held that the reasoning faculty was foreign to the art of medicine; we find a dietetic school which cured by diet, a pharmaceutic which cured by drugs, and a chirurgic, which cured by manual operations. We find that treatment (like morality) was a question of geography; that one method was necessary at Rome, another in Egypt, and another in Gaul. We find some (oddly enough termed Methodists) asserting "that the knowledge of no cause whatever bears the least relation to the method of cure." We find empirics, we find dogmatics, and naturally we also find sceptics. Gentlemen, if we look around to-day, we must confess that philosophers are still few and empirics many; that dogmas and systems of physic still come and go; that diets of herbs and diets of flesh have each their adherents, their cures, and their critics; that drugs mineral, drugs vegetable, and drugs animal, have their day; that doses mount to the heroic or sink to the infinitesimal; that yesterday blood was withdrawn which to-day is injected; that pathology and treatment are still questions of geography and even of patriotism; that faith has its triumphs and scepticism its moments of exultation; but this we mark, that amid and in spite of all the experiments, all the imaginings and wranglings of the would-be menders of mortality, life still goes on, mankind multiplies, disease passes away, and the sick get well. But one conclusion is possible. Living beings, and man among them, must have some inherent force, some natural power of recovery from the effects of accident and disease. It is of this that I would speak to-night as the *Vis medicatrix naturæ.* Though the mechanisms by which living organisms resist disease and repair injury are varied and complex, yet fundamentally the *Vis medicatrix naturæ* is but one aspect of the *Vis vitæ*—the Energy of Life—by which they grow in size and multiply.

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[1] An Address delivered before the Medical Society of University College, London.

Though inorganic bodies such as crystals may grow by the deposition of molecules on their surface, it is only living matter which can attract and intercalate among its own the molecules of outside matter; it is only living matter which can reproduce its kind by the shedding of particles gifted with this power of growth. This power, this innate force, may, apart from metaphysical speculations, fairly be called the vital force—the *Vis vitæ.* Of its ultimate nature we know nothing, as we also have no ultimate knowledge of gravity, chemical affinity, and electricity, all of which forms of energy are manifested in the complex phenomena of life. Neither can we explain or foretell the unequal distribution of vital power, nor say why one small mass of protoplasm contains within itself the potentiality of life for a thousand years, while another is exhausted in a thousand hours. From one egg-cell came the forest tree of California, which shared our atmosphere with Solomon and still towers high to-day; from another not unlike it, and so far as we know of identical composition, the flower of last summer, which is now mouldering to dust. We do not even know why some forms of protoplasm, which we call vegetable, can absorb and endow with vital properties the molecules of non-living matter, while others, which we call animal, can but assimilate such matter as has lived.

The green-fly sticks its proboscis into the tissues of the rose-leaf, and sucks a steady stream of vegetable juices. From the other end of the insect there issues an almost equally steady stream of animal protoplasm in the shape of young aphides. But what change is worked in the brief passage through that minute laboratory we know not: within a millimetre of space is a world of metabolic mystery. Ultimate problems of life, vegetable and animal, are yet hidden from us. We can now but accept the facts of assimilation, growth and reproduction, as properties of the *Vis vitæ*, while we turn our eyes to the phenomena of defence, repair, and recuperation.

Amid the multitudinous influences of the environment to which living beings are exposed, it is not possible sharply to distinguish between the physiological and the pathological. The same physical or organised external agent may be to one organism healthful, to another harmful, or at different times to the same organism stimulating or destructive. Our pathological inquiry, therefore, trends upon the sphere of general biology. More especially does the mechanism of defence against external influences, and the means of adaptation to varying external conditions, concern the general biologist; nor can I here do more than lightly touch on these most interesting points.

Mechanical injury is widely defended against in both plants and animals by the development of armour of the most varied kind.

In *plants*, naked protoplasm is almost invariably clad in a stiff wall of cellulose, which in diatoms is strengthened by an imperishable siliceous test, while the wood and bark of trees form a barrier of defence to their delicate growing tissues. The attacks of animals are further guarded against in many plants by the growth of sharp spines from the epidermal cells. Generally speaking, plants are more dependent on armour than are animals. From their lack of mobility they have to sit behind fortifications.

In *animals*, the siliceous tests of diatoms are paralleled by the skeletons of radiolaria and of sponges, while the foraminifera and corals are strengthened by shells and skeletons of lime salts. Ascending the scale, we find the spiny shells of sea-urchins, the manifold shells of molluscs, and the chitinous skins of arthropods, though we should note that a soft body with a pliable skin (as in the octopus) may bear ruder impacts and greater pressures than many a mail-clad creature. In vertebrata, the tortoise and the armadillo, the hedgehog and the hippopotamus, give instances of defensive coverings, while the hair and feathers of others form no mean protection. Both animals and plants, moreover, largely defend themselves from the mechanical injury involved in being eaten by various external secretions, usually of slimy matter.

Glandular hairs abound in many common British plants, such as the catch-fly (*Silene*) and *Lychnis Viscaria*, which derive their names from their viscid secretions so fatal to insects. In some species of lettuce also, though the secretion is not poured out of the plant, impacts so delicate as the tread of an ant break the turgid cells of the surface of the involucral bracts, and the exuding latex snares the invader. In other plants, such as nettles, the hair-glands secrete formic acid, or even a virulent enzyme; while in the so-called carnivorous plant, the slimy secretion has acquired a new power, that of digesting animals caught by its stickiness, and thus the weapon of defence has become part of the mechanism of growth. In our common English sundew the adhesive force of the secretion is so great, that I have seen a dragon-fly firmly held by the glandular hairs of a single leaf. Similarly, among animals, many molluscs and fishes (*e.g. Myxine*) excrete from the skin a glutinous matter which forms an effectual protection. The thread-cells of hydra and medusæ, and the formic acid of ants, may also be mentioned, while the venom of serpents is a still further development. Here also may be noted the protection of some plants and insects by the formation within them of poisons, or by the secretion of strong-smelling substances which keep off would-be devourers. Thus far may be indicated the outer line of defence against mechanical injury and the attacks of beasts of prey.

With regard to other physical agencies—such as heat, light, drought, moisture, and chemical composition of the environment—the adaptability of living matter is no less extensive.

While there is a moderate range of temperature most favourable to life, yet great extremes are borne. Thus the protoplasmic movements in the cells of *Nitella Syncarpa* continue, even when the surrounding water is cooled to −2° C.; the alga, *Sphærella nivalis,* which forms the so-called red snow of the Arctic regions, withstands for months a temperature of −20° C., while insects (*Poduras*) and spiders, after being embedded all winter in glacial ice, spring again into life with the summer's sun. *Bacillus anthracis* is said to survive a temperature of −130° C., and yeast one of −100° C. At the other extreme, *Oscillatoriæ* flourish in the Carlsbad spring at a temperature of 53° C., and green algæ in the Solfatara at Naples, at a heat of 60° C. (140° F.), while it is now well known that some bacterial spores withstand boiling water for a certain time. In these cases the temperature of the organisms must approximate to that of the environment, but animals such as man, with a heat-regulating mechanism, endure even greater extremes. Thus Drs. Fordyce and Blagden remained some time in an atmosphere of 260° F. (which cooked an egg hard), and Chantrey's workmen entered ovens heated to 350° F.; while in the Arctic regions explorers often breathe, without discomfort, air at −60° F., or −70° F. (*i.e.* 100° F. of frost), and, as Captain Parry says, undergo changes of temperature of 120° F. in less than one minute without the slightest ill effect. The excellent health enjoyed by Nansen and his comrades in his last Arctic expedition, is another proof of the power of the human frame of resisting extreme changes of temperature and of the perfection of the mechanism for maintaining the body temperature approximately constant.

Light can similarly be endured in intensity, or dispensed with, by living beings: witness the flowers of the tropics on the one hand, the fungi of our cellar walls and the fish of the ocean depths on the other.

Water or abundant moisture is essential to the pond-weed and the rice-plant, the medusa and the frog, while great drought and heat are endured by lichen and cactus, by lizard and camel. The eggs of the water-snail shrivel up and die in a day of dryness, while those of a water crustacean (*Branchippus*) can be kept dry but alive for ten years. On the other hand, both plants and animals adapt themselves to great changes in water supply. The terrestrial plant *Polygonum amphibium*, with its erect stem and narrow hispid leaves, is transformed, by putting into water, into a form with floating long-stalked glabrous leaves, the former drying up. Many molluscs and crabs live almost indifferently in air or water; there are true fish (*Periophthalmus* and *Anabas scandens*) in which true gills may at times function as air-breathing organs; and the axolotl retains its gills and remains an aquatic animal while water is abundant, only replacing them by lungs when removed on to dry land.

The *chemical composition* of the environment, aerial or aqueous, may vary enormously without being inimical to life. Thus some bacteria flourish with abundance of oxygen, others in its absence, others with or without it; and it is possible, by gradual acclimatisation, to convert an aërobic into an anaërobic form, and *vice versâ*. Some plants and animals can only live in salt- others in fresh-water; though here again toleration of both may occur. The migratory fishes, such as the salmon and the eel, are familiar instances; but it has been shown that several fresh-water snails (*Lymnæa*, *Planorbis*) can be gradually accustomed to live in water containing 4 per cent. of salt—*i.e.* salter than the Mediterranean—while the edible mussel can similarly be induced to live in fresh water. Among crustacea, the fresh-water form (*Branchippus stagnalis*) has been produced by careful dilution of salt-water from *Artemia Milhausenii*, a supposed different genus which lives in saline lakes. While all air-breathing animals need the presence of free oxygen for prolonged life, and while adult mammals are rapidly killed by its absence, yet snails and frogs can live and exhale carbonic acid for many hours in an atmosphere of hydrogen or nitrogen, and quite young mammals can also do so for a shorter time. It is a too familiar fact that the bulk of our fellow-men love not "thin air," inhaling cheerfully atmospheres warmed and enriched by *esprit de corps*—atmospheres which the few of us breathe but to suffer from.

However, all this, you may say, is not the *Vis medicatrix*. *Cui bono* this *résumé* of elementary biological facts? To which I reply that it reminds us of the important but oft-forgotten elementary fact, that living beings are tough; have a good deal of life in them; and that a most important property of vital substance is an extraordinary latent energy or power of adaptation to very varied conditions. We cannot grasp the problems of disease and of recovery therefrom without perceiving, at least dimly, the wide expanse of health. Certain external conditions—physical and biological—are perhaps usual to an organism, and are spoken of as *normal*; but not all changes in these conditions are necessarily *abnormal* in the sense of being pathological. The organism can withstand, and may even benefit from, very extensive departures from the usual or so-called normal; and it is the non-recognition of this apparent truism that has led, and still leads, to much foolish doctrine and muddled treatment, in both medicine for the state and medicine for the individual.

In nature, we find living beings, not lying naked and helpless before the buffets of the outer world, but armed *cap-à-pie* for offence and defence, and with a goodly store of energy in reserve to cope with times of storm and stress. Let us pass on to the moment when the outworks are taken; when the enemy has got inside the fortifications and dealt a wound upon the organism within. And here it will be convenient to depart somewhat from the strict order of our investigation, and, before dealing with the mechanism of internal defence, to consider shortly the power of *repair after injury*, and the *restoration of lost parts.*

Speaking broadly, the lower in the scale of life the organism, the more complete the power of regeneration, and the more closely allied it is to *reproduction.* Unicellular organisms can survive the loss of very large portions of their protoplasm, so long as the nucleus is left intact, and it seems to be a general law that the nucleus is essential to life.[1]

Among metazoa, the hydra, as is well known, may be cut into several pieces, and each will grow into a complete hydra, giving an instance of an animal with definite specialisation of parts, but with each part retaining in it the potentiality of the rest. If you cut a slit anywhere in a hydra, from that slit will grow a ring of tentacles and a complete new mouth. Similarly, the completely separated basal portion can form an oral pole, and the mouth end a base. Thus we find repair of injury associated with multiplication of the individual.

In medusæ similar phenomena occur; while in the echinoderms one limb of a star-fish can re-form the whole animal, and the holothurian, when roughly handled, ejects his viscera, and then reconstructs them. Many worms can be cut into several lengths, each of which reproduces the complete animal with cephalic ganglia and sense organs, and many frequently multiply asexually by transverse fission.

In most higher animals, where the præpotent ganglia are collected at one end to form a well-marked brain, parts separated

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[1] Some remarkable experiments of Boveri seem to show that the nucleus of the egg-cell is not essential, being replaceable by the spermatozoon or male nucleus. Having enucleated the egg-cell of a sea-urchin, by shaking it in water, he fertilised it with the spermatozoon of another species, and obtained a dwarf larva showing purely paternal characteristics.

from the head do not regenerate the whole animal. The eggs of the higher animals retain, however, at certain stages of development, in each cell the potentiality of the whole animal. Thus, if the egg of amphioxus, after segmentation into two cells, be shaken so as to separate those two cells from each other, each will grow, not into a malformed amphioxus, but into a complete larva of half the usual size; if after cleavage into four cells, into four larval amphioxi, each complete but one-fourth the natural size.[1] Here at least there can be no early separation of Weismann's theoretical germ plasm and somatic plasm.

In adult arthropods, molluscs, and vertebrata, however, though considerable regeneration may occur, no new individuals are formed. Reproduction, whether sexual or asexual, has become a specialised process. The crab re-forms his lost claw, the snail his tentacle, the newt a leg and the lizard his tail, but the separated claw, tentacle, leg or tail, die.

In the higher plants, on the other hand, although formative tissues capable of growth and re-creation are more localised than in animals, it is common for isolated parts to reproduce the whole.

If we cut into a branch of rose or willow-tree, the bulk of the injured tissues take no share in repair. Only the cells of the cambium multiply and produce an indifferent meristematic tissue, which fills the wound as callus, and may become the mother-tissue of new buds or form new specialised tissues. But the callus does more; it not only heals the wound, cements the graft to the stock, but if a piece of the branch be cut right out and planted in the earth, it sends roots downwards and shoots upwards and reproduces a new tree.

The plant, though possessing highly specialised skeletal and formative tissues and reproductive organs, is, like the hydra and the star-fish, dependent on no central brain, and therefore any of its growing cells can reproduce the whole. Germ plasm and somatic plasm are equally diffused. Truly embryonic tissue can be formed from almost any part of the plant.

Returning to animals, if we examine more closely the regenerative process in the types mentioned, we shall find a close resemblance to that of reproduction.

In the hinder end of a worm whose tail has been cut off, we see the process of embryonic development repeated—first, a two-layered "gastrula" with ectoderm and endoderm, with mesoderm formed later, and then the various organs formed from these layers, as in the egg; and in amphibia and reptilia a similar re-formation of the embryonic tissues precedes the moulding of a limb or tail.

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[1] In the frog, Roux found that if, in the two-celled stage of cleavage, one cell be destroyed by a hot needle, the other develops into half an embryo; but this half embryo may at quite a late stage regenerate the missing half. And Morgan has shown that if the uninjured cell (of the two-celled egg) be inverted so as to mix up the yolk and the protoplasm equally, it will form a complete but dwarf larva.

Hence, we can at least partly understand how it comes that a newt's leg is re-formed with equal accuracy whether the section of amputation be transverse, oblique, or irregular. The cells of the stump end merely revert to an embryonic condition, and repeat an ancestral process.

Reproduction has been spoken of by Spencer and Haeckel as discontinuous growth. The aim of growth is differentiation of function; when that is attained, growth ceases to be continuous; augmentations of matter lead to separation of parts, and we have "reproduction." When a part is artificially separated, we have "regeneration," which we may similarly term "continuous reproduction." A further proof of the likeness of the two processes is found in the fact that regenerate organs, such as the limbs of prawns, the tail of the lizard, often tend to revert to more primitive types.

One of the most marvellous features of regeneration is the *order* with which it is effected, the apparent *regulation* of the growth in both extent and direction to exactly replace the missing part; what it is that says to the growing cells, "Thus far and no farther." We have not fully answered the question, but have at least advanced a step in showing it to be one with that of embryonic development.

Regeneration, as met with in adult mammals and in man, offers fewer points of general biological interest, though more of practical importance. Certain parts of our bodies are always undergoing what may be called "physiological regeneration." Such are the epithelia of all parts communicating with the outer world, whether in skin, respiratory, alimentary, urinary, or generative systems; the lymph glands, spleen, and bone marrow, the cells of ovaries, and at times of the mammary gland. In all these nuclear figures (mitoses) are regularly found. In others the process stops at, or soon after, birth. Such are the secreting glands—liver, pancreas, kidney—bone, cartilage, the connective tissues and smooth muscle fibre. In others again—striated muscle and nerve—mitotic figures cease before birth.

In most of these, however, regenerative changes may follow injury, or increased growth succeed unusual stimulation.

With regard to the first group, I need only remind you of the speed with which an area of skin, denuded of its epidermis, is covered over by the growth of the neighbouring epidermal cells. In some way or other the removal of a group of these cells intensifies the process of proliferation, which is a life-long habit with them. The now common surgical procedure of skin-grafting has, moreover, familiarised us with the remarkable fact that epidermal tissue removed from its natural bed may acquire new vascular connections with subjacent granulation tissue, and form an islet of new skin that comes to resemble that surrounding it. Thus the skin of a European transplanted on to a negro becomes dark, while that of a negro transplanted to the white loses its pigment.

Passing to those organs where cell proliferation does not normally continue active after birth, we find that they yet have great regenerative or re-creative capacity. The researches of Ponfick and others have shown that the liver possesses this power to a remarkable extent. Thus in dogs and cats more than three-fourths of the liver may be excised without other disturbance to the animal than a temporary diminution in the excretion of urea. Within a week, however, regenerative changes are observed in the portion of liver remaining, and in five weeks' time it has regained its former size, while the urea excretion also becomes normal. Microscopical examination of the stump of liver shows numerous mitotic figures in the gland cells, and eventually rows of new liver cells are intercalated between the old ones. New blood vessels and bile ducts also appear, but new lobules are not formed. In the regenerated liver the lobules are much larger and less regularly constructed than normally. There is thus some hypertrophy of elements, but the main event is the great hyperplasia of gland cells. Similar re-creative changes have been observed in the liver of man after extensive damage by hydatids. These observations show us what enormous reserve power the hepatic gland possesses; how a fraction of it is able, for a time at least, to perform the functions necessary to life, and how this fraction may start again into growth and reproduce the whole. There is both a functional and a formative reserve of energy.

We can thus understand those clinical cases not a few, in which the patients, contrary to expectation, go on living in fair comfort for years, although the liver is apparently entirely disorganised.

There recently died a patient of mine whose liver had been very large, hard, and nodular, to my knowledge, for five years, who in that time had been tapped of ascitic fluid 110 times (15 to 20 pints at a time), and who, except for the ascites, went on pretty comfortably, and with normal nutrition, until the last few months of his life.

In paired organs, such as the kidney, we do not meet with such marked re-creative power in the single organ, but the functional loss is made up by increased activity of the other.

Thus, one kidney may be excised or destroyed by disease without serious results to the animal, and sooner or later the renal secretion becomes normal, and the opposite kidney is found to be hypertrophied. Here again the gland cells of the convoluted tubules multiply and show karyokinesis, but there is also an enlargement of the cells and tubules, and especially of the glomeruli. Hyperplasia and hypertrophy go hand in hand.

The extent of the functional reserve power of the kidney is well shown by the remarkable experiments of Bradford, in which, after three-fourths of the total kidney weight of dogs were excised, the animals excreted more urine and more urea during the two to six weeks which they lived, and this without any regeneration of the remaining fragment.

Tuffier, however, excising first one kidney and then part of the other, found the fragment hypertrophy.

If we ask what is the *cause* of the regenerative or hyperplastic process in these viscera—liver and kidney—we find ourselves, as in the case of the worm, the newt, and the lizard, unable to completely answer the question, though we can go a little farther. We have, as before, the tendency of living cells, under certain conditions, to reproduce the process of embryonic development, but we also have something more—an increase of the normal stimuli to functional activity. We could point to no special stimulus exciting the leg of the newt or the tail of the lizard to regeneration; but in the liver and kidney we have the blood laden with various products waiting to be metabolised or excreted by the cells of these glands, and probably urging them to increased activity.

In the case of the liver hardly any other explanation offers itself, and in the kidney, though an increased blood supply may be a factor, this explanation is probably true. It has been shown (Bizzozero and Sacerdotti) that when, after unilateral nephrotomy, the flow of urine is checked by starvation, the hypertrophy does not occur in the other kidney (although inanition is no bar to regenerative activity in the liver), and that when in a normal dog the work of the kidneys is increased by the repeated transfusion of defibrinated blood, from dogs from whom both kidneys had been removed, signs of active cell proliferation are seen in the convoluted tubules. The hypertrophy of the kidneys in diabetes is further proof of the efficacy of increased work as a cause of growth and enlargement.

I need not do more than refer to the enlargement of the *muscles* of the athlete, to the hypertrophy of the heart in certain cases of valvular defect or of increased arterial tension, and probably also in cases of prolonged athletic strain, and to the enlargement of one lung, where the other is shrivelled from pleurisy or other disease, as instances where increased functional activity is responded to by increase in structure. In all these organs, whether by increase in the size of the cells or by increase in their number, or by both, nature indicates the possession of a large reserve of power. A healthy man does not live up to his income.

Muscular substance, then, clearly has latent generative power, but how far it is capable of regeneration after injury has long been a point in debate. With regard to striated muscle, it seems certain that only very slight traumatisms or breaks of continuity can be restored by growth of muscle substance; all others are filled with fibrous tissue.

It is highly probable, however, that some degenerative changes, such as are induced by frost or typhoid fever, are followed by functional regeneration. Experiment and observation have shown increase in the number of nuclei 'and their surrounding protoplasm, after the death of the contractile substance, which afterwards appears to be regenerated. Embryonic development is again copied.

Clinical experience also leaves little doubt that cardiac muscle may recover from very considerable degeneration; witness the numerous cases of influenza, rheumatic and typhoid fever, where the heart muscle remains very weak for months or years, but eventually regains its strength.

The *nervous system* is perhaps the most differentiated of all the tissues of the higher animals, and in it, in the adult, both growth and regeneration after cell destruction appear not to occur. We have no evidence that ganglion cells are ever reproduced, or that they increase in number with increased functional activity.

That the brain and spinal cord have a functional reserve of power, and that they are capable of being trained to greater efforts, is certain; but we do not as yet know of any concomitant cell multiplication. Structural differentiation may be produced within the cell, or occur as subdivision of branches, but if so, these changes are inaccessible to our methods of observation.

Regeneration of peripheral nerves is, on the other hand, a well-known fact, though its method has long been, and still is, a matter of dispute. The balance of evidence, however, I believe, still strongly supports the old view, that peripheral nerve fibres, motor and sensory, are to be looked upon as processes of cells in the spinal cord or root ganglia, die when separated from the cells, and are regenerated solely by outgrowth from the central end still attached to the cell.

The observations of surgeons, showing the great rapidity with which sensation is regained after the divided ends of a cut nerve have been sutured, have been a difficulty to this view, and have suggested that the peripheral part, as well as the central, must have remained alive and capable of reunion. These observations, however, have not infrequently lacked the accuracy of a scientific observation, and may probably be explained by the overlapping of the areas of sensory nerves.

Within the brain and spinal cord regeneration of nerve fibres appears extremely slight, though after section of a posterior nerve-root, new fibres have been found to grow into the cord from the spinal ganglion (Ströbe). Recovery of function is, however, by no means so limited.

Thus in animals the paralysis and anæsthesia caused by hemisection of the cord is usually after a time recovered from, and recovery may again ensue after a second hemisection at a different level on the other side; though in neither case do regenerated nerves bridge the gap. In man it is common for hemiplegia or aphasia due to damage to a cerebral hemisphere to be at least partially recovered from; in disseminated sclerosis, early and temporary paralyses occur, though the sclerotic patches, so far as we know, remain unhealed; and even in locomotor ataxy the symptoms of disease may pass away, though sclerosis of the posterior columns remain. Obviously nervous currents escape along other than the accustomed channels, and (although we cannot, as with arteries, speak of an anastomotic circulation) the resistance between contiguous nerve-twigs may be overcome and impulses leap across boundaries that before enclosed them. Nerve-cells also may develop faculties previously latent, as when Broca's convolution in the right hemisphere takes on the work of its damaged fellow in the left.

It may here be mentioned that nervous influence does not appear necessary to the regenerative process in other organs. Thus the leg muscles may regenerate after the sciatic nerve is cut, and section of the cervical sympathetic rather helps than hinders repair of injury in the rabbit's ear, owing to the vascular dilatation produced thereby.

Let us next pass to the great group of mesoblastic tissues which connect, support, and nourish the others. We shall have to consider them again in dealing with the phenomena of inflammation, but certain points may be dealt with here.

And, first, those which connect—*connective tissue.* It is this with which we habitually associate the idea of repair, for whatever happens to epithelium, nerve or muscle, connective tissue can always fill the gap. Here again we find still a dispute as to the origin of the new cells which fill the gap in an aseptic wound; some stoutly holding that leucocytes wandering out from the blood may settle, put out processes, and form fibrous or reticular tissue; while others assert that, though wandering cells may be found in an aseptic wound, yet they take no part in the process of repair, which is entirely due to proliferation of the fixed cells of the part.

Time will not allow us to enter upon this discussion. I can only say that the balance of evidence favours the latter view, in mammals at least, though Metchnikoff's observations point to the power of the leucocytes of the tadpole forming fixed branched cells. The origin of leucocytes themselves is as uncertain as their end and as the relation of the different forms to each other.

In regenerating lymph glands, Ribbert thinks he finds lymphocytes growing from endothelial cells, while Flemming and Hansemann assert their production solely from pre-existing lymphocytes.

Wandering cells must, however, some time have sprung from fixed ones, and I cannot but think it probable that the process of development may be repeated in after life. The rate at which they are (under certain conditions) formed and used up is difficult to account for on the hypothesis that they are solely formed by karyokinesis from each other.

Secondly, the *supporting tissues*, of which the chief is bone. The regenerative power of bone has long been known, and has been associated chiefly with the periosteum, changes in which are the most conspicuous feature in the healing of fractures. Here again the process of embryonic development is closely copied—first embryonic tissue, then cartilage, then bone. Lately, however, Macewen and others have shown that the periosteum is not essential to the regenerative process, but that bits of the cancellous tissue or of the bone marrow, when transplanted, grow into bone.

Thirdly, the *nutrient tissue*—blood and lymph—part fluid, part solid, ever bringing food and oxygen to all the body elements, ever receiving and removing their refuse, aiding them in their struggles, sharing their injuries, giving them dissolution in death.

No wonder that blood is constantly wearing out and as constantly being renewed. Regeneration is here a physiological process. In the lymph glands and in the red bone marrow, leucocytes and red corpuscles respectively are constantly being multiplied (and possibly in other organs also) at such a rate that it has been calculated (Hayem) that the loss of a pound of blood may be made good in a fortnight.

In cases of prolonged or repeated hæmorrhages, where the manufacturing power is incomplete, new workshops are opened, and the yellow marrow of the long bones becomes (as also in pernicious anæmia) red fom the multitude of red corpuscle-making cells within it.

The white corpuscles, which, as we shall later find, are sacrificed in thousands in the battle with invading parasites, are in time of war rapidly produced in thousands, probably mainly in the lymph glands, though the exact method is still in dispute.

The blood fluid or plasma—a secretion whose composition obviously depends upon changes in all the cells of the body, but whose main features are probably due to the metabolic activity of capillary walls—may also be renewed after serious drain, as where a man, collapsed and shrivelled from the albuminous evacuations of cholera, in a few days regains his natural juices.

We shall have again to consider blood, both plasma and corpuscles, in dealing with inflammation and the combat with microbes. Here I will only further allude to the importance of the coagulating property of blood, both in arresting hæmorrhage and in glueing together cut surfaces as a preliminary to their repair.

(*To be continued.*)

NOTE ON THE USE AND ABUSE OF CHLORATE OF POTASH IN THE TREATMENT OF DISEASE.

By Henry Ashby, M.D., F.R.C.P., *Physician, Manchester Children's Hospital.*

Probably chlorate of potash is prescribed less frequently at the present time than it was twenty years ago, and it is used also in smaller doses than of old. In the "British Pharmacopœia" of 1898, the dose given is 5 to 15 grs., instead of 10 to 20, thus reducing it by one-half. Doubtless a knowledge of the poisonous action of the salt in large doses has suggested this change.

The dangers of the salt are not recognised by the public at large, as considerable quantities are consumed in the shape of lozenges, tabloids, "pellets," and so-called "throat cures"; nor by druggists, for I was surprised to learn from a well-known pharmacist a short time ago, that he did not think that chlorate of potash was any more poisonous than bicarbonate of soda!

In past times, 20- to 30-gr. doses were commonly given every four hours to women suffering from puerperal septicæmia, under the impression that the salt, when taken internally, yielded up its oxygen to the hæmoglobin of the blood. Doubtless the same fallacy was running in the head of the physician who directed his consumptive patients to drink *ad lib.* of a saturated solution of this salt!

Nowadays the salt is most often prescribed for inflammatory conditions of the mucous membrane of the mouth, as well as for acute tonsillitis, scarlet fever, and diphtheria. As a cure for diphtheria, it is, I fancy, falling into disrepute; its benefit is very doubtful, and certainly, in the later stages of a severe case, it is likely to do harm rather than good. It is largely used, also, in the treatment of scarlet fever, and here also its curative virtues are not great. I must have prescribed it many hundreds of times in the scarlet fever ward at the Manchester Children's Hospital,—I cannot say with any striking results,—and in later years I gave it up entirely. In tonsillitis, its value, to say the least of it, is doubtful.

Its value in the treatment of "ulcerative stomatitis" has long been recognised; and on account of its striking curative effects in this disease it has secured for itself a certain amount of credit in the treatment of other forms of stomatitis, which it hardly deserves. Given internally in suitable doses, its effect on "ulcerative stomatitis" is as striking as is fresh orange juice on the hæmorrhagic stomatitis of infantile scurvy, improvement commencing within twenty to thirty hours of the commencement of the treatment.

Concerning the pathology of ulcerative stomatitis, and how far we can admit its claims to be considered a "self-standing disease," there is not much to be said. We know that it does not occur in infants prior to the eruption of the teeth; that children who are attacked are mostly in a low state of health; and that this disease appears to be infectious, or, at least, occurs in small epidemics. Various searchers have from time to time proclaimed the discovery of a specific organism in the discharges from the gums, the most recent being Bernheim, who, in thirty cases, found a motile bacillus and a spirochæte. It is possible that more than one disease may be included under the name of "ulcerative stomatitis."

The attack is ushered in with fever; there is tenderness of the gums and teeth, with excessive salivation; the lesions are confined to the gums and cheek, and possibly the tongue. The gums are swollen, often very markedly so; they readily bleed; a fœtid purulent discharge issues from their edges where they come in contact with the teeth. There is usually, also, a sharply cut ulcer, perhaps half an inch to an inch in diameter, with a yellowish base, opposite the lower molar of one side; the ulceration may involve the fissure between the gums and the cheek, and also the tongue on the corresponding side. In those cases where there is this form of stomatitis present in young children who have only cut their incisor teeth, the only part of the gums affected is that around the teeth, and there may be a yellowish infiltration of the surface of the mucous membrane of the lip which comes in contact. The position of the deep ulcer found opposite the lower molars is no doubt determined by the action of the buccinator muscle in chewing. This muscle presses the bolus of food between the molars, and the friction of the molars against the mucous membrane gives rise to the ulcer. This friction is the exciting cause. In a healthy condition of the mucous membrane it does not occur. The disease occurs equally in children with healthy as with carious teeth.

In such cases, chlorate of potash given in 3- to 5-gr. doses (child, 3 to 7 years) every four or six hours, so that the daily dose is 20 to 30 grs., acts like a charm, and in a day or two a marked improvement is manifest. The ulcer becomes cleaner, the gums less swollen, the fœtor of the breath disappears, there is less tendency to bleed, and in a week or so the child is practically well. The salt gives rise to smarting when taken, especially when not freely diluted, but the smarting soon passes off. The salt appears to be secreted in the saliva very soon after being taken. It acts better given internally than applied locally. How it acts it is difficult to say.

With regard to the other forms of stomatitis common in children—such as thrush, catarrhal and aphthous—it is safe to say that they are not nearly so readily acted on by chlorate of potash. I have often prescribed this salt in out-patient practice on the same day to a child suffering from a typical ulcerative stomatitis, and another with a number of shallow small ulcers, situated on the tongue and mucous membrane of the cheek, and noted the effect in both cases in the course of a few days or week. In the former case there could be no doubt whatever as to the effect; while in the latter the improvement was usually far less marked. It is doubtful if any other form of stomatitis is benefited by the exhibition of this salt.

It is not perhaps easy to say what is a safe dose of chlorate of potash to give to children, or how long the salt should be taken without any fear of its producing a deleterious effect. It must be borne in mind that some children are more sensitive than others. Children under two years of age are especially sensitive, and I believe the salt is very apt to produce anæmia and depression in such.

The following case is a very instructive one, and illustrates the dangers that are only too likely to attend domestic medication:—

I was asked to see a child of 14 months who was reported to be dangerously ill, and was suffering from an enlargement of the liver. I had seen the girl some months previously when suffering from indigestion and some tenderness of the bones, due to infantile scurvy. She was then anæmic, but I was told she had much improved in the interval. In the present instance it was stated that she had been fretful for a fortnight on account of teething (first molars), but for three or four days prior to my visit she had appeared seriously ill. The medical attendant had only been called in a day or two before, and had discovered a much enlarged liver. The temperature had been below normal. When we saw the child together she was in a somewhat lethargic state, and cried pettishly if disturbed, but did not seem to be in any pain. Her lips were dry and cracked, the gums around the newly cut molars were spongy, the saliva was a brownish colour. She was intensely anæmic, but not jaundiced. Both liver and spleen were much enlarged; the former appeared to bulge outwards and distend the abdominal wall, so that on lifting the bedclothes the bulging could be seen at a glance. Its lower edge could be felt as low as the umbilicus. The edge of the spleen extended quite as low as this. There were some rhonchi to be heard in the lungs; the heart's action was very feeble; the stools were white in colour; the urine was reported to be of a brownish colour.

The case at first sight was exceedingly puzzling. The condition indicated some grave blood disease—but what? The diagnosis was cleared up by finding that, on the advice of some meddlesome friends, her relatives had, unknown to the medical attendant, given her, on account of "teething," for the past three weeks, "*as much chlorate of potash as would lie on a threepenny-piece, three times a day.*" This would roughly amount to 5-gr. doses three times a day. How long this amount took to produce serious symptoms cannot be said, except that it was clear that before the end of three weeks she was seriously ill from the effects. I saw her two days later, the chlorate having been stopped in the meantime. Her condition was improved; the liver and spleen were smaller, and she was a better colour. But the following day I learnt that symptoms of cardiac failure had supervened, and she died a few hours after. Unfortunately, no post-mortem was possible.

This case is, unfortunately, nothing like as complete as could be wished, but there cannot be the least doubt that it was a case of subacute chlorate of potash poisoning. It certainly suggests a caution in the use of this salt.

One of the early symptoms which have been described in chlorate poisoning is drowsiness. There would certainly be an increasing anæmia, with perhaps jaundice. A brown coloration of the urine would, of course, be exceedingly significant. Enlargement of the liver and spleen would probably follow, and very likely a hypostatic congestion of the lungs. Oozing of brownish-coloured blood from the gums would possibly take place.

Apart from chlorate of potash producing such serious symptoms, there is reason to believe that it may produce a chronic anæmia, with enlarged spleen; and in all such cases which present themselves, it is worth while to try and ascertain if this has been the case.

ON THE RELATION OF THE NERVOUS SYSTEM TO DISEASE AND DISORDER OF THE VISCERA: DISORDERS OF VISCERAL SENSIBILITY.[1]

By Alexander Morison, M.D., F.R.C.P.Ed., *Physician Out-Patients, Great Northern Central Hospital and Children's Hospital, Paddington Green, London.*

(*Continued from Volume IV., page* 513.)

It may be remembered that, while discussing some points in the physiology of the visceral nervous system last year, I drew a parallel between viscero-motor actions in general and cardio-motor action in particular, arguing that, while the products of such motions differed with the organs which were the seat of motion, the mechanism of such action, whether nabolic or katabolic, was essentially the same. It is possible in Protean nature to push a parallel too far, and I have no desire to become too general in discussion. But it appears to me that it will be convenient, in considering the relation of the nervous system to disease and disorder in the viscera, to divide these into two large categories, namely, into disorders of sensibility or sensation, and into disorders of motility or motion.

The brain could no doubt be thus considered collectively with the other viscera, but there is a certain difference between the former and the latter which, together with the special character of this lectureship, renders it desirable that that viscus should receive separate treatment. The distinction consists in the fact that, whereas the control of the viscera generally by the brain is through the agency of nerves, structures continuous with it, and inseparable from it, the influence exerted upon the brain by the rest of the body is not only by way of the sensory channels which come to it, but also by means of the blood which in great measure goes from it. There is indeed throughout the body, as there is in every organ, a functional unity, which might be more accurately termed a tri-unity, due to the consentaneous operation for the production of a common result of the peripheral organic cell, whether in brain or muscle, a central regulating cell and its nerve channels, and the bond of union between the two, namely, the blood with its stimulating and life-giving properties.

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[1] The Morison Lectures, delivered before the Royal College of Physicians, Edinburgh.

Recent physiological thought appears rather to have dissociated this essential unity, and much ingenuity has been exerted to show how very well the organs can get on without the nervous system. The question of non-neural cardiac rhythmicality we touched upon last year, and traced the controversy from the now untenable assertion of an absence of nerves in general in the cardiac apex, to that of an absence of ganglion cells in particular in that portion of the organ.

Personally, I endorse Henry J. Berkley's physiological standpoint, although I cannot agree with his description of the cardiac ganglion. His words are—"Cajal, in portraying the Auerbach plexus by the vital blue method, gives a picture that is not entirely dissimilar to our cardiac nerve-plexus with the neural enlargements and nerve-cells. Though in the cardiac plexus the nerve-cells are vastly inferior in number, different in appearance, and do not occur in clusters, yet the general likeness is sufficient to allow of some comparison being drawn between the two."[1] This is

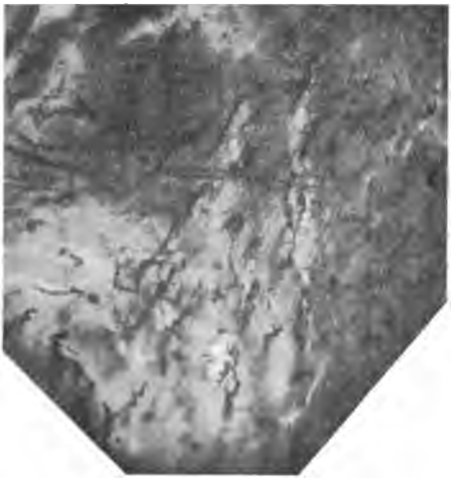

FIG. 1.—Golgi preparation. Ganglion in the apex of the heart of a cat.

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[1] *Johns Hopkins Hosp. Rep.*, Baltimore, 1894, p. 89.

fairly correct as regards the lower third of the heart which Berkley examined, and I shall show some of these ganglia of the apex which agree in great measure with this description. But in applying his words to cardiac ganglia generally, Berkley's description is not correct. As I demonstrated last year, the cardiac ganglia of the vagal system, as I take them to be, may consist of very large clusters of cells, such as the fine specimen which was figured in this Journal last year. My reason for regarding such ganglia as pertaining to the vagal system is, that they bear much the same relation to the peripheral distribution of the nerve that the plexiform ganglion does to it at its origin, and they are to be met with in many of the organs to which the vagus is distributed. The important point, however, is that, character apart, the apex of the heart, although not profusely, is nevertheless sufficiently ganglionated, and the Hallerist must look for some other support than his anatomical argument. Physiology affords more scope to the imagination. But we saw that the presence or absence of ganglionic cells was, after all, a matter of small moment to the Hallerist. They were nutrient or something else. In any case they had little to do with motion in any organ. You cut a vagus and the heart does not feel it long. You detach an organ and stimulate it, and the functional result is there. It has been shown, however, that if you cut both vagi the staying power of the heart is seriously compromised. Balint[1] has shown that if aortic regurgitation be artificially established, and one vagus cut, the action of the heart is not materially interfered with; the compensated organ continues to act efficiently. But that if both vagi be divided, the heart, even although it has undergone compensatory hypertrophy, soon gives out, and the animal dies in about a week with well-marked evidences of retrograde stases. I have already mentioned a case in which Billroth accidentally excised a portion of one vagus during an operation in the neck, with only temporary disturbance of the heart's action; and my colleague, Mr. Watson Cheyne, has recently mentioned to me another case not yet published, and which he has kindly permitted me to refer to on this occasion. During an operation for malignant disease involving the larynx, he found it necessary to excise a considerable portion of one vagus. The immediate consequence of this appeared to be an arrhythmical action of the heart which subsided in a few days; but the stability of the organ has apparently been more permanently impaired, because slight emotional excitement reinduces the condition, which was not present prior to the operation. Survival after the division of one nerve is probably the result of what may be termed incomplete peripheral decussation in the organic plexus, a subject, as it appears to me, of much practical interest, and one which I have examined somewhat, but my results are still too rudimentary for publication. The nerve from one side probably sends fibres to both sides in such unified organs as the heart, while division of both nerves naturally destroys such a nerve supply and saps the fountains of persistent rhythmicality.

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[1] *Deutsche med. Wchnschr.*, Leipzig, 1898, Nos. 1 and 2.

Inasmuch, however, as anatomy lies at the foundation of our knowledge of all forms of organic activity, it would have been a difficult matter for the adherents of a neural theory of sustained motion to have defended their position, had it been possible to point indubitably to mobile structures which continued efficiently to act for a lifetime without such endowment. The conception of such a condition would *a priori* seem unreasonable, but as the unexpected does occasionally happen, it was just possible that such a fact might conveniently come to the aid of the distressed Hallerist. This was supposed to be so in the case of the intracranial vessels. Some of the peculiarities of the intracranial circulation certainly appear, at first sight, to lend probability to the occurrence of an exceptional condition in this sphere. The resistant skull-cap, Alexander Monro the second's suggestion of the full bottle held upside down without spilling its contents, Kellie's well-known experiments, and much argument founded thereon, suggested that vasomotor nerves had little to do with the intracranial circulation. Some, like Hill and Bayliss,[1] argued that nerves were absent because they had not been found, and for the purposes of their theory it was necessary to assume such absence.

In my ignorance I had assumed that these vessels were innervated, until Sir John Tuke informed me last year that they were supposed not to be. Were this so, I felt that I was standing upon somewhat thin ice in much that I had to say, and found it advisable to proceed somewhat a-tiptoe until I felt more secure, although I suppose walking a-tiptoe does not materially lessen your weight, but it may avoid the clumsy kick which would bring about a sudden and ridiculous immersion. I therefore determined to investigate the point at an early stage of animal life, when the tissues appear to be, as a rule, most amenable to staining methods. As I have related in this Journal for November of last year, I selected the fully-grown fœtus of a cat for the purpose, and was fortunate enough to find not only that the pial vessels were fully innervated by an ample plexus, but also that branches of nerve might be found in close relation to blood vessels which bore fully developed and permanent ganglion cells.

As I owed it to Sir John Tuke that I had undertaken the research at all, I considered it my duty to inform him first of the result, which I did while he was at Aix-les-Bains last June. I have since discovered that Obersteiner found some such nerves on an old-gold preparation in 1897, and understand that Dr. Gulland also found the intracranial vessels to be innervated in July of this year.

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[1] *Journ. Physiol.*, Cambridge and London, 1895.

Thus, then, another non-neural persistent rhythmicality has found its way to the limbo of many an ingenious hypothesis. The preparations under the microscope show these structures, the relation of which to sleep and cerebral activity must be a very important one.

Assuming, therefore, that the nervous system bears an important relation to splanchnic as well as somatic motion in health, I assume also that that relation is quite as important in the case of disordered visceral sensibility and motion.

Having argued that motion, whether sensory or mobile,' to use a convenient although tautological expression, is one in kind in all organs, although modified by the fuctional nature of the organ manifesting it, I shall take as the type of disordered sensibility and motion abnormal conditions of the heart, for fuller discussion, as those with which circumstances have rendered me most familiar, and shall apply the lessons they seem to point as rationally as I can to disordered sensibility and motion in other organs.

On the threshold of such a discussion it seems necessary to inquire what the local elements in visceral or referred pain are. It occurred to me that the uterus might be taken as the best physiological type of visceral pain, and that an examination of the finer innervation of that organ might throw some light upon the general question. This I have accordingly done; and should I in the course of my anatomical remarks relate facts known to others and discovered by others without due acknowledgment, I must plead ignorance of such previous work, and can therefore disavow any intentional plagiarism.

A lateral branch from a large nerve in the uterus is given off, so that the portions chiefly constituting it appear to run in opposite directions in the outer strands of the parent nerve, and within its adventitious sheath. This will be evident from an examination of the specimen. Fibres may also cross over the trunk from the opposite side to join the branch. As the branch increases its distance from the main stem, its division for distribution takes place, as one would expect, by gradual subdivision, which naturally diminishes the size of the parent branch. On examining the mode of distribution of a small branch or twig, and noting its relation to blood vessels, we find that some of the smaller subdivisions tend to follow the course of the vessel, and others cross it for final distribution to individual muscle fibres. The branches following the vessels form a plexus around them, a sort of plexal tube or sheath, through which the vessel runs, and by twigs from which it is innervated.

Before distribution to the organic muscle, triradiate nuclear points of dispersion may be observed, as in other structures, such as the pial membrane and the pelvis of the kidney; and nuclei strengthening fine fibrils may be observed in the uterus, as elsewhere. But I have also noticed a peculiar relation of some neural swellings or nuclear points on the uterine fibrils which I have not seen elsewhere, but which may nevertheless quite possibly occur. A rounded nucleus may be placed quite laterally to the fibril, so as to constitute a rounded knot or excrescence upon it, and which is so rounded and displaced laterally as to convey at first sight the idea of a small ganglionic cell, but which, on examination by high power ($\frac{1}{12}$ immersion), reveals nothing more than an irregularly stained nuclear body, somewhat resembling a small Pacinian corpuscle, but without the characteristic striation of the latter.

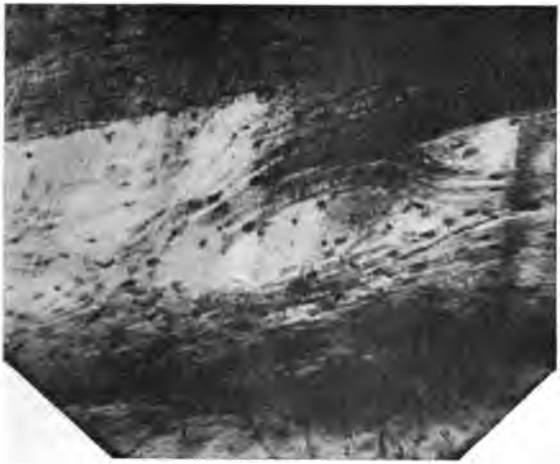

FIG. 2.—Showing rounded nucleus on nerve-fibril in pregnant uterus of cat. The fibril is continued on the other side of the nucleus, but is out of focus.

These peculiarities may be seen under the microscope and on the screen. As we have already seen, the trunks and branches of the uterine nerves have a more or less tortuous course, a tortuosity which, in the case of the larger nerves, I believe to be in inverse proportion to the stage of pregnancy or size of the womb.

The local causes of pain in the parturient organ, which may for convenience be termed visceral pain, are uterine and extra-uterine. By extra-uterine pain I mean such as is caused by pressure upon surrounding textures, including those innervated by purely somatic nerves as the uterine contents are extruded. With such pain I am not at present concerned, although it perhaps constitutes a great, if not the greater, portion of the pain incidental to labour, taken as a whole. The total pain is, however, undoubtedly commensurate with the degree of uterine contraction, and some of it appears to be localisable in the uterus itself, for pain is present before extra-uterine pressure is a considerable factor in the situation, and also after labour is over, when coagulated blood or other residuum has to be extruded. Such essentially uterine or visceral pain must, it appears to me, be caused by intramural pressure upon sensory nerve structures distributed to the muscle, and although I have not found any indubitable Pacinian corpuscles, the lateral rounded nuclei to which I have referred, if they be sensory in character, may be among those structures the intramural squeezing of which, during contraction of a certain degree of severity, is a local cause of pain, and the point to which the sensorium refers the centrally experienced sensation, and of necessity the site of peripheral sensory stimulation. Spasm, for example, in the calf of the leg may give rise to severe pain, and the pain is proportionate to the severity of the tetanic contraction. We know from the researches of Sherrington and Batten, to which I referred last year, that there are good grounds for regarding the muscle spindles as sensory organs, and doubtless, if they be so, they must likewise be the chief seat of referred pain attending somatic muscular cramp. On the same principle, the tetanic squeezing of visceral muscle (and we know from experimental physiology that visceral muscle is apt to develop a cumulative contraction out of proportion to the stimulus applied) may be the local stimulus inducing the pain which is referred by the sensorium to its place of origin.

The absence, however, of visceral pain under circumstances in which we might *a priori* expect it to be present, is a striking clinical fact. As we saw last year, the visceral nerves, in the absence of inflammatory states, are so little sensitive, that organs may be freely manipulated, and even injured, without the evident production of painful sensation. Nodules of malignant disease may also stud a viscus, and cause little pain. Cysts may rupture into the pericardium and elsewhere, causing death, of the presence of which there was no sensory or other evidence during life. On the other hand, it is commonly observed that in certain situations, and without inflammatory lesions, visceral pain may be as well marked in the splanchnic as in the somatic nervous system. Large calculi may lie in a gall-bladder for a lifetime without causing pain, but even a small one under the peristaltic grip of the bile duct may cause an agony which, from its very intensity, may prove fatal. The same is true of renal calculus. The stone, once engaged in the ureter, may jeopardise life, and even cause death from collapse due to the severity of the pain, while very little and it may be no pain may attend its presence in the pelvis of the kidney, which we have seen is richly endowed with nerves having the anatomical character of peripheral sensory nerves. Slight causes may, moreover, provoke great visceral pain, such as the colic, which is due to very moderately irritating material in the alimentary canal.

It would appear, therefore, that visceral pain, in the absence of inflammatory conditions, may be of direct and indirect origin, and that it has some relation to the degree of pressure exercised upon sensory nerve-endings, either directly by such action as the passage of a calculus through a narrow peristaltic tube, or the indirect spasm induced, it may be, by slight causes in the visceral muscle. It is therefore the implication in disease of orifices having contractile cavities behind them, which are most calculated to make pain a prominent feature in clinical histories. Thus the strictly local ravages of disease in a viscus may be painless, but the provocation by it of tetanic muscular action or cumulative peripheral pressure may cause much suffering.

Finally, visceral pain may be the peripheral expression of a central lesion. That pain should be referred to a locality where the original stimulus is situated, as it must of necessity be somewhere, does not seem in any way remarkable. It is but a form of touch, a painful touch, and the whole utility of touch, whether painful or otherwise, is to indicate locality and the character of the circumstances surrounding a sentient being. Pain may, indeed, in some measure be regarded as a beneficial provision for the preservation of the individual. But the experience of pain not at the site of its cause is somewhat remarkable. The surgeon is familiar with the pain of vesical calculus referred to the end of the penis, and with the knee pain of hip-joint disease. In like manner do the visceral crises of tabes appear to be caused by a peripheral projection of sensory irritation from structures in process of sclerosis, to which I shall refer again, as I also shall to the somatic projection of pains having their site of stimulation in the viscera, and to the stimulation of one viscus by another.

With this introduction I pass to the consideration of cardiac pain as a type of disordered visceral sensibility.

The natural provision made for safeguarding from undue mechanical stimulation the nerves entering and permeating an organ, is nowhere better seen than in the heart. If the heart of an animal be taken which has died in systole, and be fixed in that tetanic state forthwith by being placed in alcohol, microscopic sections show well the means adopted by nature to protect both vessels and nerves from undue squeezing by the close-grained organ. The specimen I describe is from the apex of a sheep's heart prepared in the manner stated. The largest nerves may be seen to be situated nearer the vein than the artery, as they enter the organ at its surface, while the neuro-vascular atrium is also well padded with fat. The whole constitutes a perfect contrivance for obviating undue pressure from the surrounding muscle, which may be observed to be in a state of systolic contraction. The same perfect adaptation to circumstances which we see at the surface obtains as we follow the nerve trunks into the organ, the vessels and nerves coursing largely together, and being protected in more open situations by associated fat cells. The heart being, like the uterus, an organ having a wide range of contractile variability, if the suggestion I have made in explanation of the tortuosity of the uterine nerves held good, we should expect a somewhat similar provision to exist in the heart also, and we find that it is so. Coils, such as are met with in the uterus, are not found, for sufficient reasons which need not be discussed further than by pointing out that the return of the organ to smaller size is but momentary in the case of the heart, and the difference between the maximum and minimum less than in the case of the uterus. Berkley[1] refers to the "wavy" nature of the nerves passing through the cardiac muscle. My preparations, like his, were made from the lower third of the ventricle, and include, of course, the absolute apex. The rich innervation of the part is well seen in the specimens, which I stained by the quick Golgi-Cajal method. The specimens are from the heart of a cat in advanced pregnancy, which was killed by being anæsthetised and then drowned. The heart was thus in an uncontracted condition when first placed in the reagent, and this may account in a measure for the satisfactory results obtained. This, however, is not quite certain, for the capriciousness of the Golgi method, which yields such superb results when successful, is

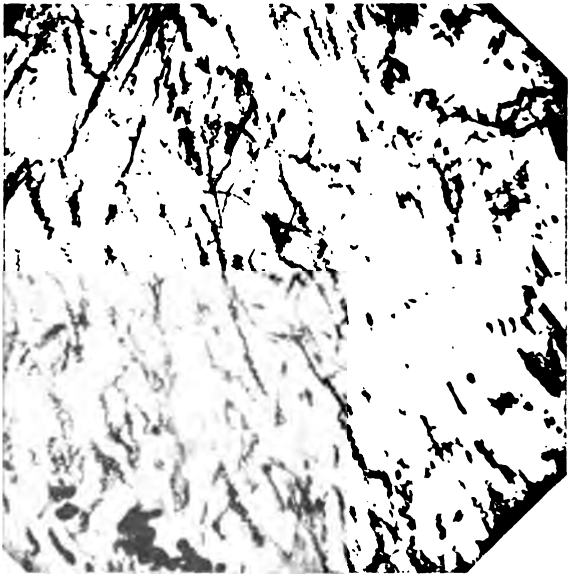

FIG. 3.—Golgi preparation. Tortuous nerves in the lower third of the heart of a cat.

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[1] *Johns Hopkins Hosp. Rep.*, Baltimore, 1898.

not yet altogether explicable. It will be observed that waviness is a very mild term to express the almost corkscrew tortuosity of some of the nerve trunks depicted. One can readily conceive the perfect adaptation of such a contrivance for suiting the altering state of the organ in systole and diastole. In the systolic organ, as in the non-gravid uterus, the threads of the screw, so to speak, are approximated, while in the diastolic organ, as in the gravid uterus, they are extended.

Now, dilatation of the heart is in a sense a partial and persistent diastole, a condition in which the threads of the neural screw must be somewhat extended, and it is conceivable that such extension may be associated with a degree of sensory stimulation, having in its train various degrees of motor consequence, to which reference will be made in their proper place. At present we are concerned with sensory phenomena, and we know that the sensory incentive to motion—a reflex—need not be experienced as a sensation unless the resultant motion be of a type to induce sufficient peripheral stimulation to cause a degree of pain. This, I believe, it may do. Then, one of the forms, the indirect form, of angina pectoris is the result. Stimulation of the cardiac nerves on this principle, which are not provocative of visceral spasm, may cause various irregularities of rhythm, to which also I shall refer later, as well as various degrees of sensory discomfort not amounting to angina which cardiac patients frequently experience.

We have seen that the cardiac nerves, in penetrating the heart, are more or less intimately associated with blood vessels, and in the case of the larger vessels this association may be seen very distinctly in some of the specimens I show. The nerves, in fact, on transverse section, may be seen to crown the arteries as the arteries themselves crown the heart. On longitudinal section, they may be seen to course with and around the vessels. We can understand, under these circumstances, in the event of a hardening and irregularity of the arteries, such as we meet with in advanced stages of arteriosclerosis with calcareous degeneration, when associated, as it frequently is, with a cognate change in the interstitial tissues of the organ, and in the interspaces along which the vessels and nerves find their way, how actual frictional contact with irritation of nerve trunks may induce frequent and ultimately fatal attacks of cardiac pain.

Angina pectoris, thus originated, may be regarded as the direct type of angina or visceral pain, like the pain caused by the passage of a calculus along the bile or urinary ducts.

It has been cogently objected to this theory of angina pectoris, that, were it true, angina should be more constantly associated with the conditions mentioned than it is found to be. Atheromatous arteries are frequently met with after death, which do not appear during life to have caused cardiac pain.

I trust I may not be thought to assume too dogmatic a tone in the presence of some whom I regard as the chief authorities upon the subject I am discussing, when I express my belief that the association of angina with calcareous degeneration of the coronary arteries and hyperplasia of interstitial cardiac tissue, is dependent in a great measure upon the degree of change in the vessels and in the channels in which they run, associated, it may be, in some cases, with a certain amount of visceral neuritis which, as we know, would be calculated greatly to increase the sensibility of the nerves involved.

Dealing as we are with the relation of the nervous system to visceral disease and disorder, I shall not be expected to enter fully into the discussion of alternative theories of angina pectoris. I do not deny that concurrent circumstances not strictly neural may so exalt the sensibility of the organism, local and general, as to render some cases more prone to visceral pain than others; but pain is a property of the nervous system, and I do not think that much will be gained by emphasising remote causes, and by overlooking those which are evidently more direct. As George Balfour remarks in the last edition of his classical work,[1] "angina is a nerve pain, a neuralgia of the heart; but it is something more, it is a neuralgia only developed on a sudden call for exertion. . . . Whenever, from any emotional cause, or from the slight exertion of ascending an acclivity, or from a sudden rise of blood pressure from reflex or other causes, the augmentor nerve has to make a call upon the heart for increased action, to which it is unable to respond because of long continued imperfect metabolism, . . . the call for increased katabolic action is at once followed by exhaustion revealed as agonising pain shooting along some or all of those sensitive spinal nerves with which the katabolic nerve is embryologically connected."

To this lucid description I can add nothing, except that, for the general term "exhaustion" in the above etiology, I should read more or less dilatation, and assign the subsequent neural stimulation to physical provocation from alterations in the tension and action of the cardiac wall.

There are those who have described an anatomical sensitisation of the endocardium, and if they be correct, it is conceivable that the incentive to angina from increased blood pressure within the chambers of the heart may be due to stimulation of such endo-cardial innervation. Clifford Allbutt also ascribes great influence to intra-aortic tension as an etiological factor in angina pectoris. For myself, however, I am more disposed to regard intramural and intra-aortic stimulation of nerves as the cause of those reflex muscular changes which induce visceral pain, just as I attribute to similar causes the essentially visceral pains associated with a uterus in rhythmical action of a certain intensity. Such conditions as favour a dynamic dilatability of the chambers of the heart, whether from local disease or general debility, are calculated to induce such stimulation and reaction as I have referred to, and which are most calculated to result in angina, when, to the indirect influences I have mentioned, there are added local causes of intramural stimulation of the cardiac nerves.

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[1] "Diseases of the Heart and Aorta," pp. 318 and 319.

Under the microscope and on the screen I have placed preparations from the heart of a man, aged 53, who was under my care at the Great Northern Central Hospital, with almost constant angina pectoris for eleven months, and who ultimately died during an attack of cardiac pain. The preparations are from the apical portion of the heart, as I wished to obtain tissue from the same neighbourhood as that from which I stained the nerves shown. The vascular, circumvascular, and intermuscular sclerosis is well seen, and is very suggestive of the possibility of that direct irritation of the concurrent nerves which I believe to be one of the causes of angina pectoris. It is a neural form of that extramural or exocardial variety of angina pectoris which Sir William Gairdner has described as being associated with the pressure of small aneurysms at the base of the heart upon sensory nerves in their immediate neighbourhood.[1] On the screen I have also thrown a delineation of the interior of the aorta in this case, and of the coronary artery. The first shows some occlusion of the coronary orifices, with dilatation of the pouches of Valsalva, which is indicative of excess of intra-aortic pressure, such as that which Clifford Allbutt regards as causal of angina in many cases, and the second exhibits the hard thickened lumen of the coronary vessels which crowned the heart in its most sensitive portion like a chaplet of thorns. This case has been published at greater length.[2]

So-called pseudo-angina is of two kinds, and in only one of these is the qualifying adjective appropriate. In a certain proportion of these cases I agree with Clifford Allbutt and others, that the condition is in reality a variety of intercostal neuralgia. The angina may be real enough, but the organ affected is not the heart. In others, again, the neuralgia dependent, it may be, on the ordinary causes of neuralgia, such as anæmia, occurring in predisposed or neurotic subjects, the seat of the disorder appears to be the heart itself, inasmuch as the precordial distress may be associated with inhibitory phenomena in that organ. These cannot be regarded as false but as true anginæ.

Painful affections of the heart may, as is well known, be associated with somatic points, areas, and radiations of pain, and Walshe has described the opposite process of peripheral aura resulting in central cardiac distress.[1] In the case which Walshe refers to, the pain began in the hand and spread up the arm to the chest. This must, of course, merely be regarded as the peripheral projection of a central stimulation such as the shoulder pain associated with some hepatic conditions, and other well-known somatic manifestations of visceral stimulation. For the local cardiac origin of angina is to my mind indubitable, and this conclusion has been impressed upon me by the situation of the anginoid pain in a rare case of dextral valvular disease of the heart, which I have published.[2] The specimen is in possession of this college, and I throw on the screen a copy of the drawing I made shortly after the autopsy. The hugely distorted and diseased pulmonary valves, as well as the diseased tricuspid orifice, ultimately induced a dyspnœa which was associated with pre-cordial pain of sufficient severity to cause radiation down the right arm, just as in the majority of cases of valvular disease which are sinistral the painful radiation, when not of sufficient severity to become general on that side, or even bilateral, are restricted to the left arm, and most usually to the course of the intercosto-humeral nerve, owing to its origin from the second intercostal, which itself arises from the second dorsal nerve, which also, as we know, supplies visceral branches to the heart.

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[1] Reynolds's "System of Medicine."

[2] *Treatment*, London, October 1897.

It does not follow, however, that cardiac pain may not have a truly extra-cardiac origin. Less frequently than in the case of some of the other viscera, the painful crises of dorsal tabes may affect the region of the heart, as is stated by von Leyden and Goldscheider,[3] who quote other authorities for the same fact. The attacks may have the severity of *angina vera*, and even threaten life. The pulse is described as being small, weak, and irregular. It is known, moreover, that cardiac may be superadded to the more common gastric crises of tabes. Assuming that in a given case the crisis induces cardiac and not *intercostal* pain mistaken for it, the site of stimulation in the majority of cases is probably that affection of the posterior root ganglion to which I referred in the last lecture, and the depressor phenomena which may ensue, due to an implication of the pneumogastric system by that cross-stimulation which causes retardation to follow acceleration and depression to modify augmentation, and *vice versâ*. Whether the sensory phenomena of such crises may originate, as some have supposed, in the pneumogastric system, may be left an open question. That the pneumogastric does not play the important part in the painful symptoms of visceral crises which was at one time assigned to it, is fairly certain in view of our present knowledge of the pathology of the disease. In some cases, however, there appear to be grounds for associating such cases with central pneumogastric irritation; for, although the nerve has a largely trophic influence, its spinal sensory fibres, coming as they probably do from the far-reaching solitary fascicular strand, may be regarded as quite as efficient channels for sensibility as the lower spinal nerves and posterior root ganglion.

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[1] "Diseases of the Heart and Aorta," 4th edition, p. 198.

[2] *Trans. Path. Soc. London*, 1876.

[3] *Op. cit.*, p. 544.

In 1878 I published in the March number of this Journal the case of a man who had glycosuria, with partial left hemiplegia, and well-marked left hemiparæsthesia. He had had syphilis thirty years before coming under my care. He died in an attack of angina pectoris. The anginal attacks only developed shortly before the fatal issue. Permission was only obtained to open the head. The value of the case is not therefore great for determining the question of a central causation of angina, but some of the intracranial conditions were well marked, and might be argued to have some bearing upon the short but persistent angina which killed the patient. At the autopsy, some convolutions, not definitely noted, were found to be softened. There were two small cystic cavities in the upper and posterior part of the right optic thalamus. There was also a well-marked granular gelatinoid degeneration of the floor of the fourth ventricle, which was most marked in the lower and left portion of that space. The acoustic striæ were obliterated by gelatinoid material in this situation. It is conceivable that in such a case as this the origin of cardiac pain might be in the higher centres or in the sensory nucleus of the pneumogastric nerve, and I have mentioned the case that I might not appear to emphasise the lower spinal etiology of angina pectoris too much. All the phenomena of sensory localisation and radiation, however, in cases of visceral angina, whether cardiac, hepatic, renal, or uterine, indicate the spinal rather than the cranial nerve supply of the heart as the chief territory of objective pain. This point would appear to have been first emphasised by Allen Sturge.[1] Ross[2] appears to have come to the same conclusion independently. Clifford Allbutt made a similar suggestion in his Goulstonian Lectures in 1884; and, while laying more emphasis than Sturge did on peripheral excitation, essentially the same conclusion impressed itself on me before I was aware of his valuable paper on the subject. No doubt others have and will come to a like conclusion without knowing anything of the work of any of us. The human mind, considering more or less carefully the same phenomena with an average knowledge of the underlying conditions, draws like conclusions, just as the eye, travelling with the same care over the same landscape as others, observes the same features as they have done. The petty little differences about priority in scientific matters, therefore, which some of our most distinguished men have indulged in, and which posterity reads with more amusement than sympathy, seem rather beside the mark. There is some sadness and much truth in the comment which Mephistopheles made on the exultant self-complacency of the young graduate in the second *Faust*, once the modest student whom we encountered last year, and which has been well done into English by Dr. Anster—

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[1] *Brain*, London, Jan. 1883, vol. v. p. 492.

[2] *Ibid.*, vol. x. p. 354.

> "Original! move onward in your pride,
> Oh! how the spirit would sink mortified,
> Could you but know that long ago,
> All thoughts whatever, dull or clever,
> That cross the twilight of your brain,
> Have been o'er and o'er again
> Occupying other men."

The inimitable playwright adds that the younger portion of the audience did not applaud.

(*To be continued.*)

A CASE OF ABDOMINAL ANEURYSM BURSTING INTO THE PSOAS MUSCLE, AND SUBSEQUENTLY INTO THE LEFT PLEURAL CAVITY.

By C. E. **Beevor**, M.D., F.R.C.P., *Physician to the National Hospital for the Paralysed and Epileptic, Queen Square, and to the Great Northern Hospital, London.*

The following case is of interest, as showing that an abdominal aneurysm may exist for a considerable time without giving rise to any but slight symptoms; that it may extend into a large muscle, such as the psoas, without the patient being aware of it; and may then burst upwards into the left pleural cavity, causing instant death.

The case is also of interest as it shows the importance of not overlooking very slight objective symptoms, in determining a diagnosis between organic and functional diseases.

The patient, Frederick B., æt. 44, traveller, came to my out-patient clinic at the National Hospital for the Paralysed and Epileptic, Queen Square, in February, complained of general weakness and pain in the spine and left hip, and numbness and weakness of the left thigh. On examining him, I found that he had tenderness over the spine, and in the left lumbar and iliac regions; there was no paralysis of the hip-joint, but the movements were weaker than on the other side; the left knee-jerk was decidedly less marked than on the right; and on the front of the thigh tactile sensation was impaired.

Suspecting a growth in the left lumbar region, I examined there, but could not find any evidence.

As there was no vacant bed at the National Hospital, I took him into the Great Northern Hospital, in a few days' time, on 25th February. From the history taken by Mr. Wadd, the house-physician, the patient was healthy and free from illness until five years ago, when he commenced to have constant aching pain in the spine, which at first was situated high up, but afterwards became localised to the lumbar and sacral regions (said to have followed influenza). Bowels have been constipated, and subject to attacks of colicky pain in the abdomen. Last November (three months before admission) he had pain in the left spine, and it became localised in the left hip; it was very severe and he had to take to his bed, as he had been following his employment up to then. Appetite failed, with rapid loss of weight since Christmas (two months). He was treated for an affection of the kidneys at St. Bartholomew's Hospital, in the previous June and July, as an out-patient.

ON ADMISSION. — Poorly nourished, emaciated man; complains of numb feelings down front of left thigh as far as knee, and pain which is confined to neighbourhood of great trochanter and iliac crest. No tenderness or pain along course of great sciatic nerve, and he does not mind pressure over it. No wasting of thigh muscles. Power in all muscles of left thigh is not so good as on right side, but he is able to completely flex thigh on body, and then extend leg without giving rise to pain. Knee-jerk on the left is not so brisk as on the right. Plantar reflexes present and equal on two sides. Slight impairment of tactile sensation down front of left thigh as far as knee.

Spine.—Pain on percussion over last two lumbar spines.

Chest.—Emphysematous.

Heart.—No physical signs of disease.

Pulse.—Regular, soft; rate, 84.

Abdomen.—Flat and resonant; no visceral enlargement; no mass felt.

Urine.—Sp. gr. 1020; no albumin; no sugar.

5th March.—Feels slightest touch over left gluteal region, but says it is not so distinct as over right. Tenderness in both iliac fossæ; nothing abnormal felt, pain most marked in right; also present over both posterior-superior spines.

22nd March.—General condition getting worse steadily, but there are no fresh physical signs.

27th March.—On examining abdomen, there was noticed a heaving expansile pulsation in the left lumbar region. Traced downwards, percussion dulness commences at eighth space in midaxillary line, and extends downwards to 1 in. above iliac crest; traced forwards, the dulness extends to a line 2 in. above and internal to anterior-superior iliac spine; and traced backwards it extends as far as the spinal column; behind, it extends downwards to the iliac crest. On palpation a distinct mass could be felt in the left lumbar region, the inner border of which is midway between the anterior-superior iliac spine and the umbilicus, but the border can be traced down to within 1 in. above iliac crest.

28th March.—Very marked bulging over lower ribs behind, extending down throughout left lumbar region. Urine normal. Knee-jerk, right still better than left; slight blunting of sensation on front of left thigh.

29th March.—The expansile pulsating swelling is rapidly increasing in size, and there is more pulsation. A distinct margin in front, midway between umbilicus and anterior-superior spine, extends lower down towards Poupart's ligament, and appears to be spreading by a well-defined wedge-shaped swelling. Marked projection of lower ribs is seen in front and behind. Patient is not tender to manipulation, and he has no pain over the swelling. Still feels indefinite pains round pelvis and down left thigh. No bruit heard over swelling. Heart sounds normal.

30th March.—Good night, slept well. Lying flat in bed reading paper, when he suddenly cried out, and died almost instantaneously, with extreme pallor. No previous coughing or straining.

COPY OF POST-MORTEM NOTE BY DR. SYERS (FORTY-EIGHT HOURS AFTER DEATH).—Absolute dulness on percussion over left side of thorax, both in front and behind, except under clavicle, where there is modified resonance on percussion.

On removing sternum, heart is seen pushed over to right. Left pleural cavity is completely filled with huge blood clot, forming an accurate cast of the cavity. Left lung small and airless.

Inspection of interior of abdomen shows a large convex mass occupying the whole of the left side of the cavity, and extending into the pelvis. This mass displaces the intestines to the right. Left kidney pushed forwards, upwards, and outwards, and the corresponding ureter is prominently conspicuous on the surface of the mass. Further examination proves the mass to consist of recent blood clot, which is entirely behind the peritoneum. The abdominal aorta at the level of the last dorsal and first two lumbar vertebræ is the seat of an aneurysmal enlargement. The entrance from the aorta is entirely posterior, measuring 1 in. in the long diameter, and ½ in. in the short; the edges of the aperture are rounded and smooth, and it opens at once into the aneurysm, which has evidently recently burst into the retroperitoneal tissue, and into the psoas muscle, which is hollowed out and full of blood.

The bodies of the last dorsal and upper two lumbar vertebræ are excessively eroded, the intervertebral cartilages being less affected than the osseous structure. The last two ribs are also much eroded on the left side.

Superiorly, it is found that the blood has ruptured the left side of the diaphragm close to its posterior attachment, and by this means has gained access to the left pleural cavity. The aperture is the size of a crown-piece. The abdominal aorta is atheromatous in places, but not excessively so.

Heart small, cavities empty; thoracic aorta throughout its course is somewhat atheromatous, but has not lost its elasticity. It is not dilated, nor is there tendency to aneurysmal enlargement in the aortic system; other organs healthy.

Left kidney markedly paler than the right; the reason is obvious, the left renal vessels being pressed upon.

REMARKS.—The salient points of the history are—(1) The pain, which was gradual, and occurred in the spine for five years, and for three months before admission, in the left hip. Whether this pain, lasting for five years, was part of the disease which proved fatal, it is not possible to say. (2) The numb feelings on the front of the left thigh as far down as the knee. (3) The progressing general wasting and weakness. In his present state, at the time when I saw him at the Queen Square Hospital, and on his admission into the Great Northern Hospital, the conditions were—(4) The weakness, without complete paralysis, of the left hip. (5) The deficient action of the knee-jerk on the left side as compared with the right. (6) The slight deficiency of tactile sensibility of the left thigh, especially on the anterior surface, and extending upwards along the abdomen to a level corresponding to a line drawn outwards from a point half-way between the pubes and the umbilicus, to just above the iliac crest on the left side, and also spreading round to the left gluteal region. (7) The tenderness over the two last lumbar spines, and on deep pressure in the left iliac fossa and left lumbar region.

The most important symptom was the difference of the knee-jerks on the two sides, and next in importance the slight loss of tactile sensation, and the weakness of the left leg. The alteration in the one knee-jerk was the most important, as it is a symptom which could not be feigned, and it was a certain sign of organic disease; the loss of power and sensation, though possible in a functional or malingering condition, taken along with the deficient knee-jerk, pointed to a lesion of the roots of the lumbar plexus, or of the spinal cord at the upper part of the lumbar enlargement. Now, as it is unusual for one-half of the spinal cord to be pressed upon without soon producing disturbance of the other side—which we had not here—I concluded that we had to do here with a lesion of the roots of the lumbar plexus; the severe pain in the hip was also more in favour of pressure on the spinal roots than on the cord itself, and also that there was no affection of the sphincter vesicæ or ani.

Looking at the anxious expression of the patient's face, and the emaciation, I thought, on admission, that it was probably a case of malignant disease in the lumbar region, situated in front of the psoas muscle, and pressing on the roots of the lumbar plexus; on examination, however, I failed to discover any growth in that region, or elsewhere.

While in the hospital the patient was very complaining, and behaved very much like a case of nervous exhaustion or hysteria, and if it had not been for the one objective sign of the deficient knee-jerk, corroborated by the deficient power and sensibility of the left leg, I think one might have easily been led to look upon this case as one of hysteria; and although the patient wanted to get up, he was kept resting in bed. This case, therefore, emphasises the importance of not overlooking slight objective symptoms.

Another point to which I should like to call attention, is the difficulty of detecting slight losses of tactile sensibility; in these cases it is not sufficient to touch the patient on a part to be tested, and ask him if he can feel it, but the part must be compared with a known normal part—the opposite side if possible—by touching both with the same degree, taking care not to move the touching finger from the spot to be tested.

From the post-mortem examination, it would appear that the aneurysm must have burst and slowly dissected its way into the psoas muscles, which it hollowed out with blood, without the patient being aware of it, as he had not complained of anything special on 27th March, three days before his death, when the aneurysm was first noticed.

Another point is that, although the bodies of the last dorsal and first two lumbar vertebræ were eroded, the erosion had not laid bare the spinal canal, and the maximum tenderness was over the two last lumbar spines and not the two first; but this is explained by the skin at the lower part of the back, at the level of the fourth and fifth lumbar spines, being supplied by the eleventh and twelfth dorsal roots, while the iliac crest, great trochanter, lower part of the abdomen, and the front of the thigh is supplied by the twelfth dorsal root and the three upper lumbar roots, while the knee-jerk gets its nerve supply from the third and fourth lumbar roots. Hence the pain along the left iliac crest and great trochanter, the impaired sensation and diminished knee-jerk.

THE AFTER-TREATMENT OF CASES OF GASTROSTOMY.

By R. H. Lucy, M.B., F.R.C.S., *Surgeon, South Devon and East Cornwall Hospital, Plymouth.*

The *essentials* required in any method of establishing a permanent opening into the stomach are—(1) That the operation selected should be simple in its details, and therefore capable of rapid performance; (2) that the resulting fistula should be a long one, and also preferably take an angular course; (3) that it should pass through muscle-fibres capable of exerting a distinct sphincter-like action on the walls of the artificial channel.

These indications are, I think, best fulfilled by the operation of Albert.[1]

The second may be attained by bending the cone of withdrawn viscus over the left costal margin, as originally carried out by Albert, but equally as well and with less drag on the stomach (and consequently less shock and pain), by utilising the left linea semilunaris and the outer fibres of the left rectus muscle, as the structures over which the stomach may be folded.

[1] Jacobson, "Operations of Surgery," 3rd edition, pp. 905, 906.

The third indication is secured by bringing the cone through the *separated* fibres of the left rectus muscle.

Another marked advantage of this method is the safety with which immediate feeding may be inaugurated—a very important point when dealing with patients whose dysphagia has become complete (aphagia) for some considerable time prior to operation. The absence of vomiting and the comfort thus afforded the patient are remarkable. In the three cases subsequently narrated, the following procedure was carried out:—

The incision was commenced above at the costal margin, just internal to the left linea semilunaris, and carried vertically downwards for 3 in.; the sheath of the rectus being opened, its fibres were separated with the handle of the scalpel, leaving a bundle about ½ in. thick to the left of the incision. All bleeding being stopped, the combined posterior layer sheath of rectus, transversalis fascia, and parietal peritoneum were picked up as a fold between two pairs of peritoneum forceps, and opened for the *upper* half of the incision only. Two fingers passed in sought the collapsed stomach beneath the diaphragm; the transverse colon and great omentum being pushed down, a long cone of the anterior wall of the stomach was then drawn through this "buttonhole," in the direction of least tension, identified as the viscus wanted, and a holding-up suture of silkworm gut passed through *all* its coats at the apex. This being drawn on by an assistant, a continuous suture of fine silk was placed round the base of the cone, uniting the combined fasciæ and peritoneum to the sero-muscular coat of the stomach, avoiding blood vessels, and taking care not to pierce the mucosa. The skin to the left of the incision was next undermined for 1 in., and a buttonhole ¾ in. long made in the skin, through which the apex of the cone was drawn. Four quilt sutures were inserted between the edges of the buttonhole and the apex, the bundles of the rectus below the emergence of the stomach from the abdomen united, and the original incision closed and covered with a gauze and collodion dressing. With the holding-up suture as a guide, a small stab incision was made at the apex with a sharp tenotome, and a No. 7 or 8 Jaques' catheter, stretched on a stout probe, sprung in and fastened *in situ*, by threading one end of the holding-up suture on a fine needle, passing it through the side of the catheter, and tying off. A feed of 4 oz. peptonised milk with 2 drms. brandy was then given through the catheter, a pinch-cock placed on the latter, and both wounds covered with wood-wool wadding and a binder. A strip of strapping split into two tails, passing one on either side of the catheter, kept the dressing from shifting.

COMMENTS.—This incision gives convenient access to the stomach, the latter can be drawn out without tension, and a suitable portion selected free from vessels and great omentum, and the smallness of the aperture through the peritoneum saves time in the suturing, and more accurate apposition of parietal peritoneum and stomach. The holding-up suture allows the cone to be manipulated easily, and, being passed through *all* the coats, ensures the mucous membrane not being pushed before the point of the tenotome when making the apical opening. Also, the sphincter-like action of the rectus fibres is retained by merely separating them, which also protects from subsequent ventral hernia. The catheter being put on the stretch during insertion, accurately fits the opening, and so prevents leakage at the outset; feeding the patient while still on the table goes far to minimise shock and vomiting, and does away with the necessity of continuing rectal feeding.

The majority of patients submit themselves for gastrostomy when much reduced in strength, from starvation and the toxæmia induced by the malignant growth in the œsophagus, the most usual cause for which such operation is required.

The main indications, then, in the *after-treatment,* are to supply—(1) Fluids for the working of the vascular system, the kidneys, and the bowels; (2) stimulants; and (3) food. The gastric functions having for long been in abeyance, the latter must be fluid, predigested, and given frequently and in small quantities at first.

For the first twenty-four hours 5 oz. of peptonised milk at 100° F., with ½ oz. of brandy, should be passed into the stomach every three hours. If thirst be urgent, an enema of a pint of tepid water may be given by syphon tube. Provided no vomiting occurs, it is well to give on the second day 10 oz. of peptonised milk and water, equal parts, at similar intervals; and as cough and tenacious phlegm troubles the patient and prevents sleep in nearly all cases (a true " stomach cough "), but especially in those whose stricture is situate high up, a 5-minim dose of nepenthe or laudanum should be included in an evening feed, and acts like a charm. On the third day dilute hot broth may alternate with the milk; and on the fourth, to give the gastric functions a chance of resuming active work, the milk should be half peptonised, half plain. If no discomfort is complained of, the former may be gradually replaced by the latter. Pepper and salt should be added to all feeds, to stimulate and aid digestion. Benger's food, thin gruel, beaten-up eggs, and various farinaceous preparations, may be added; and when the large-size tubes come to be worn, minced meat and vegetables may be passed down a large bore glass funnel (with the aid of a glass rod or plunger) at least once a day, with or without previous mastication by the patient.

The bowels should be relieved by simple or turpentine enemata, and later on aperients given with the evening feeds. Any attempt to swallow even liquids should be prohibited—it only causes choking, perhaps vomiting, and certainly strains the stitches and exhausts the patient. The sutures are removed on the tenth day, and if any signs of leakage of gastric juice be observed, the catheter is removed and a suitable size of flanged red rubber tube (such as are sold for empyema) is inserted, the skin round the fistula dusted with the dried carbonate of soda, and a piece of lint spread with boric ointment placed beneath the flange of the tube. A small indiarubber "cork" is used to plug the tube, and is removed at each feed. No tapes are required to hold the tube in position, the pressure of the binder on the flange being sufficient. Should the tube work loose or allow of leakage, the next size larger is inserted, and it is well to supply the patient with two of each size—one for wear, the other to be kept soaking in a saturated solution of borax in water, till changed for the soiled one. As a rule, No. 3 size succeeds the catheter, followed by Nos. 4 to 6, the largest allowing minced meat to be administered, as previously described.

To stimulate the already depressed vitality, all feeds should be given at blood heat. If the fistula rapidly enlarges and requires a constant increase in the size of tube, leaving out the latter between feeds will allow of a rapid diminution in the size of the fistula.

Case 1.—Mrs. B., æt. 63, was admitted into the South Devon Hospital with dysphagia of five months' standing: dilatation of the stricture, situated some 12 in. from the front teeth, was followed by improvement in strength and weight for six weeks; as she then began to lose weight, gastrostomy was performed on 16th April 1898, and she is now well and comfortable.

Case 2.—J. B., a watchmaker, æt. 70, was sent to me by Dr. Anderson of Bodmin on 2nd August 1898, with a history of less than three months' dysphagia, latterly become complete even for liquids. Used to be 13 st. 8 lb., but had lost 4 lb. in the past fortnight, and for the past three days had only been able to rinse out the mouth with water. A small bougie revealed an obstruction in the gullet, 8 in. from the front teeth. His weight when first seen was 10 st. 2 lb., a loss of 3 st. 6 lb. in three months. Gastrostomy was performed on 8th August; in this case small doses of nepenthe soothed the cough and hiccough in a wonderful manner. Later reports state that the growth is spreading upwards to the larynx, and that tracheotomy seems imminent, but that there is no leak or excoriation about fistula. He returned home on the eighteenth day after operation, wearing a No. 4 flanged tube, and greatly increased in weight and strength.

Case 3.—M. D., a pensioner, æt. 58, came under observation in March 1898, at the South Devon Hospital, complaining of regurgitation of fluids soon after swallowing, and of inability to swallow solids for the past six months. Nos. 15 to 20 œsophageal bougies passed occasionally enabled him to swallow liquids and remain stationary as regards weight, but on the 16th August he was readmitted for increasing dysphagia and loss of weight, and operated on the next day, a No. 10 catheter being inserted. No vomiting occurred, and he left the hospital after a good recovery on 15th September, but died on 23rd October, no necropsy being obtained.

All these cases were examples of malignant stricture of the gullet—No. 1 near the cardia, No. 2 below the larynx, and No. 3 near the cardia, with pouching of the œsophagus at one side, which usually arrested the bougie when passed 13 in. from the front teeth. Owing to the amelioration produced by occasional dilatation in this case, operation was postponed, with the result that death followed the operation in a little over two months. The want of fluid in the tissues in these cases is as striking as the want of food, as evidenced by their shrunken aspect, the feeble cardiac action and diminished volume of the pulse, the scanty high-coloured urine, and the obstinate constipation. For the above reasons it is most important that rectal feeding be established some days before operation, and with as large a volume of fluid as the rectum will tolerate.

INSTILLATIONS AND THEIR VALUE IN THE TREATMENT OF CYSTITIS.

By A. A. Warden, M.A., M.B., C.M., *Resident Surgeon, Hertford British Hospital, Paris.*

> "La tension d'une vessie enflammée, pour peu qu'elle se prolonge ou se repète, est une des conditions qui permettent à la vessie d'influencer la destinée du rein ; nous aurons à y revenir, car d'un bon traitement de la vessie dépend souvent la guérison d'une maladie grave du rein."—Guyon, "Leçons cliniques sur les maladies des voies urinaires."

There are only two main principles in the treatment of inflammation of the bladder, no matter what the particular nature of that inflammation, and they are—(1) Local; (2) general.

The popular one to this day, and, I believe, the less efficacious of the two, is the general treatment by medicinal and hygienic measures.

The object of the following remarks is to prove that, while these general principles may not be altogether neglected, should indeed in many cases be carefully insisted upon, a local antisepis of the bladder is of primary importance, and is of such simplicity and safety as to be within the reach of every practitioner.

Recent knowledge of the nature of infective diseases would seem to prove that we might rely greatly, in our attack upon the invading micro-organisms, upon the reinforcement of the general resistance. Our treatment of many of the microbic diseases, *e.g.* pneumonia, specific fevers, etc., is mainly a tonic one, or in more recent years and in such diseases as diphtheria, hydrophobia, tetanus, etc., by the specific antitoxin. It is, indeed, a question that is still unanswered, whether the antitoxins do not act, in part or wholly, by their tonic, strengthening effect upon the organism. Roux, one of the foremost representatives of Pasteurism, in his recent address at Buda Pesth (1894), states, for example, that rabbits protected by injection against hydrophobia survive an injection of four or five times an ordinarily fatal quantity of serpent's venom. In the same way we may explain the good effects of large injections of saline solutions in various infective conditions, namely, its general, tonic, stimulating effect, a method of treatment that has lately been greatly practised, and that has been found of at least temporary value *in articulo mortis* in many diseases. The injection of a considerable quantity of salt solution, or merely warm water, high up into the rectum, has long been practised, *e.g.* in such conditions as profound anæmia, and collapse after hæmorrhage.

And, to come closer to our subject, the investigations of Bouchard,[1] upon the influence of the intestinal contents upon the toxic powers of the urine, and the fact that the common pyogenic microbe of urinary infection—the *Bacillus coli communis*[2]—has its habitat in the intestine, prove that local agents are not our only means of modifying the contents of the bladder. The clinical experience, further, of each one of us can probably furnish cases of cystitis, with recovery as a concomitant, if not a consequence of the use, by the mouth, of such medicinal agents as salol, boracic acid, potus imperialis, or perhaps only milk in considerable quantity, or barley or lime water,—all excellent diuretics.

Against these facts place our knowledge that infected urine is, except in the case of the tubercular form, the active cause of the cystitis, and that it is possible, directly and locally, most effectually to modify in the bladder the toxic nature of that urine, and it might be difficult, *a priori*, for our reason to give the preference to either of the two methods of treatment. Happily, we can appeal to a judge who can give a more decided answer, against which there is no appeal, namely, clinical experience. (A well-known dictum of Professor Guyon, which, once appreciated, is never forgotten.)

No specific antitoxin has as yet been discovered to combat the effects of urinary infection; medicinal treatment, therefore, relies upon no direct germicide. We may make the urine a less favourable medium, we can flush out the kidneys; but neither stomach nor kidneys are tolerant of large quantities of santal, salol, soda, etc., and the quantity must be large to act effectively upon processes in the bladder. Direct vesical antisepsis is, in this respect, very greatly superior to an antisepsis that can only act *viâ* the kidneys.[3] Guyon's experience bids him state his belief firmly, that *medicinal* antisepsis plays an extremely small part in the treatment of urinary infection in its acute or chronic forms,[1] and general treatment may practically be limited to tonics and stimulants and dilute diuretics.

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[1] "Leçons sur les auto-intoxications," Paris, 1887.

[2] Albarran, "Le rein des urinaires," Thèse de Paris, 1889. See also Guyon's "Leçons clin.," tome ii.

[3] Guyon, *Mercredi méd.*, Paris, July 30, 1890.

The symptoms of cystitis are too well known to require repetition. There is one, however, which not only is the foundation of all, but which also gives the key to the treatment, namely, the bladder's *intolerance of tension.* This intolerance explains most of the other symptoms, the frequent, urgent, painful micturition. The distention of the congested bladder causes instant and powerful contraction, with perhaps hæmaturia, and almost constant agony.

We have here one reason why the patient should object to a treatment which increases the secretion of urine, and therefore the desire to micturate and the painful contraction of the bladder. Large and even moderately small local injections are, for the same reason, absolutely contra-indicated. We must fall back, therefore, upon the use of instillations, *i.e.* a method of locally applying, *drop by drop, an active medicinal solution to the inner surface of the bladder.* They were first described by Guyon,[2] and the thirty years that have elapsed since then have been spent, thanks to the perfection of the technique and the recognition of the value of certain substances, in the realisation of the hopes that they then raised.

The strength of the solutions used, the small number of drops, and, above all, the slowness with which, one by one, they reach their destination, are the important points to be attended to. Instillations cannot be hurriedly given, or they become injections, and lose the character of instillations.

Strength of solution.—The solution, though by no means always a caustic one, is usually strong, far stronger than could be used in the form of an injection in the ordinary sense. This fact is of as great value as it is in the use of anodynes, and constitutes indeed the chief virtue of instillations.

Instruments and technique.—The instruments required are simply a small graduated syringe and the instillator, which is a perforated, gum-elastic, olive-tipped bougie, the well-known *bougie exploratrice olivaire,* with a stem of somewhat greater calibre to permit of the entrance of the solution.

A hypodermic syringe will do excellently, being graduated and fitted with a nose-piece, which may be adjusted to the instillator. This adjustment must, however, be exact, as the length of the stem, and the smallness of its calibre, makes considerable pressure necessary. In the special syringe devised by Guyon, the content is 4 grms. or about 1 drm., the piston advances upon a screw (as well as by simple pressure), thus ensuring the introduction drop by drop of the solution.

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[1] "Leçons clin.," 1896, 3rd edition, tome ii. p. 42.

[2] *Bull. Soc. de chir. de Paris,* 1867 ; see also "Leçons clin.," 1897, 3rd edition, ome iii. p. 395.

To ensure, furthermore, the entrance of the solution in single, successive drops, the olive of the instillator must be finely perforated, the calibre of the tube must be small and even throughout its length. Such instruments made of gum-elastic, are preferable to the solid metallic instillator of Ultzmann and others, except that they are not so easily or rapidly sterilisable.

The syringe is filled and accurately fitted into the instillator. The piston is then advanced by turning the screw until the interior of the stem be filled with solution, so that no air may enter the bladder. It will then be seen that each half turn of the screw expels one drop. It will be wise, in view of the difficulty of perfect sterilisation of an instrument of so small a diameter, to follow Guyon's suggestion, and inject the whole content of the syringe through the catheter, and then proceed to the instillations. The patient, of course, should, as in all instrumentation of the urethra, when possible, first micturate, and the anterior urethra should be washed out by a syringe and a solution of boracic acid.

Instillations may be urethral and vesical. Of urethral instillations, with which we are not immediately concerned, we will only make two remarks. For the posterior urethra the olive should be passed through, and then drawn gently back against the membranous portion, and a considerable quantity of the solution, twenty, thirty, or even forty drops, slowly introduced, the bulk of which, of course, finds its way into the bladder. The olive is then drawn through the sphincter, a few drops being expelled in the transit, and a second instillation is made for the anterior urethra, ten, fifteen, or twenty drops sufficing. If localisation to the posterior urethra alone be desired, the instillation is made as described, the bladder, however, being filled with a solution of boracic acid, or the patient having been instructed to retain his urine. Inject only about five drops, which are at once sufficiently diluted by the contents of the bladder. To instil into the bulbous urethra, which so often holds the dregs of a chronic discharge, use a large olive, and withdraw it a little in front of the sphincter, easily recognised by the increased resistance, maintaining the olive in place for a few minutes after the instillations, and thus necessitating its entrance into the *cul de sac* of the bulb. For all instillations, an olive of twelve to sixteen will generally suffice, eighteen to twenty being reserved for localisation to the bulb.

Instillations into the bladder.—In the first place, the bladder should be empty. The patient should urinate if he can, the catheter must be used if he cannot. In the latter case, and provided the bladder can support a moderate amount of tension, it may be washed out through the catheter with boracic acid or weak nitrate of silver solution, 1 in 1000.

In the second place, the quantity used may be greater than that used in urethral instillations, but should never exceed the contents of one syringe, *i.e.* about a teaspoonful.

In the third place, as every cystitis is accompanied by posterior urethritis, the instillation will best enter the bladder after traversing the posterior urethra, *i.e.* all behind the membranous portion. Only in cases of hypersensitiveness on the part of the patient, need the instillation be introduced directly into the bladder.

In the fourth place, it may be remarked that the strength of the solution that may be used is as great as its quantity must be small. Thus, with nitrate of silver, the bladder could not be washed out with a stronger solution than 1 in 500, yet as instillations, solutions from 1 per cent. to 2 per cent., and sometimes 3, 4, and 5 per cent., may be used with perfect toleration, both by the urethra and by the bladder in the majority of cases.

Injections of solutions of perchloride of mercury may be used in the proportion of 1 to 20,000 and 1 to 10,000, but as instillations the strength of 1 in 5000 and 3000 is of daily use, and some patients even support 1 in 1000. Above 5 per cent. nitrate of silver becomes a caustic, and indeed should not be used in greater strength than 7 to 8 per cent., a solution that has as caustic an effect as the solid stick. In such strength the quantity used should never exceed three to six drops, and should be introduced with extreme deliberation. Dilute solutions will be found of more use in daily practice.

We may divide the substances that will be found of most use into—(1) anæsthetics, (2) antiseptics.

1. LOCAL ANÆSTHETICS.—These are of great value, both as treatment in themselves, and as a preliminary to the instillations of solutions more or less painful. It is unnecessary, perhaps, to add that, whatever anæsthetic be used, its absolute sterilisation, in spite of the presumably infected state of the bladder, is essential.

Antipyrin.—In mild cystitis in which there is no excessive tenderness to tension, and which it is intended to treat with nitrate of silver, antipyrin, dissolved in ten times its weight of water, undoubtedly renders the mucous surface of the bladder less sensitive to the search of the nitrate.

Guaiacol.—In very acute cystitis, both antipyrin and cocaine yield in efficacy, as a preliminary injection to the medicinal agent it is intended to use, to guaiacol, in the formula that Professor Picot of Bordeaux uses for hypodermic injection—

Iodoform	1 part.
Guaiacol	5 parts.
Sterilised olive oil	100 „

of which solution 1 drm. may be injected thrice daily without any inconvenience.

Cocaine has now, thanks to Reclus of La Pitié,[1] been so studied, that we now know what is safe and what is dangerous.

The entire quantity used in solution should not exceed 1½ grs. of the hydrochlorate of cocaine. The solution should be 1 per cent. of recent preparation, and administered with the patient in the dorsal decubitus. This rule may, to a certain extent, be modified, and solutions may be required as strong as 2 per cent., or may suffice as weak as 1 in 200 or 1 in 400. A good rule, however, is never to exceed the injection of 1½ to 2 grs. of cocaine.

2. ANTISEPTIC INSTILLATIONS.—It must not be thought that the substances referred to are the only ones with which experiments have been made. They are rather those which have won, and which hold, after many years' trial, a position of trust. For one reason and another, but only after long trial, Guyon has given up the use of such substances as chloral, salicylic acid, salicylate of soda, sulphate of copper, hydrochloric acid, and even carbolic acid, iodoform and formol, from which so much might reasonably have been expected.

Boracic acid, which Pasteur recommended to Guyon in 1876, and which has been in regular use since that date, is of comparatively little value in the minute quantity of an instillation. The same may be said, though for other reasons, of permanganate of potash. Both should only be used in quantity to wash out the bladder. A 5 per cent. solution, *i.e.* a supersaturated solution, of boracic acid may be prepared with warm water, and the addition of borate of soda in the strength of 1 in 200, the proportions thus being—

Boracic acid	5 parts.
Borate of soda	½ part.
Warm water	100 parts.

This may be used as an instillation alone, or combined with perchloride of mercury in equal quantity, 1 in 10,000. *Perchloride of mercury* in solutions of 1 in 5000 or 1 in 3000 will be found of immense service.

In the constant and often prolonged agony of tubercular cystitis, instillations of a 1 in 5000 solution of sublimate is an invaluable adjunct to the general treatment, diet, hygiene, climate, etc., that every form of tuberculosis demands.

Nitrate of silver was introduced by Mercier in 1842, and is perhaps the most valuable local agent in the treatment of cystitis. It seems of even greater effect in the form of instillation than when used in more dilute solutions to wash the bladder out, perhaps from the fact that, in the former case, the solution is left in the bladder, and the more slowly the drops have been introduced, the more thoroughly do they come in contact with its mucous membrane. The more acute, the more painful the cystitis, the more frequent the desire to micturate, the more clear is the indication for instillations of nitrate of silver. Hence its effect, little short of astounding, in acute gonorrhœal cystitis.

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[1] "La cocaine en chirurgie," 1895, and "Anæsthesie locale par la cocaine," *Bull. Acad. de méd.*, Paris, May 1896.

Laudanum added to boiled water, or sterilised and injected through the instillator, is one of the best local anodynes in many forms of cystitis. It must, however, be first rendered sterile. Guyon considers that enemata of starch and laudanum are equally efficacious.

CLINICAL RECORDS.

A CASE SHOWING HEREDITY IN HEART DISEASE.

By G. Crewdson Thomas, M.B., *House Physician, Royal Infirmary, Edinburgh.*

Mrs. R., a housewife, æt. 67, was admitted on 4th May 1898 to Ward 27, under the care of Dr. Gibson. She complained of a severe cough, shortness of breath, and a feeling of oppression in the chest.

History of the Patient.—*Hereditary tendencies.*—There was a very marked rheumatic history which could be traced through four generations, as illustrated by the appended chart. The patient's paternal grandmother (M. S.) suffered from rheumatism, and died suddenly from heart disease. She had two children, both of whom had a rheumatic taint. The elder (T. T.), who was the patient's father, had an attack of rheumatic fever about the age of 20, and for the rest of his life was troubled with his heart. He died suddenly at the age of 54. The younger child (A. T.) attained the age of 40, when she also died suddenly. The exact cause of death was unknown to the patient, but probably it was due to a cardiac lesion. The patient herself had seven brothers and five sisters—all of whom, except two brothers, died in infancy from unknown causes. Her elder brother (T. T.) died, at the age of 74, from heart disease. He never seems to have had rheumatic fever. Her younger brother (W. T.) had rheumatic fever at the age of 16, and died when aged 40 from heart disease. He was married and had nine children, all healthy.

The patient had one son (J. R.), who had rheumatic fever three times—the first time at the age of 20; the second time five years later; he had since then been an invalid, from cardiac disease. He married, and his two children were quite healthy.

The patient's surroundings had always been comfortable. She had measles, scarlet fever, and whooping-cough as a child. At the age of 30 she had typhoid fever. She never had rheumatic fever, but she has suffered from rheumatic pains in her joints for the last thirty years. During the last ten years she had bronchitis every winter. Up to six months ago she had never suffered from any shortness of breath, but she then found that she had to rest several times when ascending a stair, and also that any exertion immediately brought on palpitation of the heart.

The illness for which she was admitted, commenced with a troublesome cough following a slight cold. The patient began to experience a great oppression in her chest whenever she tried to lie down; she was also troubled with an uneasy feeling below the left mamma.

General facts.—The patient was a spare, frail-looking old woman, with a weary, anxious expression. On admission she breathed with difficulty, and had to be propped up in bed.

Circulatory system.—She suffered from a pain over the cardiac region, as above mentioned. This pain was always worse at night. During the day it seldom troubled her.

On inspection a diffuse pulsation was seen over the whole of the præcordium; this was most marked from the fourth to the sixth interspace. Pulsation was also seen in the epigastrium and veins of the neck.

On palpation a forcible cardiac impulse was felt, most pronounced in the sixth left interspace, 4½ in. from the midsternal line.

On percussion the base of the heart was at the lower border of the third rib. The right border was 1½ in., and the left border at the level of the fourth interspace was 5 in. from midsternum.

On auscultation in the mitral area the first sound was loud, and was accompanied and followed by a soft blowing systolic murmur, propagated into the axilla. The second sound was accentuated.

In the tricuspid area the first sound was accompanied by a systolic murmur. The second sound was accentuated.

In the pulmonary area a systolic murmur was heard. The second sound was accentuated and reduplicated. In the aortic area a faint systolic murmur was distinguishable, the second sound being markedly accentuated.

The pulse.—The artery wall was thickened and tortuous—rate 98 per minute—very irregular in time and force. The tension between the beats was high. The arteries all over the body are sclerosed and tortuous, especially the temporal arteries.

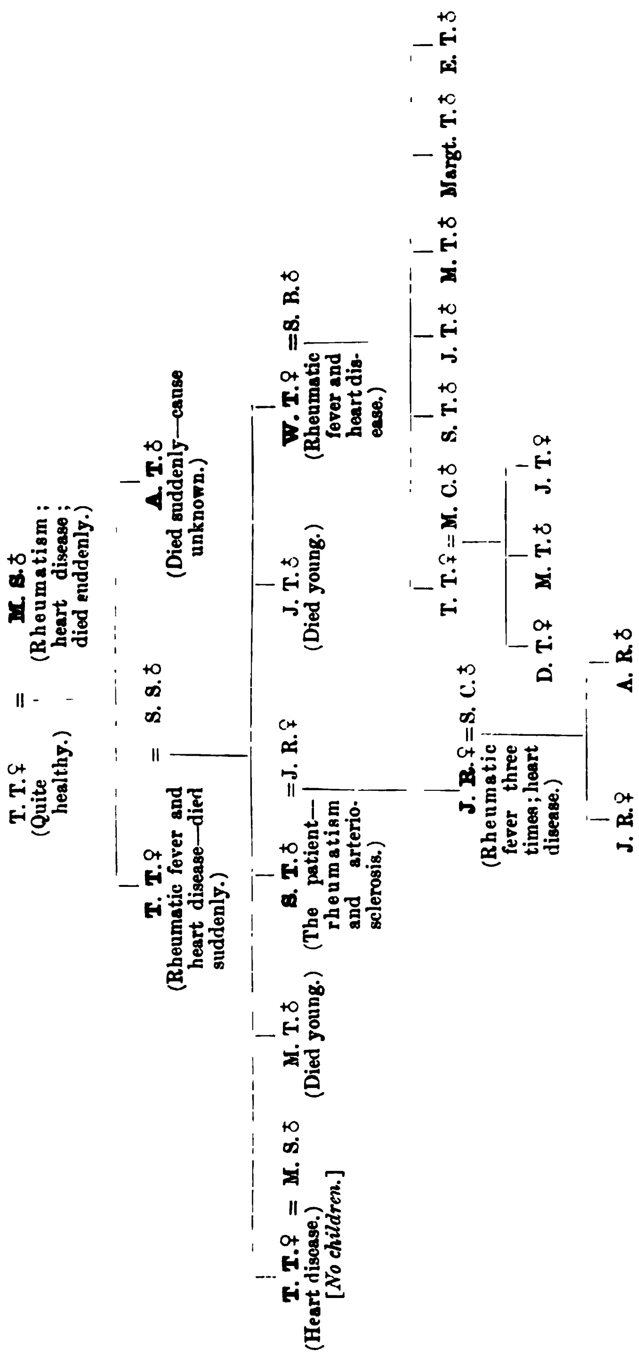
Respiratory system.—The patient suffered considerably from dyspnœa on any exertion, as before mentioned. There was no œdema of the bases. The physical signs consequent upon chronic bronchitis and emphysema were well marked.

Locomotory system.—The patient had suffered from rheumatism in her wrists and ankles for the last twenty-five years. This was usually not severe, but occasionally she had been compelled to rest in bed, owing to her ankles becoming swollen. This was especially the case with the right ankle. For the last few years they had not caused her any trouble. At the time of admission there was no swelling, but both ankles were rather stiff. The interpharyngeal joints of her hands were enlarged and nodulár.

The other systems did not show any points directly bearing on either the rheumatic or cardiac conditions.

As an interesting example of the hereditary transmission of tendencies to cardiac disease the case is well worthy of being placed on record.

Chart showing Mrs. R.'s family history.


T. T.♀ (Quite healthy.)
=
M. S.♂ (Rheumatism; heart disease; died suddenly.)
T. T.♀ (Rheumatic fever and heart disease—died suddenly.)
=
S. S.♂
A. T.♂ (Died suddenly—cause unknown.)
T. T.♀ = M. S.♂ (Heart disease.) [*No children.*]
M. T.♂ (Died young.)
S. T.♂ (The patient—rheumatism and arterio-sclerosis.)
=J. R.♀
J. T.♂ (Died young.)
W. T.♀ (Rheumatic fever and heart disease.)
=S. B.♂
J. R.♀ (Rheumatic fever three times; heart disease.)
=S. C.♂
T. T.♀ = M. C.♂
S. T.♂
J. T.♂
M. T.♂
Margt. T.♂
E. T.♂
J. R.♀
A. R.♂
D. T.♀
M. T.♂
J. T.♀

REVIEWS OF BRITISH AND FOREIGN LITERATURE.

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Lectures on Giddiness and on Hysteria in the Male. By Sir THOMAS GRAINGER STEWART, M.D., LL.D. Edinburgh and London: Young J. Pentland. 1898.

SIR THOMAS GRAINGER STEWART'S "Lectures on Giddiness," of which a second edition has just been published, probably contain the most complete account that has ever been given of this affection, which, sometimes a mere symptom, must at others be called a disease.

Everyone knows what giddiness or vertigo is, in one or other of its forms; but nothing can be more difficult than to define it in words, more particularly as it does not always mean the same thing. In one case external objects seem to circle round the subject, or the floor tilts up or recedes; in another, the patient is conscious that it is his own head that swims, and there is no apparent shifting of his surroundings; in a third, he is confused by double vision, or by seeing objects out of the perpendicular, and the floor sloping. It would conduce to clearness of description and comprehension if we had separate words for each variety, and it would be a point gained if we could be quite sure whether the difference between the first and second variety above mentioned were merely one of degree or really one of kind.

The author describes giddiness as having for its essential feature a feeling of uncertainty of our position in space, relatively to surrounding objects, and as constantly associated with impairment of equilibrium. He further points out that giddiness is the sensory correlative of staggering, and staggering the motor correlative of giddiness. These, which approximate as nearly to definitions as the subject permits of, and the explanation of giddiness of peripheral origin as due to contradictoriness of sensory impressions, are distinct acquisitions to the study of giddiness.

Giddiness from contradictory sensations is illustrated by the uneasy feeling experienced in the leaning tower of Pisa, by cliff, rotation, and swing giddiness, and by the giddiness of sea-sickness. Our author includes agoraphobia in this catalogue. But while an ingenious explanation of the dread of open spaces is given from this point of view, the cause would seem to lie in the cortical centres to which sensations are referred rather than in the sensory impressions themselves. There is claustrophobia as well as agoraphobia, and it would be difficult to apply any similar train of reasoning to this affection.

The most common cause of ocular giddiness is double vision, paralysis of one or other of the external muscles. Perhaps that due to paralysis of the fourth nerve is the most troublesome, as the divergence between the two images is increased when the eyes are directed downwards. A most interesting case is related of giddiness and staggering from miner's nystagmus, the giddiness ceasing when the eyes were fixed by pressure.

The most distressing form of vertigo is Menière's disease, or labyrinthine vertigo, characterised in well-marked cases by suddenness of onset, apparent rotation or oscillation of external objects, and liability to fall. In severe examples there is vomiting, and a distressing sense of faintness or actual syncope. The fall is often due to an instinctive attempt to avoid an imaginary danger of falling in the opposite direction; the patient in fact throws himself down. This is unquestionably due to some disturbing influence in the semicircular canals which constitute the main organ of equilibration. This, as is shown by illustrative cases, may have its origin in the external meatus or in the middle ear, and an important practical inference is the necessity for a careful examination for any such cause. When the vertigo is due to disease affecting the labyrinth itself, so that the lesion is out of reach, treatment must be directed to the maintenance of the nervous tone. This affection is a striking illustration of the fact that a persistent morbid condition may be attended with only occasional symptoms. The most probable explanation is, that the nervous system can resist the disturbing influence up to a certain point, and that the supervention of symptoms may be determined either by an increase of irritation or a diminution of the resistance. Charcot's recommendation of quinine in the treatment of Menière's disease, which is quoted with approval, must, so far as it is successful, act by increasing the nervous stability, which is better than dulling the nervous sensibility by bromides. Aural surgeons, however, not unfrequently warn their patients against quinine.

Giddiness may be among the effects of lesions of different parts of the nerve centres, more frequently of the cerebellum and its crura than of the spinal cord or cerebrum, as might perhaps have been expected, since disease of the cerebellum is often attended with staggering, the motor correlative of giddiness. The semicircular canals, moreover, probably report to the cerebellum. Giddiness, however, does not often enter into the diagnosis of central lesions. An important precaution is insisted upon, when giddiness is present in association with spinal or intracranial disease, namely, that one eye should be closed, since the giddiness may be due to double vision.

A form of giddiness which it is most important to recognise is the *vertigo a stomacho læso.* It is common, often gives rise to serious alarm, and it is curable. Accordingly, it is fully considered, and to this part of the subject we would specially call the attention of practitioners. In the milder degrees of stomach giddiness, it is described as a swimming in the head; and this may go so far as to render the patient unable to cross a room, except by the aid of chairs and tables, without implying apparent circuitous movement of external objects. If it could be definitely made out that stomach vertigo was always of this character, and that auditory vertigo was always attended with objective translation of one sort or another, it would be a great practical gain. The author is disposed to attribute the *vertigo a stomacho læso* to ptomaine poisoning, but this is scarcely consistent with the fact that it may be at once relieved by eructation. Poisons, however, may give rise to giddiness—alcohol, for instance, and tobacco; and if there is any truth in the story of the drunken man waiting at the lamp-post till his own house comes round, the vertigo is attended with apparent circulatory coursing of surrounding objects.

It has of course been impossible to take up all the points in these lectures, but sufficient has perhaps been said to send the reader to the little book itself. The case of hysteria in the male, bound up with the lectures on giddiness, is a curious and interesting study, and we can imagine it furnishing material for an evening's discussion at a medical society. One of the symptoms was a peculiar rhythmic corkscrew gait, an ingenious record of which is furnished by a photograph of the footprints along the ward. W. H. Broadbent.

Gout: its Pathology and Treatment. By Arthur P. Luff, M.D. (Lond.), F.R.C.P. (Lond.). London: Cassell & Company Limited. 1898.

This modest volume is largely the outcome of the Goulstonian Lectures on the "Chemistry and Pathology of Gout," delivered by the author before the Royal College of Physicians of London in 1897. The results of more recent investigations respecting the treatment of gout have been incorporated, and a detailed account of the treatment of the various forms of gout has been added.

The late Professor Hughes Bennett used to remark that "anyone could write a big book, but it took a very able man to write a little one." Even a cursory examination of the volume before us enables us to apply to it the latter part of this somewhat cynical utterance.

Dr. Luff plunges at once into his subject, and criticises the various theories now promulgated respecting the pathology of gout, to wit—(*a*) The view that excess of uric acid is the primary cause; (*b*) that morbid changes in the structure of the tissues are the primary cause; and (*c*) that nervous disturbance is the primary cause.

Under the first head he discusses the views of Sir Alfred Garrod and Sir William Roberts, both as to regular and irregular gout, and he agrees with the latter that uric acid is not a toxic agent in the system. He further discards the theory of Kolisch, that the alloxur bases (antecedents or allies of uric acid) are responsible for a degree of renal degeneration which leads to the development of gout, and an increased output of these bases. The views of Ebstein, which regard necrotic changes in the affected tissues as the primary cause of gout, the necrosis being due to the influence of dissolved urates, and the results of his experiments on birds, and with urates, he considers disproved by those of Roberts, which directly contradict them, and by the observations of Garrod, Cornil, Ranvier, and the reviewer, as well as by his own, which show no necrotic areas in textures impregnated with uratic deposits. He agrees with Ord and others that such deposits are commonly to be found in tissues which have begun to degenerate. A great advance in our knowledge was made by Roberts, who showed that uric acid is primarily taken up by the blood and lymph as a quadriurate and not as a neutral urate, and that the former never induces necrosis in any tissue. Dr. Luff next discusses Cullen's views as to nervous disturbance regarded as the primary factor in the production of gout. Cullen was the great opponent of the humoral views which prevailed in his day, and he altogether dropped that factor in the pathogeny of gout. The progress of physiological chemistry at a later date showed that Cullen was in error in this matter. Scheele and Wollaston prepared the way for Garrod's epoch-making discovery of sodium urate in the blood of gouty persons in 1847. The influence of the nervous system in respect of gout was henceforth forgotten, and humoral doctrines again prevailed. About forty years ago, Cullen's views were revived and associated with the humoral doctrine. The latter was too well established to be ever again completely displaced by any theory. It was Laycock, Cullen's successor in the Edinburgh Chair of Physic, who thus promulgated what has been since termed by his pupil, the reviewer, the neuro-humoral theory of gout. This view is accepted by Edward Liveing and P. W. Latham, and it mainly consists in the conception that there is a functional disorder, probably in the medulla oblongata, of parts concerned in general trophic influences, whereby there is generated an excess of uric acid, and an inability to destroy it in the tissues. The centre for joints is believed to be situate in the medulla oblongata. In gouty paroxysms there is held to be a temporary failure of excretory power in the kidneys, due to nervous inhibition. The view of inherited gout (primary) held by the reviewer is, that there is a uratic toxæmia induced by this neurosis, under little or no specific provocation; while in acquired gout (secondary) the toxæmia is induced by habits of life which provoke it; and if with this there arise depression or nervous exhaustion, a gouty neurosis may be established by the influence of the toxæmia upon the special trophic nerve centres. The caprices and paroxysmal quality of gouty attacks of all kinds appear to indicate the nervous origin of the outbreak, and the instability and sensitiveness of the nerve centres determine the special features of it. The late Dr. Ralfe believed that the uratic accumulation in the blood was due to non-conversion into urea of the uric acid normally found in the tissues, and that this was due to disturbed innervation.

It is obvious that no one who is not a good physiological chemist, as well as an experienced physician, is qualified to deal with the difficult problem presented in the study of gout. Our author is, happily, well equipped in both capacities, and we therefore lean on his opinions with confidence. Dr. Luff next deals with the cause of the presence of uric acid in the blood of the gouty, and of the deficient excretion of it in their urine. Careful experiments and sagacious reasoning prompt our author to affirm that, in health, uric acid is only formed in the kidneys, and never appears in the general circulation except in morbid states. This view disposes of the theory that urichæmia is due to diminished destruction of uric acid by oxidation or otherwise. Hence we have to deal with (*a*) normal production and deficient excretion, or (*b*) with over-production and normal excretion. Now, over-production does not necessarily lead to gout, as is proved by its occurrence in leukæmia and anæmia; nor, though the production of uric acid is greatest in the young, does gout commonly occur in them. Dr. Luff's experiments prove that deficient excretion of uric acid is the rule in gout, and that the occurrence of urichæmia is due to subsequent absorption from the kidneys. He prefers the Gowland Hopkins method for the estimation of uric acid in the urine, and he has devised a process of his own for determining the amount present in the blood.

He is led by his researches to believe that the urichæmia of blood diseases, such as leukæmia, is due to different conditions from those which obtain in health, and that in these it may be formed in other organs than the kidneys; while in gout the source of the uric acid is the same as that which furnishes it in health. He next indicates that uric acid is not a normal constituent of the blood, and shows how Garrod came to change his views on this point by certain experiments, and to regard urichæmia as due to absorption from the kidneys of the uric acid formed there. He showed, and Luff also, that uric acid is not present in the blood of mammals (other than man), birds, or reptiles. The urichæmia of various blood disorders Dr. Luff believes to be derived from the nuclein of the leucocytes. The kidneys in these cases, however, being adequate for excretory purposes, no gout occurs.

This brings the author to the view that gout has its origin in renal inadequacy, and he quotes copious evidence in support of it. This was Garrod's original conception; but our author believes, and we agree with him, that gouty affections of the kidneys are not always revealed clinically. He places the primary defect in the tubal system of the organs, and conceives that the sclerosis of interstitial change is secondary to this. Respecting the formation of uric acid, Dr. Luff brings evidence to prove that urea is at least one source of it, and that this and glycocine are converted into uric acid by the agency of certain of the renal cells. It is right to add here, that this view is not held by many physiologists, and that probably the last word has not yet been said on this matter.

The liver has long been regarded as a source of uric acid in the system, but our author would limit the hepatic agency in this metabolism to its production of the antecedents of uric acid, in the form of urea and glycocine, the final conjunction of these to form uric acid taking place in the kidneys.

Dr. Luff discredits Dr. Haig's view that uric acid, as such, is taken into the system in certain food-stuffs. He finds the ratio of uric acid to urea to be about 1:42, varying, however, from 1:28 to 1:55. He accepts Sir William Roberts' views as to uric acid being present in the blood in gout as a non-toxic, soluble sodium quadriurate. This is deposited as a biurate, which is a foreign body wherever it occurs.

In part ii., Dr. Luff discusses the etiology and clinical features of gout. With reference to saturnine gout, he regards the noxious influence of lead as falling upon the kidneys and checking eliminative power in those organs, thus predisposing the system to gout. In his account of the gouty paroxysm, he makes no reference to Dr. George

Balfour's recent view, which regards this process as mainly due to thrombosis of small vessels in the site of election, and holds to the orthodox idea of gouty inflammation, prompted by deposition of irritating uratic matter. All the recognised varieties of gout are noticed succinctly, and by the light of clinical observation.

Part iii. is occupied mainly by the author's investigations of certain points connected with the treatment of gout. These relate to inquiries as to the conditions affecting the solubility and precipitation of sodium quadriurate and biurate. Dr. Luff combats the view that diminished alkalinity of the blood favours uratic deposition, and the belief that increased alkalinity of it dissolves and sweeps out uratic accumulation. Sodium biurate is not more soluble in highly alkaline fluids. Nor does he sanction the belief that uric acid may be precipitated from the blood by certain metallic salts in insoluble form. After much labour and research on original lines of inquiry, our author draws the following conclusions:—

That the alkalinity of the blood is not appreciably diminished during a gouty attack; that a diminished alkalinity of the blood by organic acids does not affect the solubility of uric acid in it, nor does an increased alkalinity increase the solubility of deposits of sodium biurate.

That the gout-inducing properties of certain wines are not due to their acidity, but are probably due to their disturbing effect on hepatic metabolism.

That the solubility of sodium biurate is markedly increased by the mineral constituents of most vegetables, and diminished by the mineral constituents of meat. The most active of the former are spinach, Brussels sprouts, French beans, cabbage, turnip-tops, turnips, and celery.

In the treatment of gout, Dr. Luff believes colchicum to be of high value in diminishing the formation of uric acid, and suggests that its specific anodyne quality depends on its action as a cardio-vascular depressant. Its cholagogue action further checks the production of the antecedents of uric acid in the liver. Guaiacum, he thinks, acts similarly on the liver. Salicylate of sodium he considers harmful, or at least unavailing, and we agree with the latter view. Citrate of potassium is recommended as a diuretic, also to render the urine alkaline, and the quadriurates more soluble and stable than they are in acid urine. A limited use of common salt is recommended for the gouty, owing to the power it possesses of diminishing the solubility of sodium biurate, and so hastening its precipitation. A chapter on the diet for the gouty is added, with excellent directions for the various meals, and one on the use of mineral waters ends the book.

We have nothing but commendation for Dr. Luff's treatise. His views will be criticised, but they come with force from one who is fortified by full and wide knowledge of the difficult problems he has to deal with. He has mastered the best works on the subject, and his opinions are the outcome of well-planned experiments, chastened by clinical experience. As we have already affirmed, it is not given to many to be so well equipped for the task in hand as Dr. Luff. The literature of gout is enormous; dogmas and plagiarisms, however, make up a large part of it. The views set forth in this little volume bespeak our confidence, and certainly afford much matter for further study and reflection. Unlike many literary efforts that have preceded it on the subject, Dr. Luff's book does not "smell of the lamp," but, on the contrary, it is redolent of fruit freshly gathered from the field of honest work.

DYCE DUCKWORTH.

Ueber Malaria—und andere Blutparasiten, nebst Anhang eine wirksame Methode der Chromatin—und Blutfärbung. Von Dr. HANS ZIEMANN, Marinestabsarzt. Jena, G. Fischer, 1898.

DR. ZIEMANN is already known as the author of two important monographs, one on blood parasites in the malaria of temperate and tropical countries, the other on the morphology of the malaria parasite. The work before us deals more fully with the same subjects, with the addition of a short section on the parasites of Texas fever, and a much more complete account of the blood parasites of birds. In an appendix extending over thirty-two pages he explains his new method of staining, especially adapted to reveal the minute structure of blood parasites.

It will thus be seen that this is not a treatise on malarial fevers for the use of the practitioner, but an investigation into the morphology and minute structure of the parasite of human malaria, and the manner in which its evolution is influenced by drugs and other agencies and conditions. Whatever light can be derived from the comparative pathology of blood parasites in the lower animals, is here brought to bear upon the subject.

Dr. Ziemann has devoted himself to the study of malaria in Germany, Italy, and West Africa, and is thus exceptionally qualified for the task he has undertaken. We shall briefly mention some of the conclusions he has arrived at, without attempting to criticise them. Some of them cannot certainly be accepted without further evidence, but they will receive the consideration due to the reasoned opinions of an able, painstaking, and independent observer.

The large pigmented spheres of the simple quartan and tertian, as well as the crescents and crescent-derived bodies of the small parasite with motile pigment in coarse granules or rods, and the flagellated bodies, are classed as sterile or cadaveric forms. This view, which, if accepted, would lay the axe at the root of one of the most hopeful theories respecting the extra-corporeal life of the parasite, supported so far by observations, is based partly on the rounded form of the spheres, which is to our author a sign of death, but mainly on the destruction of the chromatin, or nucleolar substance, as revealed by his staining methods. He lays down the two following rules:—(1) That a direct proportional relation exists between the decay of the vitality, especially of the reproductive power of the parasite, and the increase of pigment, and *vice versâ.* (2) That sterile forms are characterised by an abnormal increase in the size of the parasite. These rules, he holds, apply alike to the large and small parasites.

To the objection that fever has followed the injection of blood containing apparently nothing but crescents, he replies that in all instances of this kind there is reason to believe that small parasites in an immature form were also present, although unobserved, along with the crescents.

He has not been able to satisfy himself of the existence of a special capsule in the crescent, nor does he agree with Mannaberg that they are derived from the conjugation of two amœboid bodies.

Dr. Ziemann recognises only three forms of parasite—that of the large parasite quartan and tertian, and the small parasite forms associated with æstivo-autumnal and tropical fevers. He doubts the existence of an unpigmented quotidian parasite, and does not admit that the parasite of the æstivo-autumnal quotidian differs specifically from that of the tertian. While allowing that the symptoms of fever vary greatly in different localities, he does not look upon these differences as indicating parasitic variations peculiar to these localities. He has had occasion to make the important observation, that the small parasite of tropical malaria undergoes no modification when the patient is removed to a cold and healthy climate.

His views upon the mode of infection are conservative. In discussing Bignami's theory, that the mosquito takes up the virus present in the soil of malarious localities and inoculates it into man, he points out that subcutaneous injection of malarial blood is only rarely followed by infection, and that, in most cases, it requires the endo-venous injection of not less than 2 c.c. of blood to attain this end. Assuming that the virus in the soil is not more potent than that in malarial blood, he remarks that an extraordinary number of mosquito bites would be necessary to produce infection. He says that the Cameroons, one of the worst haunts of malaria, is little troubled with insect pests of the mosquito kind, and that he never heard that mosquitoes were more common than usual in fever years. In short, he falls back upon the old doctrine, that the virus is in the soil, finds its way into the atmosphere, and enters the system along with the inspired air.

In experimenting on the action of drugs on the parasite, he found that phenocoll hydrochlorate had no effect on the development of the parasite. His observations on the action of methylene blue did not lead him to share the views of those who consider it to be a substitute for quinine. It appeared to have little or no influence on the parasite. His directions for the administration of quinine are marked by a good sense, often wanting in those who write from the purely scientific point of view. His clinical experience in Italy and West Africa has evidently not been thrown away. He points out that infection may take place, and parasites appear in the blood, while the only symptoms may be *malaise*, loss of appetite, headache, pains in the back and limbs, and anæmia. The temperature in his own case did not rise above 38° C. In this premonitory stage, the development of a severe infection may be arrested by the prompt administration of quinine. He counsels the earliest possible administration of the drug in primary cases. In tropical fevers of the remittent type, in which successive batches of parasites are coming to maturity at short intervals, the frequent administration of moderate doses of quinine, without reference to the state of the parasite, is advised. This is wise advice. The state of the patient, *not* that of the parasite, should be the guide in such cases. In simple quartan and tertian he thinks the quinine is best given some five or six hours before the accession. Perhaps this is the case, but the exact time of administration is really a matter of less consequence than many suppose. The state of the patient's stomach is also to be considered in the mild forms. He strongly recommends intramuscular injections in severe fevers, as preferable to the subcutaneous method, which in his hands, he says, produced "extraordinary unpleasant gangrene of the skin," even when all antiseptic precautions were taken. The precaution neglected, leading to these unpleasant results, was doubtless not pushing the needle *through* but *into* the skin. It will readily be understood that when one aims at lodging the charge into the substance of the gluteal muscles, there will be little risk of skin gangrene. The intramuscular method is a good one. His advice as to doses is eminently sound. He does not advocate the absurdly massive doses which some have recommended. He seldom required to give more than 2 or 3 grms. a day, even in severe West African fevers; in milder fevers 1 grm. a day was found sufficient. He recommends that the use of quinine in smaller doses, and at longer intervals, should be continued for some time after all the blood parasites have disappeared from the peripheral circulation, in order to prevent relapses.

It is impossible in the space at our disposal to describe Dr. Ziemann's staining method. This is the less necessary, as no one who is at all interested in blood parasites will be without this very instructive and suggestive work. ANDREW DAVIDSON.

Pathological Anatomy. By Professor O. BOLLINGER. Two volumes. London: Baillière, Tindall, & Cox. 1898.

THIS, which forms Vol. VI. of Messrs. Baillière, Tindall, & Cox's "Handy Atlas Series," includes illustrations and short descriptions of the naked-eye appearances of diseased organs of the circulatory, respiratory, and digestive apparatus, including the liver, bile ducts, and pancreas. The volume is exactly what it pretends to be, illustrated with short and pithy descriptions of the more important lesions met with in the post-mortem room. Professor Bollinger has taken a series of typical specimens, has had their appearances reproduced with great accuracy, and then in a few words draws attention to the special features to be observed in each plate, with the result that any fairly intelligent student who will examine these plates carefully will enter the post-mortem room with some knowledge of what he has to look for; whilst, after he has seen the various conditions here described on the post-mortem table, he will, by a rapid glance over the plates, be able to refresh his memory as to the appearances presented.

It may be well to state at the outset that the plates are of very unequal merit. Some of them are admirable—Plates 2A and 2B, representations of the heart, being models of what such plates should be—but a few of the others, especially those of the heart and blood vessels, and a few of those representing diseased conditions of the lung, are not by any means good examples of reproduction of pathological appearances. They are hard, flat, and smudgy, and although there may be a general resemblance to the diseased conditions that they are intended to represent, they certainly would afford no information to a student who had not already seen the diseased conditions; and even then in many cases he would be at a loss to reconcile the picture with the specimen. After making this criticism, and stating that here and there, throughout the work, similar faults may be met with, one has nothing but praise for the remainder of the pictures; and we think that any student, whether qualified or not, who will study this work even moderately carefully, will find that his post-mortem work will be of much greater value to him than if he enters upon it without any such preparation. The artist seems to make a specialty of tuberculosis, for certainly his best pictures are those in which this condition, as affecting the various organs, is represented. Some of the commoner processes, such as fatty and nutmeg liver, are also well reproduced.

At the end of the volume is a short summary of about 180 pages, in which the various conditions illustrated in the earlier part of the work are dealt with more or less systematically, and the links between the various processes delineated in the pictures are described. One cannot expect to have a complete text-book under such conditions, but it must be evident that if the student would devote more of his attention to observing the conditions here described, and fill in the gaps from his own observation, his text-book knowledge would be not only very materially added to, but in many cases would be corrected and extended.

In Vol. VII. of the same series we have a second part of the same work. One cannot but be struck by the fact, either that the subjects treated, taking them altogether, have lent themselves more to good illustrations, or that the author has in most cases been more fortunate in the selection of his subject, as, with the exception of one or two drawings of the kidney, of the spinal cord, and of the brain, the plates are really excellent, though probably the weakest plate in the whole work appears in this volume. The other illustrations in this volume represent common and typical diseases of the kidney, of the nervous system and bones, of the urinary apparatus, and of the sexual and reproductive organs. Everything that was said in favour of the first volume may be repeated concerning the second, and in this part especially the medical practitioner will find in handy form appearances which he will recognise as constantly coming within his ken in post-mortem work, however few his post-mortem examinations may be during the year.

The work of the printer has been well done, and we strongly recommend these two volumes to the notice of those who wish to acquire, or to keep up, a knowledge of morbid anatomy.

G. Sims Woodhead.

Stöhr's Text-book of Histology. Edited by Dr. Alfred Schaper. Second American, from eighth German edition. London: J. & A. Churchill. 1898.

A year ago the first American edition of Professor Stöhr's well-known text-book was published, and was then reviewed in this Journal. A second edition, revised and enlarged, has now appeared, showing that the book has been appreciated by the students in this country and America, just as the original has always been the favourite text-book of the German student. The more frequently one studies Stöhr, the stronger does the feeling become, that after all it is the most useful and interesting text-book of histology, within moderate compass, that at present exists. There are, of course, points omitted in the description of the structure of some tissue or other; but for brevity and aptness in the descriptive text, and for simplicity and clearness in the illustrations, it practically stands alone. The book has been carefully edited by a well-known and able histologist, who has, in addition, had the good fortune of having acted previously as docent of anatomy in the Zürich Anatomical Institute. The parts dealing with general and special technique are fully and clearly written, so that the book is essentially one for the working histologist, as well as for the general student.

A short description of the various sections of this text-book was given in the previous review, so that it is unnecessary to do more just now than draw attention to any special features in this new edition. Many new illustrations have been added, the larger number appearing in the last German edition, while we have to thank the editor for the inclusion of ten additional ones. The technique in many parts has been greatly improved, new methods being described so fully that there is no necessity, in the majority of cases, to apply to the original papers. The sections dealing with the nervous and alimentary systems have been brought well up to date, while in the chapter on the eye a few excellent illustrations have been added. The sections dealing with the blood and bone-marrow, although short, are excellent; here again the technique being a strong point. Among so much that is good it seems invidious to pick out some points that might bear a little improvement. The structure of the spleen is hardly sufficiently explained, although what is given is certainly up to date, and the description of striped muscle is still rather scanty. On every page one meets with Americanisms, with which we are gradually becoming conversant. It is essentially a *useful* text-book, simply and clearly written, and edited by men who are working histologists.

T. H. Milroy.

Inflammation of the Bladder and Urinary Fever. By C. Mansell Moullin, M.D. Oxon, F.R.C.S. London: H. K. Lewis. 1898.

This excellent book embodies the great increase in knowledge which has been gained in recent years, from the application of the principles of bacteriology to the diseases of the urinary bladder. It has the great merit of containing, in a very readable form, much valuable information which is diffused in English and foreign journals, and which is therefore inaccessible to the general reader. The special work done at the genito-urinary cliniques in Paris, Copenhagen, and elsewhere is well presented. The practical aspects of cystitis in relation to treatment are by no means neglected; the only omission which attracted our attention is that of the use of suction drainage of the bladder, through a retained catheter, on the principle of the Sprengel pump, introduced by Mr. C. W. Cathcart, and very generally adopted as the best means of keeping the cavity of the bladder empty.

Mr. Moullin's style as an author is very attractive, his clinical descriptions are most interesting and suggestive. Altogether the work reflects the greatest credit upon its talented author.

ALEXIS THOMSON.

The Surgical Anatomy of the Lymphatic Glands. By CECIL H. LEAF, M.A., M.B., F.R.C.S. London: Archibald Constable & Co. 1898.

THIS small book represents one of the many fruits which may be looked for as an outcome of the introduction of formalin into the study of anatomy. The superior hardening and preserving properties of this fluid has created a new era in the study of human anatomy, more especially as regards the topography of the viscera. Mr. Leaf is to be congratulated in having applied the method to so much advantage in a department of anatomy which, although of the foremost importance to the surgeon, is too often neglected, not only by the student, but we fear also by his teacher. Mr. Leaf has done much to remove such an opprobrium. In the limited space of seventy-two pages, and with the aid of eighteen admirable coloured plates, he has given, in clear and concise language, all that it is important for us to know about the situation and relations of the various groups of lymphatic glands. In a future edition we trust the author will be able to fulfil his intention, and give us more information regarding the drainage areas associated with the various groups of glands he so well describes, as this is the aspect of the subject regarding which the surgeon is most in need of accurate information. An important outcome of Mr. Leaf's work is the demonstration, apparently for the first time, of the existence, at any rate in the groin, and therefore probably elsewhere also, of communications between some of the lymphatic trunks and the small veins.

We regard Mr. Leaf's small volume as an important contribution to anatomical and surgical literature.

HAROLD J. STILES.

RECENT ADVANCES IN MEDICAL SCIENCE.

MEDICINE.

UNDER THE CHARGE OF

A. LOCKHART GILLESPIE, M.D., F.R.C.P.Ed.,

MEDICAL REGISTRAR, EDINBURGH ROYAL INFIRMARY.

TETANY.

KUCKEIN (*Berl. klin. Wchnschr.*, 7th November 1898) gives an account of an instance of what he calls latent tetany in a case of carcinomatous obstruction at the pyloric orifice, leading to excessive dilatation of the stomach. The patient was 48 years of age, a merchant, of Russian extraction, who, but for an attack of jaundice when 18 and of erysipelas when 28 years old, had always been healthy, until four years previously, when he began to suffer from gastric pains and paroxysmal vomiting attacks. No blood was ever detected either in the vomit or the stools. The later history of the case points to the absence of serious symptoms, and the maintenance of weight until six weeks before he was first seen by Kuckein. In these weeks he lost flesh rapidly, became extraordinarily thin, and on admission to hospital was unable to answer or recognise anyone. Temperature 96°·4, pulse 124, weak. The contour of a greatly dilated and disturbed stomach could be seen reaching a hand's-breath below the umbilicus. More than a litre of dark sour-smelling contents were drawn off, containing no free hydrochloric, but much lactic acid. The urine contained albumin and casts. The nervous symptoms were pronounced. Pupils and disc normal, knee-jerk absent, pressure over the internal occipital sulcus caused Trousseau's phenomenon, the typical position of the hand gradually disappearing when the pressure was withdrawn. The patient only lived twenty hours, during which no tonic contractions of the muscles occurred, but frequent brief spasmodic contractions of the muscles of the face and extremities.

Post-mortem, the stomach filled two-thirds of the abdomen, the dilatation being due to a carcinomatous tumour almost closing the pyloric orifice. Secondary growths were found in the liver, lymphatic glands, and peritoneum. No invasion of the nervous system could be found. The kidneys were in a condition of slight acute parenchymatous nephritis.

Kuckein regards the case as one of auto-intoxication, in which the symptoms of true tetany failed to be exhibited naturally, but which could be developed to some extent by artificial means.

Sievers (quoted *Arch. f. Verdauungs.*, Berlin, Bd. iv. S. 387) gives details of two typical cases of gastric tetany. The first, a servant-girl, æt. 22, with a history of only a week's illness, was admitted into hospital with a temperature of 104°·5, pulse 144, vomiting bad-smelling masses, and with tonic contraction of almost all the striped muscles of the body. She died in forty-five minutes. The stomach was found to be enormous, U-shaped, the pylorus narrowed, and showing the cicatrices of two old ulcers. The second, a woman, æt. 42, dyspeptic for years, was suddenly seized with severe pains in the arms and legs, stiffness, and almost opisthotonos. Death, one hour after admission. The stomach was found capable of holding 8 litres of water (280 oz., or 1¾ gals.); the pylorus was greatly contracted and showed the scar of an old ulcer. Sievers remarks, after a critical survey of the twenty-seven recorded fatal cases with autopsies, that the etiological factor of Bournet-Devié—hyperchlorhydria—is not essential for the causation of gastric tetany.

Jürgensen (*Deutsches Arch. f. klin. Med.*, Leipzig, Bd. lx. S. 327) ascribes the very similar symptoms observed by him in a patient with dilated stomach, from pyloric stenosis, to the presence of a condition of uræmia.

A very useful paper upon tetany accompanying gastric conditions is that of Gumprecht (*Centralbl. f. innere Med.*, Leipzig, 19th June 1897), based upon the symptoms observed in two cases, and on the results of a very extensive investigation into various of the attendant phenomena. The conclusions arrived at may be shortly noted. As the contents of the stomach in such cases, or alcoholic extracts of them, have no toxic properties in animals, while a single albumose can be separated which acts in a marked toxic manner, but which does not appear in the urine, auto-intoxication cannot be regarded as the underlying cause. The toxicity of urine collected during an attack proved to be of double the strength of normal urine, but this proportion did not vary at all between attacks. This toxicity seemed to depend largely on the salts present in the urine. He finally states as his opinion that with our present poverty of methods of examination we are not able to assume a correlation between any poison in the body and attacks of tetany.

Tetany of gastric origin is also considered by Coyon (*Gaz. d. hôp.*, Paris, 3rd Sept. 1898) in the course of a paper on pyloric stenosis. Severe types of gastric dilatation following the stenosis may exhibit delirium, tetany, or coma; the tetany especially in cases of retention. Kussmaul attributed it to dehydration of the tissues owing to non-absorption of fluid, Bouchard to an auto-intoxication. Whatever the causal agent, three forms were recognised—(1) Mild, confined to hands and feet; (2) almost simulating tetanus; (3) epileptic in type. In such cases the stomach should at once be emptied and washed out through a tube, chloral and bromides given, the nervous and cardiac depression combated with injections of ether.

Preble (*Medicine*, Detroit, 1898, vol. iv. p. 1) notes a case of gastrectasis with tetany, along with the "so-called pulmonary hypertrophic osteo-arthritis of Marie."

In an acute case recorded by Berlizheimer (*Berl. klin. Wchnschr.*, 1897, No. 36), the post-mortem yielded no evidence, physical or chemical, in support of the auto-intoxication theory. Berlizheimer blames a reflex action, starting from the irritated gastric nerves, for the tetany symptoms seen.

Schultze (*ibid.*, 1st March 1897), dealing with the relations between tetanus and psychoses, concludes that a close connection existent between the causal agents of tetany and of psychoses can be predicated. The first may appear and even replace a mental affection for a time during its course, while the muscular movements of tetany may correspond to a disturbance of the motor neurones, akin to that of the sensory and higher cerebral neurones so common in mental disease. A useful summary of the literature and opinions held as to the nature of tetany will be found in the *Boston Med. and S. Journ.*, No. 20, vol. cxxxv. p. 513, in which Lothrop notes that it is rare in America, but that he has had an opportunity of personally studying these cases. He emphasises the following diagnostic points:—(1) Characteristic tonic spasms, continuous or intermittent, from one to fifty a day, lasting a minute or two up to an hour or more, of all the muscles of the body in severe cases, of only the hands and feet in the majority. The hand becomes in appearance the "hand of the gynæcologist"; the foot *talipes equinus varus.* (2) Trousseau's phenomenon—production of spasm by nerve pressure. (3) Chvostek's phenomenon—increased mechanical excitability of motor nerves. (4) Erb's phenomenon—increased electrical excitability of all nerves.

The onset of tetany in twelve of Billroth's patients, after excision of the thyroid gland, was of much interest; eight of these rapidly proved fatal, two suffered from the disease in a chronic form, while in only two the symptoms disappeared.

Fischl (*Deutsche med. Wchnschr.*, Leipzig, 1897, Nos. 10 and 11) found in 170 children with symptoms of tetany and laryngismus, twenty-five instances of actual and eighty-three of latent tetany (presence of Trousseau's sign), forty-nine with true laryngismus, and thirteen with facial muscular excitability. Of the 108 tetany cases, 60·4 per cent. were rachitic, 56 per cent. had laryngismus, while of the laryngismus cases 63·8 per cent. were rachitic, 59 per cent. had tetany. He groups rickets and the two nervous types together, as probably caused by a common lesion, be it gastro-intestinal, or infectious.

Oddo (referred to *Allg. Wien. med. Ztg.*, 16th February 1897 *et seq.*) states that in children the temperature seldom rises except in severe cases, and even then only if a relapse follows close upon the primary attack; in the primary attacks being intermittent in type, in the relapse more continuous, but seldom exceeding 101°·3; now and then reaching 104°. The urine often contains an excess of phosphates; at the time of the attack, albumin; and, but only after thyroidectomy, excess of mucin. He distinguishes between the last-mentioned form, the tetany in ergot and chloroform poisoning, the hysterical, and the true tetany of children (tetanilla of Althaus). The true type is divisible into latens, fugax, frustes, simplex, chronica, and gravis.

Rommo (*Gaz. hebd. de méd.*, Paris, 1897, No. 7) gives a thorough abstract of the subject of tetany in children, and inclines to the hypothesis of a specific poison acting on the central nervous mechanism, a view supported by the occurrence of epidemics of the disease.

Meinert records a case of tetany (*Arch. f. Gynaek.*, Berlin, 1898, Bd. lv.) in a woman, who had had, at the fourth month of pregnancy, half the thyroid gland removed for dyspnœa. The symptoms commenced three days after,—painful cramps of the muscles in the hands and feet,—and continued for fourteen days. The next pregnancy was artificially arrested, in view of a return of the disease. Very similar symptoms to those enumerated above, as accompanying a form of intestinal catarrh, are described by Lesage (*Rev. d'hyg. thérap.*, Paris, 15th June 1897), but these nervous phenomena are easily removed by administration of purgatives and enemata.

Kunn (*Deutsche med. Wchnschr.*, Leipzig, 1897, No. 26) draws attention to the occurrence of cramps of the ocular muscles, alone or along with those of other muscles, in tetany; while Stransky (*Prag. med. Wchnschr.*, 1897, No. 23) puts on record the appearance of symptoms of tetany in a case of phosphorus poisoning.

Ebstein (*Prag. med. Wchnschr.*, 1896, No. 43) refers to kindred cataleptic symptoms in rachitic children; Escherich (*Berl. klin. Wchnschr.*, 1897, No. 40) refers to the frequent occurrence in rachitis to idiopathic tetany, often latent; not due to rachitis but arising from the cause thereof; gastro-intestinal tetany; tetany in the course of infectious diseases; pseudo-tetany, more chronic in nature, possibly hysterical in origin. See also Lloyd (*Journ. Nerv. Ment. Dis.*, Chicago, vol. xxiv. pp. 11, 703); Bendix (*Charité-Ann.*, Berlin, Bd. xxii., quoted *Schmidt's Jahrb.*, Leipzig, 1898, S. 294).

M'Kendrick (*Lancet*, London, 24th September 1898) reports another case of tetany in gastrectasis. A man, æt. 26, ill four days with painful cramps and tingling, but with a history of indigestion for twelve years. Patient's temperature was 98°·6, pulse 96, of moderate tension; and castless albuminuria, mental debility, and slow reactive power were present. No signs of a dilated stomach could be detected ante mortem; post mortem (death on the ninth day of attack), a great gastric dilatation, with pyloric cicatricial contraction, was found. The paper also takes cognisance of former theories and symptoms already recorded.

SURGERY.

UNDER THE CHARGE OF

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The Final Results of Operations for Extroversion of the Bladder.

At the summer meeting of the Surgical Society of Berlin, Sonnenburg exhibited several patients operated upon by his own method, which consists in excision of the malformed bladder and in stitching the lower ends of the ureters in the dorsal groove of the penis. He classified the results of the different operative procedures as follows:—

GROUP I.—In which the normal course of the urine is diverted—(1) Excision of the bladder; implantation of the ureters in the groove of the penis; Sonnenburg's operation; seven cases without a death; absence of kidney complications; incontinence; patient wears a urinal. One case has been under observation sixteen years. (2) Implantation of the ureters in the rectum without excising the bladder, performed by Roux, Simon, Holmes, Lloyd. (3) Implantation of the ureters, with extirpation of the bladder, as performed by Maydl; fourteen cases with two deaths. Longest observation of patient, fifteen months. The original continence gradually diminished; danger of pyelonephritis.

GROUP II.—In which the edges of the cleft in the bladder are brought together—(1) Direct union (Rigaud, Wymann), the result a failure. (2) By separation of the recti muscles (Schlange, Mikulicz). Four cases with three failures and one fistula; incontinence in all. (3) Separation of the innominate bones at the sacro-iliac joints by the method of Trendelenburg; six cases with three deaths, two fistulæ, and one complete union with incontinence.

GROUP III.—Plastic operations, in which a new anterior wall is made for the bladder—(1) By simple transplantation of skin flaps (Fort, Wood, Thiersch). The bladder is of insufficient capacity, is liable to incrustations and the formation of stone, and is incontinent. (2) Separation of the mucous membrane of the bladder along with skin flaps (Segond, Czerny); two cases, with persistence of a fistula in one of these; incontinence.

Sonnenburg criticises the results of the above procedures as follows:—The formation of a new bladder is of no advantage, unless the patient acquires control over the urine, and, so far, this has not been obtained. In the erect posture the posterior wall of the bladder projects forwards, and encroaches on the capacity of the organ. The various plastic operations are only of service, in so far as they may allow of the wearing of a urinal. This result is most easily and satisfactorily obtained by the author's method of excising the bladder and implanting the ureters in the groove of the penis. The transplantation of the ureters into the sigmoid flexure or rectum entails the liability to ascending infection and pyelonephritis.—*Centralbl. f. Chir.*, Leipzig, Oct. 22, 1898, No. 42.

INJURIES OF THE CERVICAL PORTION OF THE THORACIC DUCT.

WENDEL reports five cases from Küster's clinique, in which the thoracic duct was wounded in the course of operations in the supraclavicular triangle,—thrice in removing cancerous glands, once in tubercular, and once in malignant lymphoma. In one case the escape of chyle was only observed during the operation, and the wound healed well after plugging and secondary suture. In the other cases the chyle escaped for a varying period, and was regarded as a very undesirable complication. Of all the means employed to arrest the leakage, most success was obtained from plugging the wound. The variation in the duration of the escape of chyle is less dependent on the treatment adopted than on the anatomical arrangements of the duct. In investigating the latter, the author found in eight preparations out of twelve that the duct presented numerous accessory channels besides the main one, opening into the angulus venosus, which would amply suffice for the conveyance of the chyle into the circulation. The author has also been able to confirm what has already been observed by Wutzer, that there may exist communications between the duct and the azygos and renal veins (four times in twenty-nine observations), so that, when the normal outlet for the chyle is interfered with, it may make its escape by this collateral circulation; this will be rendered more likely if the wound in the neck be firmly plugged, so as to raise the pressure in the lower portion of the duct.—*Centralbl. f. Chir.*, Leipzig, Oct. 29, 1898, No. 43.

ON THE TREATMENT OF FRACTURES BY WEIGHT-EXTENSION.

BARDENHEUER has introduced certain modifications in the above treatment, which are of considerable practical importance. Bardenheuer's observations were communicated to the surgical section of the congress of German naturalists and surgeons, by Dr. Wolff of Cologne. In the first place, it has been found that the results of extension are better, if the strips of plaster employed are made longer, so as to reach *above* the seat of fracture. In all fractures below the knee, they are applied as far up as the middle of the thigh; in fractures of the femur, they are applied as high as the trochanters. In addition to the usual longitudinal strips, Bardenheuer insists on the use of additional strips of plaster, applied transversely to the long axis of the limb, and encircling the longitudinal ones. He further maintains that the weight employed for extension is usually insufficient. The weights recommended are—30 lb. for fractures of the femur, 15 to 20 lb. for fracture of the leg, and 15 lb. for fractures of the malleoli. In the latter, Bardenheuer objects to the use of a stirrup, and is in the habit of applying the strips of plaster over the malleolar projections; it is then much easier to retain the foot in good position.

The sooner the treatment by weight-extension is instituted after the receipt of the fracture, the better are the results obtained.

During the last five years, 1837 fractures have been treated at the City Hospital of Cologne. There has not been one single case of non-union; when extension has been properly employed, the amount of callus formed is less, and passive movements of the joints are carried out more easily. Statistics obtained from the books of the various trade societies show that out of 106 fractures of the malleoli, 75 per cent. were able to return to work within thirteen weeks; and out of sixty-one fractures of the leg, 60 per cent. were fit for work within a similar period. —*Centralbl. f. Chir.*, Leipzig, Nov. 19, 1898, No. 46.

POSTERIOR GASTRO-ENTEROSTOMY WITH JEJUNO-JEJUNOSTOMY.

M. **GALLET** of Brussels recorded three cases of the above at the recent French Congress of Surgery. The operation is indicated in pyloric stenoses, in which pylorectomy is impossible. M. Gallet insists on the selection of a method of operation which will be successful in preventing the regurgitation of bile into the stomach, and of food into the duodenum. His procedure consists in effecting a junction between the stomach and intestine by means of sutures, and between the two loops of jejunum by means of a small Murphy button. The portions of stomach and jejunum which it is intended to join, having been brought alongside each other, are sutured together by their serous coats, over an extent of from 4 to 5 cms. The jejunum is then incised parallel with the line of suture. Through the wound thus made one-half of the button is introduced, by means of a long pair of forceps, into each segment of the jejunal loop, and the two halves of the button are then locked together, at a distance of about 10 cms. from the stomach. The gastro-jejunal junction is then completed by three layers of catgut sutures in the usual way. The proximal loop of jejunum, intervening between the two junctions, is then divided between two ligatures, and each end is invaginated within itself. Exclusive of the time spent on exploring the pylorus, the operation only lasts half an hour. In two of the cases recovery was rapid and uneventful, the button was passed on the tenth day. In the third case the patient died of exhaustion on the twelfth day.

THE USE OF MURPHY'S BUTTON IN CZERNY'S CLINIQUE.

M. **JORDAN** of Heidelberg reported to the French Surgical Congress that in Czerny's clinique, resource was had to Murphy's button on every possible occasion. He estimated that it had shortened the operation of gastro-enterostomy to fifteen minutes instead of three-quarters of an hour, when sutures are employed. In the early days of the button, Czerny had had three deaths from perforation; in recent years he has abandoned the button when dealing with the large intestine; in other cases he has employed the button on more than one hundred occasions without a single disaster. He insists on the importance of locking the two halves of the button very firmly together. Supplementary sutures are usually unnecessary. The button may be passed as early as the seventh or eighth day; more often at the end of three weeks.—*Gaz. d. hôp.*, Paris, 29th Oct. 1898.

NEPHROTOMY, NEPHROSTOMY, AND NEPHRECTOMY.

MM. Guyon and Albarran presented an important article on this subject to the recent Congress of French Surgeons. They date the history of the modern operations from the achievements of Durham and Bryant in 1870. They propose to restrict the term nephrotomy to an operation in which the kidney is not only exposed but incised; and the term nephrostomy to an operation in which the pelvis of the kidney is opened through the renal substance, and in which the wound is intentionally kept open.

I. NEPHROTOMY is indicated—(1) For the exploration of the kidney, pelvis, or ureter; (2) for the removal of calculi; and (3) as a preliminary procedure in certain operations on the pelvis and ureter.

1. *In exploratory nephrotomy*, it is not sufficient merely to expose the kidney and to inspect and palpate its exterior; it is necessary to

incise its substance. The pedicle is compressed by an assistant, while the kidney tissue is opened by the same incision as is employed in post mortem examination; the incision should be sufficiently free to open the superior and inferior calyces of the pelvis, and to expose the maximum of renal tissue, otherwise the presence of tubercle, calculus, tumour, nephritis, etc., may be overlooked.

2. *In nephro-lithotomy*, there is the choice of reaching a calculus in the pelvis by incising the wall of the latter, or of gaining access to its cavity through the renal substance. The latter has the disadvantage of damaging the kidney tissue, which is already the seat of an interstitial inflammation, which always accompanies stone; the former, on the other hand, is very much more apt to be followed by urinary fistula, and the facilities for exploring the calyces are by no means so satisfactory; further, the mechanical difficulties of removing a branched calculus, through an incision into the pelvis alone, are very much greater. Pyelotomy, as it may be called, is therefore to be restricted to cases in which a medium-sized calculus has been felt in the pelvis by the fingers, and in which its shape will permit of its being easily removed.

II. NEPHROSTOMY may be defined as the formation of a renal fistula, with a definite object in view. The lumbar route is recommended. The kidney is exposed and incised freely, as described in nephrotomy. Each half of the divided kidney is then secured by stitches to the edges of the parietal wound.

The object of the operation is to evacuate a renal retention or hydronephrosis (aseptic or septic), or to re-establish the renal functions in cases of anuria; it may be regarded as distinctly conservative in its tendencies.

1. For *anuria*, the operation was first performed by Clement Lucas in June 1895. In anuria, dependent upon calculous obstruction, the authors recommend nephrostomy as preferable to any operative procedure directed to the ureter which may be obstructed, or to the pelvis in which the calculus may have lodged. It is a simpler, more certain, and more rapid operation; the freely opened and exposed renal tissue recovers its secreting power more rapidly and with greater certainty, and if the calculous obstruction be got rid of, either then or later, the wound in the kidney heals in the most satisfactory way. In all serious cases of calculous anuria, it is the operation to be preferred.

2. In *hydronephrosis*, the same operation is to be preferred to nephrectomy, in every case in which there is any prospect of getting rid of the obstruction to the outflow of urine, whether by catheterising the ureter, by nephrorrhaphy in floating kidney, or by procedures intended to improve the outflow of urine from the pelvis into the ureter (uretero-pyelostomy).

3. In *septic pyonephrosis*, the evacuation and drainage of any pocket or pockets containing pus is simply carrying out the ordinary principles of surgery. The results of nephrostomy in these cases are most gratifying, even in the most severe, and when recovery may appear hopeless. Some surgeons are inclined to prefer nephrectomy in such cases, rather than the simpler and more conservative operation of nephrostomy, on the ground that the latter may result in a troublesome fistula. The authors point out, that the fistula may cure spontaneously, especially if

the normal outflow of the urine be re-established; and even if it persists, it is not incompatible with good health and an active life; and, finally, if at a later period the secretion of the affected kidney be shown to have no physiological value, it may be excised under conditions more favourable for its success. Primary nephrectomy is therefore very rarely called for in septic pyonephrosis, and it is not to be undertaken unless one is assured of the functional value of the other kidney. This assurance is rarely obtainable, for, in the type of patient under consideration, the only certain method, namely, catheterisation of the ureter, is not to be practised, on account of the septic condition of the bladder.

4. In *tubercular pyonephrosis* the conditions are very different from the preceding. Nephrostomy may not give relief at all, or only for a limited time. A fistula always persists, and no patient has been known to survive the operation longer than two years. In tubercular pyelonephritis, a primary nephrectomy is the procedure of choice, provided the condition of the other kidney is compatible with physiological requirements. The authors have performed the radical operation seventeen times, with sixteen recoveries. Their results, joined to those of Czerny, Bardenheuer, Kustner, Israël, Tuffier, and Routier, represent a total of seventy-six primary nephrectomies for tuberculosis, with eight deaths, equal to a mortality of 10 per cent. In eighty-four nephrostomies for tubercle, the mortality amounted to 17 per cent. The latter operation is therefore only to be had recourse to when the state of the other kidney is unknown, when there are serious tubercular lesions elsewhere in the body, and when the local conditions render the risks of a nephrectomy too great. When performed under these conditions, it may give great relief as a palliative measure, or it may be followed by the more radical operation under more favourable auspices.—*Presse méd.*, Paris, 19th October 1898.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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Lysol Poisoning from Uterine Douching.

Cramer (*Centralbl. f. Gynäk.*, Leipzig, 1898, No. 39) reports a case of fatal poisoning from the use of lysol as an intra-uterine douche, observed in the clinique of Professor Fritsch of Bonn.

The patient, a 2-para, æt. 22, was admitted in labour on *3rd July* 1898.—The case was premature (seventh month), and presented by the breech. She was delivered the same afternoon,—the placenta, which was healthy, being expelled ten minutes later. The child lived only two hours. The patient received a vaginal douche of 4 litres of a 1 per cent. solution of lysol, and her temperature rose shortly afterwards to 102°·2; pulse, 120. The uterus was then irrigated with a 1 per cent. solution of lysol. When 1½ litres had been injected, the patient suddenly became restless, had slightly stertorous breathing, and became comatose, with a weak and rapid pulse. Irrigation was at once stopped, and in three minutes the patient regained consciousness, but complained of giddiness. At 10 P.M., temperature, 98°·6; pulse, 108.

4th July.—Temperature, 98°; pulse, 102. Patient jaundiced and weak; urine dark brown, and contained disintegrated blood corpuscles. On filtration a dark brown substance was deposited, and the filtrate was of a clear green yellow colour, and contained albumin, hæmoglobin, and bile pigment. The presence of phenol was also demonstrated. On the two following days the patient's condition improved, the jaundice was less marked, and the urine returned to the normal colour.

7th July.—Temperature, 102°; pulse, 90. The uterus was irrigated with a 1 per cent. boric solution.

8th July.—Temperature, 96°; pulse, 78. Patient much weaker, and complained of epigastric tenderness. Next day had frequent attacks of hæmatemesis; jaundice absent; urine of normal colour, but containing albumin and blood casts. Temperature, 98°; pulse, 90.

9th and *10th July.*—Hæmatemesis persisting; received another vaginal douche of boric lotion. On the 11th the patient's condition grew worse, there was frequent vomiting; stertorous breathing with Cheyne-Stokes respiration, and on the 13th she died comatose. On post mortem examination, it was found that there was hyperæmia of the dura mater, while the central ganglia, pons, and medulla were congested. The liver extended 1½ in. below the ribs. The kidneys showed the appearances of acute nephritis, both to the naked eye and microscopically. Some opaque fluid was found in the pelvis; the right Fallopian tube was fixed to the pelvic wall; the right broad ligament contained a softened thrombus in its centre, and there was also thrombosis of the spermatic vein on the same side. There was also some grey deposit on the posterior surface of the uterus, corresponding to the placental site. The pathological diagnosis, therefore, was puerperal endometritis, parametritis, and endophlebitis, with, in addition, acute nephritis.

All the cases of lysol poisoning on record have either resulted from the application of the pure drug to the skin, or from the accidental swallowing of the drug. This is the first recorded case where poisoning has resulted from the use of a dilute solution, although a ½ per cent. solution has frequently been used for washing out the peritoneal cavity, without bad results.

In 1878, Fritsch stated that sudden collapse during intra-uterine injections was due to the fluid entering directly into the circulation and dislodging thrombi. It must also be remembered that, in the case in question, it was proved that the patient had been using ecbolics previous to admission, for criminal purposes, which may have produced kidney irritation, which was aggravated by the use of the lysol.

AN EMBOLISM FOLLOWING PLACENTA PRÆVIA.

ZORN (*München. med. Wchnschr.*, 1898, No. 18) reports the case of a woman, a multipara, who was the subject of placenta prævia, at or near full term. Podalic version was performed after perforation of the placenta. The operation was performed without much loss of blood, and the patient rallied well, but symptoms of collapse appeared three hours after delivery. The patient improved after the administration of stimulants, but the collapse reappeared twice at short intervals, and she died four hours post-partum. The post mortem examination revealed that the genital tract was uninjured, but the uterine vessels and the right side of the heart contained considerable quantities of air, while there was no other pathological manifestation present. The author alludes to the rarity of such cases, as he has found only five recorded in the literature of the subject.

Transmission of Tuberculosis by means of the Placenta.

Auche and Chambrelent (*München. med. Wchnschr.*, 1898, No. 19) report the case of a woman who suffered from extensive tuberculosis, and who died three days after confinement at the seventh month. On post mortem examination, the placenta showed numerous tubercular nodules, in which the presence of tubercle bacilli was demonstrated. Rabbits and guinea-pigs were inoculated from these nodules, and developed general tuberculosis. The child lived twenty-six days, and at the sectio tubercular nodules were found in the lungs, liver, spleen, and endocardium. Experiments made upon animals from these nodules also produced general tuberculosis.

Remarks on the Treatment of Tuberculosis of the Uterus and Fallopian Tubes.

W. W. Russell (*Ann. Surg.*, New York, Oct. 1898) discusses the above from the standpoint of the operator. He divides tubercular salpingitis into two forms, according to the situation of the disease. (1) The form in which the disease is found on the peritoneal covering of the tube, in association with primary tuberculosis of the peritoneum. (2) Where the disease starts in the tube and the peritoneum is exempt, or only slightly affected. It has not been the custom to remove the tubes or uterus in the first form. The disease should be considered malignant, and all the affected tissues removed. The author holds that, before operation, care should be taken to ascertain that the disease is localised, and that the operation should be performed by the abdominal route.

At the present time the disease is frequently recognised only after operation, but, in the future, experience may lead to an earlier diagnosis.

In cases where doubt exists, the author recommends that both the uterus and the tubes should be extirpated.

THERAPEUTICS.

UNDER THE CHARGE OF

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Case of Poisoning by Camphor.

Berkholz (*St. Petersb. med. Wchnschr.*, 1897, No. 51) reports a case of camphor poisoning in a young married woman, who had taken 225 grs.

rubbed up with water, with the view of preventing conception. It was taken at 9 P.M., and there were no symptoms till eleven o'clock, when she complained of severe headache, followed almost immediately by a severe convulsion and vomiting. At 12.30 she was in deep coma, with pin-point pupils, pulse full and strong, 120 per minute, and rapid respiration. The stomach was washed out at once, when the patient became violent and delirious, crying out, and requiring to be held. The pupils became dilated, but she was only partially conscious. At 12.45 she was given 45 grs. chloral hydrate along with an equal quantity potassium bromide. There followed marked improvement, with some return of consciousness, and she complained of cold, malaise, and pain in the stomach; 30 grs. of chloral and bromide were given, and she became quieter, but did not sleep. In three days she had completely recovered.

CARBOLIC ACID GANGRENE.

STEINMETZ (*Thérap. Monatsh.*, Berlin, Sept. 1898) records a case of gangrene in which a dressing of 8 per cent. carbolic acid was applied. A boy, æt. 7, injured the fore and middle fingers of his left hand, and the mother applied a dressing moistened in an 8 per cent. solution of carbolic acid. On the first morning both fingers were white, and on the second the tip of the middle finger was black, the cloth having fallen off the other during the night. There was no pain, but this was probably due to the anæsthetic action of the application on the sensory nerves. Seven days later, when brought to Steinmetz, the distal half of the middle finger was black and leathery and dry. A line of demarcation formed on the sixteenth day, and part of the finger was amputated.

Dilute solutions of carbolic acid cause always dry gangrene, owing to thrombi forming in the local blood vessels. Even a 3 per cent. solution has been known to cause gangrene in healthy subjects, and in two cases a 2 per cent. and 1 per cent. solution respectively.

In the same Journal (October 1898), Havemann records a similar case of gangrene of the thumb in a boy, where apparently a 3 per cent. solution had been used to dress a slight wound. The skin was dry, like parchment, and cuts made into it did not cause bleeding, although a small quantity of dark, thick blood was usually exuded. A line of demarcation formed, but the parents would not allow amputation, and the distal end was allowed to separate gradually. It left a good stump.

TREATMENT OF TETANUS.

PATTESON has treated a case unsuccessfully with antitetanic serum (*Dublin. Journ. Med. Sc.*, November 1898), and publishes it with the view of adding to the clinical material at our disposal for gauging the value of the method. A boy, æt. 16, was seen suffering from acute tetanus, after having two days previously wounded his foot with a nail. Within twenty-four hours he suffered from rigors and pain in the neck. The wound was touched with carbolic acid and poulticed with boric acid, and 10 c.c. antitetanic serum given, with full doses of chloral hydrate and potassium bromide. The dose of serum was repeated every four hours, and the whole wounded area excised and cauterised. In spite of all treatment, he died on the fifth day after the infliction of the wound, apparently from cardiac failure. Tetanus bacilli were found in the excised portion. This case confirms the now generally accepted opinion, that the antitetanic serum is only of value in cases where the onset of symptoms has been delayed, or as a prophylactic measure previous to operations.

Metelli (*Suppl. al Policlin.*, Roma, 1898, No. 50) also reports a case of tetanus following on injury to the great toe and treated by Baccelli's method—the hypodermic injection of phenol. It occurred in a man, æt. 30, with trismus, slight opisthotonos, with spasm of respiratory muscles, and great anxiety; temperature, 38° C.; pulse, 110. He was ordered hot baths, quinine by the mouth, and chloral hydrate by the rectum. At first there was improvement, but on the next day the symptoms became more severe. On the third day three hypodermic injections (a syringeful each time) of a 4 per cent. solution of phenol were given, and continued on the fourth and fifth days. On the sixth day the condition was grave, and five injections of a 5 per cent. solution were given. From this time on, the improvement, although slow, was continuous for fifteen days, and the patient ultimately recovered.

PATHOLOGY AND BACTERIOLOGY.

UNDER THE CHARGE OF

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ON HYPERLEUCOCYTOSIS AND LEUCOCYTOSIS.

DR. A. MOTTA COCO has made an experimental investigation into this subject (*Riforma med.*, October 1898, and *Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, 12th October 1898), by injecting the diplococcus into dogs, rabbits, and guinea-pigs. He usually succeeded in producing the general infection. He numbered the leucocytes by the Thoma-Zeiss method. He found that—(1) An attenuated diplococcus culture injected into guinea-pigs was followed by a clear leucocytosis, but the increasing curve of leucocytes did not correspond to the number of diplococci in the blood, which was greatest when the hyperleucocytosis was very slight. The leucocytes were chiefly large, uninucleated elements, with a small amount of homogeneous protoplasm and a large nucleus. There was a considerable number of multinucleated leucocytes, of lymphocytes, of large uninucleated neutrophiles, and some cells with the characters of marrow cells. (2) At a later period a hyperleucocytosis was observed, the cells showing mostly an alteration of the nuclei, and a destruction of the protoplasmic granules in the mononucleated cells of the medullary type. (3) Inoculations of virulent cultures, if they follow upon others of medium strength, produce considerable leucocytosis, upon which no hyperleucocytosis follows, whereas, if strong cultures be used from the beginning, no leucocyte reaction occurs. (4) Inoculations, however, of virulent cultures upon animals which have the disease, or which have become accustomed to the virus, are followed by a considerable hyperleucocytosis, the leucocytes being chiefly uninucleated, of the medullary type and multinucleated. (5) The number of diplococci in the blood, and their virulence, stands in inverse ratio to the num ber and alterations of the leucocytes. It would thus appear that the leucocytes possess bactericidal properties, which become free after their destruction. (6) In the cases of well-marked leucocytosis he observed a rich production of marrow cells in the bone marrow, similar to those in the blood.

How to Obtain a Pure Culture of Amœba.

Tsujitani (*Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, 18th November 1898) has succeeded in growing the amœba, after working at the problem for several years. He used three different kinds of amœba in his experiments—(1) *Amœba lobosa*, which he obtained from a hay infusion. It is about 14 to 26 μ long and 5 to 14 μ broad, and has one to six vacuoles. The protoplasm is granular, and the nucleus is excentrically placed. It will withstand 20 per cent. sodium carbonate solution for 238 hours, and 1 per cent. salt solution for 116 hours. (2) A form which springs from dust. It is about 17 to 34 μ long, and 9 to 17 μ broad. It has one or two vacuoles. The protoplasm is granular, but the nucleus when present is not excentric. It possesses many sharp processes, like *Amœba spinosa*. It is capable of resisting 20 per cent. sodium carbonate solution and 1 per cent. salt solution for 170 hours or more. (3) The third kind is an *Amœba lobosa*, got from the ground. It is about 14 to 34 μ long, and 7 to 10 μ broad. It has an excentric nucleus, and one or more vacuoles. It withstands acids and alkalies as well as the above-mentioned forms. Those amœbæ will develop rapidly in certain nutrient media at the room temperature, but still better at incubator temperatures; but bacteria grow therein as well, and it is impossible to isolate the amœbæ directly by any physical or chemical means. He therefore sought for a bacterium whose powers of resistance are weaker than those of the amœbæ, and found it in the cholera bacillus, which was devoured by them. Although living bacilli form an excellent food for the amœbæ, they can also live, though not so well, upon dead ones. He obtained a bacillus from hay infusion, of which he gives particulars. He made a culture of this upon gelatin or agar and then killed the bacilli by heat. He then inoculated the amœbæ. The second kind above mentioned showed the most luxuriant growth. Good preparations of the pure cultures can be obtained by placing a drop of distilled water upon a clear cover-glass and adding a small quantity of the amœba culture. With the platinum loop a very little of a concentrated solution of hydrochlorate of quinine is added, and the whole spread out over the cover-glass in a thin layer. It is then allowed to dry in the air. It is next fixed in alcohol and ether solution, and dried, and then stained with methyl blue.

The Plague Toxin.

Gottleib Markl has produced a long paper on this subject in the *Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, 18th and 24th November

1898. He obtained a highly virulent culture of the plague bacillus from Bombay. Its virulence was exalted by passing it through the bodies of guinea-pigs, and by several culture generations upon agar. Experiments were made upon guinea-pigs, white mice, and cats. His results may be summed up as follows—(1) The bacilli contain a poison which is very sensitive to heat, and very gradually soluble in water. Fresh forty-eight-hours' old agar cultures can be sterilised by a heat of 100° C. for half an hour, or 65° C. for one hour, or 64° C. for twenty-four hours. (2) A few weeks' old bouillon culture, kept at the temperature of the room, possesses a high degree of virulence for small animals, especially mice, and it appears that this virulence is possessed by not only the bacteria themselves, but by the excreted products of metabolism. (3) The virulent action is the same whether peritoneal or subcutaneous injection be used. Mice die from sufficient doses, in from six to twenty-four hours up to five days, and in the protracted cases show distinct splenic enlargement. In those acute cases, however, no characteristic microscopic lesions have been found. (4) Mice can be rendered immune by gradually increasing doses, but this immunity, even in its highest degree, does not withstand subcutaneous injection with virulent bacilli. (5) The investigations upon a cat further showed that the action of the toxin increased the excreta, so long as immunity was not reached, and that the increased excreta were not accompanied by any rise of body temperature or local skin inflammation, such as necrosis, or the shedding of hairs. (6) The blood serum of the cat which had reached a considerable degree of immunity showed antitoxic properties towards the plague toxin, but not to the living bacillus. (7) The toxin can be extracted from the bacilli of a bouillon culture by absolute alcohol. It was found, however, not to be pure, and all attempts to obtain it in a pure state failed, alike on account of its sensitiveness towards reagents and its mingling with albumins. (8) The experiments showed that a serum with healing properties could be obtained from animals, but it could not yet be shown to be antitoxic, because of the imperfections in its method of separation. It was probable, however, that we might yet obtain such an antitoxic serum by immunising animals with the toxin and the bacilli themselves.

THE STAINING OF FLAGELLA.

IT is generally recognised that Löffler's original method is still one of the best and most generally useful. Van Ermengem's silver method has comparatively recently sprung into favour, but it is rather complicated and not very easily worked by the beginner. Pitfield's method is not so well known and is a little uncertain in its results. An excellent modification of this has just been devised by Mr. Richard Muir (*Journ. Path. and Bacteriol.*, Edin. and London, October 1898, p. 374). It will be found to give good results even in the hands of beginners. They may find at first that the flagella come out rather too faint, but a little practice will enable them to obtain clear, distinct, and beautiful results. Muir advises that all cover-glasses should be cleaned thoroughly by Van Ermengem's method. They are left for twenty-four hours in the following solution:—Concentrated sulphuric acid, 6 c.c.;

potassium bichromate, 6 grms.; water, 100 c.c. (The writer has found that heating, even boiling this solution, gives a more perfect result.) They should then be thoroughly washed in running water till all trace of the bichromate solution has gone, transferred to a clean bottle, and kept in absolute alcohol. When required, they are taken from the alcohol and dried by wiping with a clean duster. Prepare the following solutions—(*a*) *The mordant.*—Tannic acid, 10 per cent. watery solution (filter), 10 c.c.; corrosive sublimate, saturated watery solution, 5 c.c.; alum, saturated watery solution, 5 c.c.; Ziehl-Neelsen's carbol-fuchsin, 5 c.c.; mix thoroughly, and put in test tubes, which are then centrifugalised, or the mixture may simply be allowed to stand over night. The clear supernatant fluid should be removed with a pipette and transferred to a clean bottle. This mordant will keep good for one or two weeks. (*b*) *The stain.*—Alum, saturated watery solution (filter), 10 c.c.; gentian-violet, saturated alcoholic solution, 2 c.c. The stain will keep for two or three days. This stain can be used in the mordant in the place of the carbol-fuchsin. The film is now prepared by placing a small drop of distilled water upon a cover-glass, and adding to it a very minute portion of an agar culture, say of a twenty-four-hours' old typhoid bacillus; it is spread out into as thin a layer as possible, and allowed to dry in the air. Pour on as much of the mordant as the cover-glass will hold, and heat gently over a flame till the steam rises; allow to steam for about one minute, and wash well in running water for two minutes. Dry over flame, and when properly dry pour on as much of the stain as the cover-glass will hold, and steam as before for one minute, wash well for two minutes, dry and mount in Canada balsam. If black-stained preparations are preferred, heat with Gram's iodine fixing solution for one minute before drying and mounting. He got beautiful results by this method with tetanus, malignant œdema, typhoid, coli, pyocyaneus, and subtilis, and also spirilla, *e.g.* cholera, Finkler Prior, Metchnikoff, etc.

DERMATOLOGY.

UNDER THE CHARGE OF

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PSEUDO-LUPUS VULGARIS, CAUSED BY A BLASTOMYCES.

THE chief forms of serpiginous ulceration of the skin have been commonly regarded as either syphilitic or tubercular, but apparently to these must now be added one associated with the growth of a fungus belonging to the yeast family. Gilchrist and Stokes (*Journ. Exper. Med.*, N. Y., 1898, vol. iii. No. 1) relate an instance. A man, æt. 33, in whose immediate connections there was no tuberculous or syphilitic history, nor who had suffered from these himself, had in 1885 a pimple behind the ear which soon became pustular. This spread slowly in a linear direction, and in time advanced over the greater part of the face. In 1897 there was a progressive border, behind and within which was a continuous atrophic scar, whiter and thinner than normal skin. The border itself consisted of a comparatively narrow, inflammatory, red, superficial ulceration, covered with a scanty scab. In some situations, as on the eyebrow, it showed a firm, papillomatous appearance. These papillomatous growths could be separated, and sometimes a minute quantity of pus could be squeezed from between them. Other and similar lesions occurred on the back of the hand, the scrotum, the front of the thigh, and the back of the neck. Most of these latter healed spontaneously after a variable time, as has now that on the face all but entirely. There was no pain throughout, and the glands in the neighbourhood were not enlarged, while the general health was good. The condition bore a close resemblance to lupus vulgaris—in particular to lupus papillomatosus—and there were points of similarity with tuberculosis verrucosa cutis. Sections showed hypertrophy of the epidermis, with miliary abscesses. These were also present in the corium, which contained a good deal of inflammatory material distributed through its texture, and there were evidences of the formation of tubercle-like nodules. In all the abscesses, and among the granulation cells of the corium, as in the pseudo-tubercles, were numbers of doubly contoured, refractive, round and ovoid bodies, from 10 to 20 μ in diameter. Many of these showed buds, some vacuoles. They occurred singly, in pairs, or less often in groups. The parasites were usually free, but a few were within giant cells. Pure cultures were obtained from pus squeezed from between the papillomata. The organisms grew in all ordinary media, and especially well on potato and beer-wort agar. The cultures showed both budding forms and mycelium. Inoculated into animals, the same forms were reproduced, the most striking results being nodules in the lungs, inflammatory in character, and containing numerous parasites. Since the organism did not ferment sugar, it may belong to the blastomycetes or oidia. The authors term the disease Blastomycetic Dermatitis, and recommend careful search for these organisms in tuberculous lesions of the skin. They can be readily demonstrated as doubly contoured refractive bodies, if the unstained sections are soaked in liquor potassæ. The existence of true tuberculosis was carefully excluded.

VERRUGA PERUVIANA.

ODRIOZOLA of Lima has made a study of this little known disease (*Presse méd.*, Paris, 27th July 1898). It is an endemic, inoculable complaint, restricted to certain deep defiles of districts round Lima. The natives either do not suffer, or more often are protected by having passed through an attack. Strangers, however, who live for a time in the contaminated area, or even who may spend but a few hours there, are liable to be affected. The germ appears to be introduced either by water, food, or through the medium of the atmosphere. Many domestic animals suffer, but it attains its greatest proportions in the Solidungula. A severe epidemic broke out in 1871 while the Oroya Railway was being constructed. In 1885 a student named Carrion inoculated himself with blood from a verruga, and died of the pyrexia supervening thirty-nine days after. The symptomless incubation lasts fifteen to forty days. The invasion occupies several days, and is characterised by headache, lassitude, articular and muscular soreness, particularly in the lower limbs. Fever of a grave type manifests itself eight to fifteen days subsequently, with rigors and intolerable pains. It presents a remittent or intermittent type, accompanied by acute anæmia, sometimes complicated with multiple hæmorrhages, vertigo, and syncope. Various glands are enlarged, and death may occur by coma. In other cases, with the abatement of fever the verrugas develop. These cannot be regarded as the terminal stage of the fever, but rather as one of the symptoms. If in some instances the eruption seems the first indication, careful study reveals precursory phenomena, such as weakness, inexplicable anæmia lasting days or weeks, vague aching in and œdema of the legs, with evening rise of temperature. Whatever may be the early manifestations, fever always precedes the eruption, its type notably differing according as this consists of miliary or nodular verrugas. The miliary, the more superficial, may commence by a petechia, on which arises a red, shining, pointed papule; or what look like sudamina form on the forehead or limbs, becoming umbilicated, as does the variolous vesico-papule; or the elevation is of a dirty white, resembling a small papilloma. When fully developed, the individual miliary verrugas are from the size of a pinhead to a pea, sessile or pedunculated, of a wine-red colour. These, if small, exfoliate and bleed; if larger, ulcerate, exuding a transparent yellow fluid, which concretes into brown crusts on their surface. They finally dry up and disappear, leaving no trace. They commonly come out first on the lower limbs, thence ascending symmetrically over the body, but having preferential localisations. Itching, occasionally pain, attends them. They may appear on mucous surfaces, particularly on the conjunctiva, and are found post-mortem in the interior of the body also. The nodular verrugas are voluminous tumours, starting either in the derma or hypoderma, and growing with great rapidity. They occur principally on the face and limbs. The skin over them is violaceous and shining, and tender to touch. Their size ranges from a nut to a small apple. They are sessile or stalked, and the neighbouring skin is pigmented and fissured. Ulceration sets in, sometimes accompanied by alarming hæmorrhage. Eventually the tumour mummifies and separates, the wound healing quickly. Microscopically, the appearances are those of a granuloma, without giant cells or caseation, thus excluding lesions of a tuberculous, syphilitic, or leprous order. A bacillus, in some respects like that of Koch, is found in the interstitial spaces, rarely in the cells. Odriozola has discovered a small bacillus in the blood during the febrile stage, which he succeeded in cultivating, but inoculations into a dog were inconclusive.

PATHOGENESIS AND PREVENTION OF THE SERUM EXANTHEMATA OBSERVED IN THE ANTITOXIN TREATMENT OF DIPHTHERIA.

AMONG the most interesting of the epiphenomena which occasionally appear in cases of diphtheria so treated, is a cutaneous lesion, the so-called "antitoxin rash." Berg (*Med. Rec.*, N. Y., 18th June 1898) points out that this has no constant appearance, fixed duration, or regular distribution. At one time it resembles scarlet fever, at another urticaria, or erythema, or measles. As regards its frequency, Hartung's statistics give 11·4 per cent. of a large number of injections; those of the Imperial Board of Germany for 1895, 8·1; while at the Willard Parker Hospital, 24 per cent. of the cases during a period of four months, developed eruptions. Their wide prevalence would seem to indicate their dependence on causes inherent in the serum itself, or in some way related to its mode of manufacture or application. The serum derived from one horse is more prone to occasion such effects than that from another, and multiple injections than single ones. It is but rare that the advent of the eruption is contemporary with a decided increase of gravity of the disease itself. In many instances a sharp rise of temperature precedes and ushers in the appearance of the eruption, to decline when it has reached its full development. Desquamation occasionally occurs as a sequence. The morbiliform type is more easily distinguished from measles than is the scarlatiniform from scarlet fever. In inquiring into the pathogenesis, it has long been known that when the blood serum of an animal of one species is injected into the blood of another, it exerts a toxic action, and Rumno has found that this is markedly lessened by passing the serum through the Chamberland filter. Pure blood serum of the horse produces eruptions in a large percentage of cases, but the antitoxin present in the diphtheria antitoxin does not add to the frequency with which these eruptions result. Except those eruptions which manifest themselves at the site of or near the part injected, which are due to direct local irritation, all the others are either the secondary result of the irritation of the vasomotor centres, or are produced in the process of excretion of the serum from the body by the capillaries of the skin and sweat glands. In our efforts to diminish the frequency of these rashes it is important to give the necessary quantity of antitoxin in the smallest amount of serum. The serum of a horse found by experience to be least irritating should if possible be selected. Filtration is advisable, since this in no way diminishes the strength. A fine filter must be used to be effectual, but the process is slow and difficult. It might be expedited by substituting several for one porcelain candle opening into the tube.

NOTES ON LEPROSY.

EHLERS (*Dermat. Ztschr.*, Berlin, February 1898), in course of an excursion in 1897 through the Balkan Peninsula, for the purpose of ascertaining the proportion of lepers there, had the opportunity when in Greece of seeing many of the Cretan refugees, and found numerous cases among them. Though Greece itself is not free from the scourge, still he is of opinion that in this way fresh foci for its spread will have been established. Another interesting observation was the discovery of leprous patients presenting all the features of ainhum. Silva Lima and Wucherer in Bahia, in 1867, described this as exclusively confined to African negroes. It consists in the formation of a constricting furrow at the root of the little toe of one foot, terminating with the separation of the toe by a process resembling strangulation. They regarded it as a purely local disease. It has, however, been encountered in all regions, except in Europe hitherto. Zambaco Pasha holds it to be but a modified form of leprosy. Many cases have now been met with, and Ehlers saw three examples in Iceland, and one at least in Constantinople. Bergmann (*ibid.*), in Riga, states the case respecting the steady increase of leprosy there very clearly. Fifteen years ago that city was considered as free from leprosy, though it was plentiful enough there during the Middle Ages. Between 1870–80 nine cases were recorded, in 1881–86 there were twenty-seven new cases reported, in 1887–90 no fewer than sixty-four. He cites a series of instances in which the exact source whence contagion arose could be determined, but the mode of communication still eludes detection. Broes van Dort (*ibid.*, May 1898) argues from the fact that the proportion of males who are attacked by leprosy is much larger than of females, that this tells for contagion as opposed to heredity. A large majority of leprous men belong to the working classes, are especially agricultural labourers or fishermen. Such occupations favour the occurrence of fissures or wounds of the skin, liable to form channels for the reception of contagion. Women, on the contrary, are less exposed, and take more care of their skin. Again, in inherited syphilis there is no marked disproportion between the sexes, which also goes against heredity in leprosy. In discussing the cause and significance of the eruption in leprosy, Glück (*ibid.*, July 1898) says that we must regard the cutaneous manifestations, not as integral parts of the leprous process, but as complications the result of secondary infections, which give rise to fresh foci and aggravate the disease. These eruptions are accompanied by fever, and the practical outcome is that all such methods of treatment which may lead to pyrexia are to be avoided, such as injections with cultures from the organisms of erysipelas, or of its toxins, or serum-therapy. To speak of a serum-therapy till we can cultivate the bacillus seems an anomaly, but Carrasquilla reported cases where a marked improvement, though not a cure, ensued from the injection of the serum of horses which had in their turn been inoculated with blood serum from lepers. This was followed by reaction, rise of temperature, and subsequent subsidence of the morbid phenomena. Dehio, however (*St. Petersb. med. Wchnschr.*, 4th and 11th July 1898), has repeated his experiments with wholly negative results. In carrying these out the greatest care appears to have been taken to ensure a serum free from germs. He explains the effects obtained by Carrasquilla and others as probably due to such bodies as proteins, which, as tuberculin did, and as attacks of erysipelas do, occasion febrile reaction and temporary amelioration of symptoms, followed by a return to a condition worse than before. Such experiments have, he remarks, their value. "They preserve us from false illusions, and demonstrate in particular that the serum-therapy of leprosy is a problem to the solution of which we must approach with the greatest precaution, and maintain the most critical attitude."

DISEASES OF CHILDREN.

UNDER THE CHARGE OF
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A New Diagnostic Sign of Measles.

In December 1896, Koplik, in the *Arch. Pediat.*, N. Y., first drew attention to a hitherto unobserved and pathognomonic appearance of the mucous membrane lining the cheeks and the upper and lower lips, whose importance in enabling us to make a positive diagnosis of the invasion of measles cannot be over-estimated. Being of the opinion that this very significant symptom, heralding an attack of measles, had not received from the profession the notice and attention which it deserved, he emphasises its importance, and describes it at greater length in this paper. The eruption or phenomenon which he describes appears on the mucous membrane of the cheeks and lips only (not on the fauces, soft or hard palate) in from twenty-four to seventy-two hours before the eruption on the skin. It spreads and reaches its height just as the skin eruption appears and is spreading, when it commences to fade. On examining the mouth during the invasion of measles, in addition to the usual redness of the fauces, we see on the mucous membrane lining the cheeks and lips a very distinct and pathognomonic eruption. This consists of small irregular spots of a bright red colour, and in the centre of each of those spots in strong daylight we see a minute bluish white speck.

The author maintains that these spots are absolutely pathognomonic of the beginning of measles; that he has never observed them connected with any other condition; and that, when once seen, the occurrence of measles can be unhesitatingly predicted. These bluish white specks are sometimes very delicate, requiring strong window light for their discovery, and sometimes we require to evert the mucous membrane of the cheeks to see them plainly. At times only a dozen or more spots are present, whilst in other cases the whole inside of the cheeks and lips is covered with them. They cannot be mistaken for sprue, being neither so large nor so white as sprue spots, nor do they ever coalesce or assume plaque-like forms, but always retain a punctate appearance. As the skin eruption begins to appear and spread, the spots on the buccal mucous membrane gradually become more diffuse, and finally disappear in the general redness of the mouth. The great value of this new sign of measles lies in the fact that it occurs in the pre-eruptive stage, thus enabling physicians to isolate cases much earlier than has previously been possible, and in many cases to prevent the occurrence of epidemics; and it will prove of special value in hospitals and other institutions, where large numbers of children are housed, and where epidemics of measles are so justly dreaded. This diagnostic sign is also useful in differentiating measles from a number of other exanthemata and skin diseases which in their onset resemble it, amongst which may be mentioned rötheln, scarlatina, erythema multiforme, drug eruptions, and influenza.—*Med. Rec.*, N. Y., 9th April 1898.

ADDISON'S DISEASE IN CHILDREN.

THREE cases of this disease in children occurred in Variot's clinic during the past year, and as the condition is a rare one in young children (only forty-eight cases having been recorded), the following account of the symptoms is interesting:—In nearly every case tuberculosis of the suprarenal capsules was found post mortem. The symptoms were as a rule at first indefinite, the children appearing weak and apathetic, and showing great disinclination for exercise or play; and in many the true diagnosis of the condition was further rendered difficult by the occurrence of tubercular manifestations in the lungs, kidneys, bones, joints, and other organs. In many children the commencement of the illness was heralded by vomiting and diarrhœa, but in the great majority of cases the most important symptom was extreme and gradually advancing asthenia, the child avoiding the least movement, and even refusing to answer questions, dreading the fatigue which was caused by doing so.

Usually there was loss of appetite, accompanied by nausea, and in many cases vomiting of a very severe type. Pains were generally present, situated sometimes in the back, sometimes in the abdomen, and at other times referred to the head or to the bones of the extremities. The discoloration of the skin was generally slight at first, and seldom attracted much notice; but, as the disease advanced, the pigmentation became darker, and was most apparent on the exposed part of the body, face, hands, and neck, and also in those regions where it is normal to find a deposit of pigment, such as the navel, genitals, and axillæ. Brown patches were also frequently to be detected in the mucous membranes.

As a rule there were periods of remission and of renewed activity in the progress of the disease, which usually terminated within a year. Death was generally due to extreme weakness or to some intercurrent disease, and was often preceded by epileptiform convulsions, with rapid pulse and rise of temperature.—*Klin. therap. Wchnschr.*, 18th September 1898.

TYPHOID FEVER IN A CHILD TWO MONTHS OLD.

AT the Gynæcological and Pediatric Congress at Marseilles, M. Cassonte related the following case:—A child, æt. 2 months, was admitted to hospital with somewhat indefinite symptoms, characterised by diarrhœa, collapse, and subnormal temperature, which were attributed to the improper feeding to which the infant had been subjected. Owing to the occurrence of an epidemic of typhoid fever in the district, the serum diagnosis was employed, with the result that at the end of half an hour the characteristic reaction was obtained. The following day the infant died, and at the post mortem examination fourteen Peyer's patches were found infiltrated, several of which had commenced to undergo ulceration. M. Cassonte draws attention to the uncertain nature of the symptoms of typhoid fever in children. In the case specified, the absence of any rise of temperature seemed rather to exclude the diagnosis of typhoid; and of all the usual symptoms of this disease, the only one present was persistent diarrhœa. He considers that many cases of typhoid are not diagnosed in infants, and he believes, if all doubtful cases of diarrhœa were submitted to the serum diagnosis, it would be found to be more frequently present than is generally supposed, for there seems to be no good reason why infants, who are so liable to gastro-intestinal infection, should be less liable to the ravages of the typhoid bacillus than older persons.—*Journ. de clin. et de thérap. inf.*, Paris, 20th October 1898.

DOUBLE ATHETOSIS FROM INFANTILE CEREBRAL PALSY.

AT the New York Neurological Society, a boy, æt. 16, the youngest of several children, was exhibited. All his brothers and sisters had been well at birth, but the patient had been asphyxiated, although the labour was normal. He did not walk till 5 years old, and at 7 years he had scarlet fever. After that the peculiar movements which the boy exhibited became worse. Soon after birth it was noticed that he moved his head in a peculiar manner, the movements being aggravated on voluntary motion, and his face showed a constant succession of grimaces. These peculiar movements affected the trunk as well as the upper and lower extremities, whilst the tongue and eyeballs remained unaffected. The deep reflexes were exaggerated, the mental development was good, whilst the physical condition of the boy was exceedingly poor. The movements were less rapid and more vermicular in character when the child was not undergoing observation. Dr. Stieglitz, who showed the patient, was of opinion that this was a case of double athetosis, due to infantile cerebral palsy, the result of a lesion occurring at the time the child was born in a state of asphyxia. The lesion in these cases is usually to be found in the corpus striatum or in the lenticular nucleus, so that it was not surprising that there was no epilepsy nor disturbance of the mental development. Long-continued peripheral restraint in those obstinate cases often had a remarkably good effect in controlling those unruly movements.—*Med. Rec.*, N. Y., 22nd October 1898.

THE SPINAL LOCALISATIONS OF HEREDITARY SYPHILIS.

IT is a well-known fact that the nervous centres are frequently attacked by hereditary syphilis, and it is to this cause that many of the deaths of newly-born children resulting from convulsions, meningitic symptoms, etc., are to be attributed.

Hereditary syphilis is also responsible for large numbers of the cases of blindness, deafness, idiocy, epilepsy, and hydrocephalus, which we meet with in young children. Recently considerable attention has been directed to the pathology of the spinal cord in these cases, by Gilles de la Tourette, Raymond, and others. Usually the meninges and vessels are implicated in the first instance, the cord being attacked later. On examining the spinal cord, it is seen to be infiltrated with large numbers of embryonic cells, and subsequently either softening, sclerosis, or resolution takes place. The membranes are thickened and adherent, sometimes to the cord, and at other times to the vertebræ, the vessels being altered and contracted, and their walls infiltrated with round cells. The lesions are disseminated throughout the cord, but most frequently predominate in the cervical segment. The symptoms usually develop insidiously, and may conveniently be divided into motor and sensory. The latter symptoms include numbness and tingling pains, sometimes areas of anæsthesia side by side with zones of hyperæsthesia, delay, and errors in localising sensations. The motor symptoms are generally those of chronic myelitis, and include paralysis of all four extremities when the cervical area is the seat of the lesion, spasmodic paraplegia accompanied by sphincter troubles when the dorso-lumbar region of the cord is involved.

The malady may at its origin invade all four extremities at once, or it may attack them progressively—paraplegia at first, followed by sphincter troubles, then paralysis of the upper extremities, and finally bulbar and cerebral phenomena.

The pains are not as a rule very troublesome; the paralysis is spasmodic, and the muscles become contracted and rigid on the least movement, and there is generally persistent constipation and constant desire to urinate.

Remissions are of frequent occurrence in the course of the illness; sometimes improvement, and at other times aggravation of all the symptoms taking place. The termination is variable, and it is by no means uncommon for complete recovery to take place.—*Arch. de méd. des enfants*, October 1898.

REPORTS OF THE MEDICAL SOCIETIES.

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Edinburgh Obstetrical Society. — *Wednesday, 14th December* 1898.—Dr. **Halliday Croom**, President, in the chair.—The **President**, Dr. **Brewis**, Dr. J. W. **Ballantyne**, and Dr. **Haultain** showed specimens.

Communications.—(1) Dr. J. W. **Struthers** read a note of a case of chronic inversion of the uterus, treated by Dr. Berry Hart. The condition had been noticed after a confinement, and before admission it had been mistaken for a case of prolapsus uteri. Attempts to reduce it failed at first, but ultimately Dr. Hart succeeded, by making an incision, per vaginam, into the pouch of Douglas, and inserting two fingers into the depression in the uterus, and so stretching the constricting cervix, while with the fingers of the right hand he pushed up the fundus. (2) Dr. **Haultain** read "A Retrospect of a First Series of 110 Abdominal Sections for Pelvi-Abdominal Disease," which will appear in next issue. (3) Dr. **Haig Ferguson** read a paper on "Repeated Ectopic Gestation" in the same patient, with an account of a case, which will also appear in next number.

Edinburgh Medico-Chirurgical Society.—*7th December* 1898.—Sir J. **Batty Tuke** in the chair.—**Patients.**—Dr. **Rainy** showed a case of muscular dystrophy, probably due to joint irritation.—Mr. **Stiles** showed a patient in whom he had reduced a congenital dislocation of the hip-joint, according to Loring's method, with a perfect result.—Mr. **Cathcart** showed a case of myxœdema, in which considerable improvement had resulted from removal of part of the thyroid gland.

Specimens.—Dr. **Russell** and Mr. **Cotterill** showed the brain from a case in which a lesion between the visual and motor speech centres had been diagnosed. A glioma was found in this situation, but it could not be completely removed.—Mr. **Stiles** showed specimens from a case of multiple fractures, in a syphilitic and tubercular child. A skiagram of this case was also exhibited.—Dr. **Scot Skirving** showed—(1) A large ischial bursa, (2) peculiar contents of the same, (3) ruptured kidney.

Communications.—Dr. **Boyd** read a paper on pulmonary regurgitation, based upon two cases he had had under his care. In the first case the regurgitation was evidently due to ulcerative endocarditis of the pulmonary valve cusps. The temperature was irregular, and other symptoms pointed to septic absorption. Anti-streptococcic serum was used with some improvement, but the patient eventually died. In the other case the regurgitation was due to dilatation of the pulmonary artery, resulting from mitral disease, and death in this case ensued from thrombosis of the veins of the right, by causing gangrene. Most of the cases of organic pulmonary insufficiency occur in young people, and the prognosis, as a rule, is very grave, owing to the rapid breaking down of compensation. As regards the functional form, the author agreed with the views of Gibson and Steell. The prognosis is much better. Dr. Boyd fully discussed diagnosis and treatment.—Dr. **Gulland** read a paper on sulphonal poisoning. In chronic poisoning there are at first gastro-intestinal disturbances, especially constipation. Then comes on ataxia of the extremities, and, in severe cases, convulsions. Hæmatoporphyrinuria is a very marked characteristic, and when this deep red-coloured urine appears the case is a very grave one. In *acute* cases there is deep sleep, lasting sometimes for days, gastro-intestinal disturbances, and nephritis. The red-coloured urine occurs very rarely. The prognosis is good, but in chronic cases, when pronounced, is very bad. Almost all the fatal cases had constipation, and this probably favours accumulation. Chronic cases always show kidney changes. The fluidity of the blood after death favours the idea that sulphonal is a blood poison. The probable cause of death is uræmia, using the term in its widest sense. In treating cases, we should, if the case be an acute one, empty the stomach and give purgatives and enemata. In chronic cases large doses of alkalis may be given to control hæmatoporphyrinuria, the urine being always very acid in those cases.

Glasgow Pathological and Clinical Society.—*14th November* 1898.—Dr. **Meighan** in the chair.—Dr. T. K. **Dalziel** showed a tumour from a case of multiple uterine myomata, in which necrosis from torsion of the pedicle occurred. The patient was admitted with the history that six weeks before, she was seized with sudden acute abdominal pain, followed by collapse and symptoms of peritonitis. A tumour was felt in the right flank, evidently connected with the uterus. Menstruation had been regular; no hæmorrhage between periods. The tumour was adherent to the abdominal wall and to the neighbouring viscera. As the left ovary was cystic, the appendages were removed with the uterus. She made a good recovery. Pathologically the tumour was a fibromyoma. He also showed four cysts, two from the submaxillary region, one of which was a dermoid, one from the parotid region, and one from the thyroid gland.—Dr. **Meighan** showed macroscopic and microscopic specimens from a case of tubercular choroiditis in a child.—Dr. **Carslaw** showed a specimen of detachment of the retina, from a case of chronic nephritis. The patient had been totally blind for some days, and was verging on a uræmic seizure. Examination of both eyes showed bulging of the retina with quivering and neuro-retinitis of the usual form. He died three days after admission, and post mortem examination showed the kidneys to be the seat of interstitial nephritis.—Dr. **Edington** showed a cyst of tubular form removed from the under surface of the penis.

12th December 1898.—Dr. **Knox** in the chair.—Dr. **Napier** showed a case of lupus of thirty years' standing, treated by Koch's tuberculin. Injections were given in gradually increasing amounts until 2 cgrms. were given. All the lupus tissue sloughed. A complete cure was hastened by curettage and cautery. There has been no recurrence for five months.—Dr. **Rutherfurd** showed an infant of six months, on whom abdominal section was done for intussusception. Viewing the five cases which he has had, he is convinced that nothing short of abdominal section in them would have been of any avail. Dr. Rutherfurd also showed a patient wearing a simple form of suprapubic drain.—Dr. Meighan showed—(*a*) A case of orbital tumour, carcinoma; (*b*) a case of buphthalmos; (*c*) a case of microphthalmos.

Royal Medical Society.—*8th November.*—Dr. Herring in the chair.—Dr. Cooper delivered his presidential address, his subject being "Heroic Deeds done in the Indian Mutiny," dwelling especially on the exploits of medical officers in the field.—Mr. C. Heron Watson read a dissertation on "Extempore Methods of Treatment." After discussing various possible definitions of his subject, he finally proposed the following:—Methods devised at the moment from out of the individual's previously acquired knowledge, as suitable for the present circumstances, and not restricted by standing formulæ. He then proceeded to discriminate between extempore treatment from necessity and extempore treatment from choice.—Dr. Struthers described two cases illustrating the difficulty of diagnosis of abdominal tumours. In one case, an enormous carcinoma of the stomach, there were no stomach symptoms and no emaciation. The other, a tumour of small intestine, presented equal difficulties of diagnosis.

4th November.—Dr. M'Nair Scott in the chair.—Dr. Boyd Jamieson described a case of perforating wound of the abdomen in a boy, æt. 9, due to a fall upon railing spikes. Portions of gut protruded through the lacerated abdominal wall.—Dr. Cooper described the physical signs and symptoms of a tumour, which proved to be a hydatid cyst of liver in a woman, æt. 37, from Orkney.—Dr. Graham described two cases of intestinal obstruction.

11th November.—Dr. Cooper in the chair.—Dr. M'Nair Scott delivered his presidential address, and gave an account of the life and work of Ambroise Paré.—Dr. Scott showed that Paré possessed all the qualities characteristic of the pioneer.—He abandoned many barbarous methods of treatment then in vogue, such as the application of boiling oil to gunshot wounds, and the use of the cautery to arrest hæmorrhage. He deserves to be still remembered if only for his introduction of the ligature in hæmorrhage, and of podalic version in obstetrics.—Dr. Andrew Watt showed a case of probable tubercle of the malar bones in a boy, æt. 2½, and a characteristic section of hydrocephalic brain.—Dr. Edwin Matthew exhibited two nervous cases in children. One was of pseudo-hypertrophic paralysis, in atrophic stage. The other was a case of spastic paralysis, probably due to a cortical lesion of tubercular origin.—Dr. Ballantyne described a case of transverse presentation in deformed pelvis, and Dr. Maclagan one of intrathoracic aneurysm, where the physical signs and symptoms apparently pointed to a solid tumour.

25th November.—Dr. Simpson in the chair.—Dr. Herring delivered his presidential address on "The Origin of Medicine," confining his attention chiefly to the life and work of Hippocrates, to whom we were indebted for its deliverance from this thraldom and its establishment as a science. His writings proved him to have been a genuine clinician.—Dr. Waterston read a communication on "Manipulation in

Dislocation of the Shoulder," holding the explanation given by Kocher of the working of his own method to be incorrect. Experiments on the cadaver appear to show that reduction is independent of the coraco-humeral band. Mr. **Peel Ritchie** described the use of protargol (an albuminate of Ag) in gonorrhœa.

2nd December.—Dr. **Herring** in the chair.—Dr. G. F. B. **Simpson** delivered his presidential address, on "The Chamberlen Family." Dr. Simpson expressed the opinion that the invention of the forceps was the work of Peter Chamberlen, who came from Paris to London in 1569, and that it was concealed for about two centuries. It was not till 1813 that the instrument was discovered in a secret room in the old family manor home.—Dr. **Eason** read a communication on "The Theory of the Pathology of Chorea," and suggested that the disease might be due to an irritable condition of the cortical motor cells, arising because of a greater freedom from control of the highest centres.—Dr. **Sandstein** read a few notes on the "Interpretation of the Pulse," in diagnosing specific valvular lesions.—Dr. **Dingwall Fordyce** described three cases of aneurysm.

Forfarshire Medical Association.—*2nd December* 1898.—Dr. J. **Mackie Whyte** in the chair.—Dr. **Whyte** showed a boy who had suffered from ascites. He had suffered from chronic otorrhœa. There was evidence of lardaceous disease in liver, spleen, kidneys, and intestines, and in the liver there seemed to be deep fibrous constrictions. The abdomen was tapped repeatedly until October, when it suddenly increased in quantity, and the fluid ceased to gather in the peritoneum. The cause of the disease was apparently the otorrhœa. He also showed a brain from a case of purpura with hæmorrhage into the base, which seemed to have begun in the optic thalamus.—Mr. **Greig** showed part of a catheter removed from the bladder by suprapubic cystotomy. Professor **Kynoch** showed a specimen of gonorrhœal salpingitis and abscess of the ovary removed by cœliotomy. He also read a paper on the recent improvements in the treatment of vesical fistulæ, describing the various operations suggested and performed during the last fifty years, and giving notes of a case of his own in which he had obtained a successful result by paring and suturing the edges of the fistula. He also dealt with vesico-uterine fistulæ, in the treatment of which he thought that when closure of the os uteri was the only available treatment, the ovaries might be removed at a subsequent operation.—Dr. **Macgillivray** gave a short paper on the treatment of iritis, in which he emphasised the importance of the early and energetic use of atropine, combined with cocaine, in the form of an ointment, along with the internal administration of mercury.

OBITUARY.

Professor George James Allman, M.D., F.R.S.

George James Allman, who was born three years before the battle of Waterloo was fought, died in his 87th year at Parkstone, Dorset, in the end of November. A native of Cork, a school-boy in Belfast, a student at Dublin University, a graduate there in 1844, he was at once appointed Regius Professor to the University. This chair he resigned on obtaining the corresponding post in Natural History in the University of Edinburgh, a post held from 1855 to 1870. Professor Allman's energies and talents were chiefly directed towards the study of invertebrate forms, his intimate acquaintance with the characters of certain of their myriad genera leading to his being entrusted with the part of the report on the Challenger Expedition which dealt with them. The Royal Societies both of Edinburgh and London, the Royal Irish Academy, and the British Association, recognised the value of his life's work by awarding honours to Professor Allman—the first three, prizes; the last, the Presidentship.

Sir William Jenner, Bart., G.C.B., M.D., LL.D., F.R.S., Physician in Ordinary to the Queen, and to the Prince of Wales.

No better memorial of the late Sir William Jenner could be given than in the words of the *Court Circular* of 12th December: "He was not only a most able physician, but a true and devoted friend of Her Majesty's, who deeply mourns his loss." An able physician and a true friend, no more flattering epitome of the doctor's life-work could be conceived. And the story of Sir William Jenner's career fully merits the gracious words of Her Majesty. Born at Chatham in 1815, Sir William Jenner "walked the Hospital" at University College, London, gaining his diploma at the age of 22, his M.D., London, in 1844. Shortly after this he commenced his well-known investigations into the non-identity of typhus with typhoid fever; the paper recording his results, and published in our predecessor, *The Monthly Journal of Medical Science*, in April 1849, then edited by Sir William Gairdner, marked the final separation of the two fevers, which until then had still been regarded almost as one and the same. The merits of this piece of work were soon rewarded by his progressive appointments to teaching and hospital posts on a rapidly ascending scale, until in 1862 he became Professor of Medicine in University College. Attendant upon the Prince Consort and the Prince of Wales when they were attacked by typhoid fever, Physician in Ordinary to Her Majesty since 1861, Sir William Jenner soon became one of the fashionable consultants, and deservedly so.

By his death the medical profession is the poorer by one whose work will be long remembered, even although the announcement of his decease may come as a surprise to many with whom his personality has been but a name since his retirement from practice some years ago.

ANALYTICAL REPORTS.

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FRY'S PURE CONCENTRATED COCOA.

(Fry & Sons, Bristol.)

FRY'S well-known Pure Concentrated Cocoa, sent to us lately in sample, is the outcome of a special method of preparation, whereby almost all the fat present in the crude bean is removed, and the product rendered more easily soluble and digestible. The cocoa is free from sugar, starch, or other addition, possesses the peculiar flavour of the parent fruit in all its purity, and affords an excellent mean by which not only nutriment, but also stimulating refreshment, can be obtained at one and the same time from an article pleasing to the taste. It can give rise to no digestive disturbance, owing to its simple and unadulterated composition.

FRY'S MALTED COCOA.

(Fry & Sons, Bristol.)

THE same firm have sent us some Malted Cocoa, in which Pure Cocoa Extract is combined with Allen & Hanbury's Concentrated Extract of Malt. The Malt Extract used, made by these well-known chemists by a special process, and evaporated at the ordinary temperature *in vacuo*, is an excellent article, possessing all the active properties peculiar to malt in a highly concentrated form. The mixture of the Cocoa and Malt Extracts is dried at a low temperature, ensuring the continued activity of the ingredients of the malt. The product thus obtained is in the form of a powder, which, when stirred up into a paste with a small quantity of cold milk, is to be mixed with water not quite boiling, and the beverage flavoured to taste with sugar and cream. A cup of cocoa thus prepared is most nutritious, agreeable, easy of digestion, and, in fact, one of the best forms by which concentrated while still acceptable nutriment can be conveyed to the convalescent or the dyspeptic patient. Of the two preparations, perhaps we would choose the malted variety; it appears to be more palatable, and is certainly as rich, probably richer, in calorific value than the cocoa alone. This combination of a first class malt extract with so perfect a cocoa as Messrs. Fry's yields an ideal food; suitable both for the healthy and for invalids. The absence of any addition to it in the form of evanescent stimulants renders it all the more worthy of praise, and we can confidently assert that no article so nourishing as this is so easily and comfortably digested.

PROTENE FOODS.

(Protene, Limited, London.)

WE have examined the samples of foods sent us by the Protene Manufacturing Company with great interest. The articles forwarded for analysis and trial comprised protene loaves, biscuits, and flour. The main interest in these foods lies in the fact that they are prepared from flour composed of specially treated casein of milk. The flour is in the form of a fine white powder. How it is manufactured, to fit it for the purposes of baking biscuits and bread, is beyond our skill. One of the biscuit tins contained "Medical" protene biscuits, each about the size of an Albert biscuit—crisp, dry, with little marked taste, save an unobtrusive flavour, reminiscent of milk-casein. The average weight of these biscuits is just below ½ oz., two weighing ⅞ oz.

PROTENE "MEDICAL" BISCUIT.

	Parts per cent.	
Water	7	
Total Solids, at 110° C.	93	
Ash	4·1	(1·1 per cent.
Organic Solids	88·9	chlorine)

The organic solids were almost entirely composed of proteid. Testing for starch qualitatively, we found only a trace, with a little more erythro-dextrin. Allowing that only 80 per cent. of each biscuit consists of pure proteid, the proportion is quite unusually high for food-stuffs. This percentage yields 0·35 oz. of proteid per biscuit, compared with about 0·1 oz. of proteid in the same weight of beef, or 0·03 oz. in flour.

On dissolving a biscuit in water and an alkali, an almost clear yellow solution resulted. The addition, however, of an acid in excess brought down a copious curdy white precipitate of proteid.

The samples of flour and bread forwarded merit equally high commendation, as also do the biscuits of various kinds, apart from the "medical" form. Among these are biscuits flavoured with chocolate, sweetened with sugar or with saccharin, and others to which salt has been added. We confess that several of these forms are not so appetising as the medical biscuits; they are a little too greasy, but that is a fault which should be easily got rid of.

The protene flour yields abundant proteid products of digestion, shortly after it is submitted to artificial gastric digestion, and a trace of erythro-dextrin.

The great difficulty in the strict dieting of patients suffering from diabetes lies in the uninviting nature of the foods available, or the early distaste evoked by them. We would make bold to say that the manufacture of such biscuits as the "protene medical," containing almost no carbohydrate material, but nevertheless forming very typical biscuits, in form, colour, consistence, and taste, marks a great advance in our means of combating that disease. Time will show whether the favourable opinions we have formed about their usefulness in diabetic cases are to be justified or not.

In addition to their use in the treatment of diabetes, they should prove of service in gouty subjects, and in the nourishment of children who are averse to nitrogenous foods as a rule, perhaps to milk, and to whom a biscuit is an attraction. Two of the protene biscuits per diem can afford them as much proteid as an imperial pint of milk. The biscuits might also be usefully employed in many cases of amylaceous dyspepsia, in which starchy food proves difficult to digest, or milk in bulk disagrees. We hope before long to have opportunities of putting such to the test.

TANNIGEN (TRIACETYL OF TANNIN).

(Elberfeld Farbenfabriken Co. Ltd., 19 St. Dustan's Hill, E.C.)

[Owing, to an unfortunate mischance, the notice of Tannigen, in our October number, was intended to have appeared under the name of Tannalbin, the notes referring to Tannalbin having been in error appended to Tannigen.]

TANNIGEN is an ester of tannin, taking the form of a tasteless, absolutely odourless powder, insoluble in water and not affected by the action of dilute acids. It is altered by alkalies, and then acquires much of the astringent power of tannic acid. Consequently, the gastric juice is unable to alter Tannigen, which behaves in the stomach as an inert, insoluble body; but on reaching the bowel it seems to exercise beneficial astringent effects, judging by the results of the practical experience of numerous observers. Tannigen is recommended especially for acute and chronic diarrhœa in children, and also in adults. It may be given in doses of 5 to 8 grs., *ter vel quater in die*, for adults, or of 2 to 5 grs. similarly for children, suspended in half a glass of water. On dissolving some white of egg in water and adding tannigen, we found that no coagulation occurred, but the addition of an alkali to the mixture at once caused some of the proteid to assume the insoluble form, and which was not dissolved by the further addition of alkali. Dilute acids did not act in this way. Numerous papers have been published in medical journals, especially in Germany, in which the authors ascribe to Tannigen a very marked power of subduing catarrhal conditions of the intestinal mucous membrane without causing any symptoms of irritation of the bowel.

TANNOPINE.

(Elberfeld Farbenfabriken Co., 19 St. Dustan's Hill, E.C.)

STILL another recent intestinal astringent is prepared by the enterprising Elberfeld Company, in the form of Tannopine. This body, formerly termed Tannone, is a condensation compound of tannin and hexamethylane tetramine, with about 87 per cent. of the former, and 13 per cent. of the latter. It is a brownish powder, insoluble in water, in alcohol, or dilute acid solutions, not hygroscopic, but dilute alkalies dissolve it. Like tannigen it passes through the stomach unaltered, and behaves in the same manner as tannigen in the bowel. It is especially recommended in doses of from 3 to 10 grs. in children, and up to 16 grs. in adults, three or four times a day, for cases of tubercular enteritis, or for subacute and chronic forms of non-tubercular intestinal catarrh. Both preparations are infinitely superior to the crude forms from which the astringent action of tannic acid, or some of its compounds, was formerly obtained.

MONTHLY NOTES
ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,

MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

METEOROLOGICAL MEANS FOR THE EAST OF SCOTLAND DISTRICT, FOR THE FIVE WEEKS ENDING NOVEMBER 26, AND FOR THE YEAR FROM JANUARY 2.

	Temperature.			Rainfall in Inches.		Sunshine. Hours.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees.		Actual.	Accumulated from Jan. 2.	Actual.	Accumulated from Jan. 2.	
		Above 42°.	Below 42°.					
Mean .	41°·0	[*][1] 89° [2] 2317°	72° 756°	2·9	28·6	53	1238	3°·08
Observed	42°·5	[1] 102° [2] 2528°	71° 499°	4·1	26·8	44	1285	3°·1
Difference	+1°·5	[1] +13° [2] +211°	−1° −257°	+1·2	−1·8	−9	+47	+0°·02

VITAL STATISTICS.

MORTALITY IN FOUR LARGE TOWNS IN THE EAST OF SCOTLAND, DURING NOVEMBER, AND SINCE JANUARY 1. Actual Numbers and Rate per 10,000 per annum.

	Total.		Constitutional Diseases.		Circulatory System.		Nervous System.		Respiratory System.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1896 . .	[1] 1,224	217·0	204	36·2	104	18·5	123	21·8	311	55·5
	[2] 11,935	192·1	2321	37·3	1114	17·9	1359	21·8	2318	37·3
Actual Deaths and Rate per 10,000	[1] 1,139	202·0	233	40·8	92	16·3	127	22·5	258	46·1
	[2] 12,129	195·2	2551	41·0	1163	18·7	1386	22·3	2092	33·6
Difference from Mean	[1] −85	−15·0	+29	+4·6	−12	−2·2	+4	+0·7	−53	−9·4
	[2] +194	+3·1	+230	+3·7	+49	+0·8	+27	+0·5	−226	−3·7

	Digestive System.		Urinary System.		Pneumonia.		Phthisis.		Cancer.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1896 . .	[1] 69	12·2	30	5·2	100	17·8	97	17·5	50	8·9
	[2] 788	12·6	313	5·0	843	13·5	1155	18·5	524	8·4
Actual Deaths and Rate per 10,000	[1] 77	13·6	33	5·7	108	19·2	102	18·5	56	9·95
	[2] 962	15·5	292	4·7	959	15·4	1160	18·6	608	9·7
Difference from Mean	[1] + 8	+1·4	+3	+0·5	+8	+1·4	+5	+1·0	+ 6	+1·05
	[2] +174	+2·9	−21	−0·3	+116	+1·9	+5	±0·0	+84	+1·3

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[*] NOTE.—The figure [1] denotes the data for November, [2] for year to date.

The first three weeks of November were marked by very mild weather, with a mean temperature of 44°·5 F., the fourth week was cold, wet and stormy, with a mean temperature of only 36°·4 F.; the mean of the earlier period being 3° above, that of the last week 3° below, the normal. The month as a whole was warm, wet, less sunny even than an ordinary November, but with a normal variability of temperature.

The meteorological values for the year to date still afford evidence of a very abnormal excess of heat and a similar defect of cold, a diminished rainfall, and an excess of sunshine.

The number of deaths was considerably below the average, especially among those credited to respiratory diseases, *i.e.* 17 per cent., although, as so often, the deaths from pneumonia do not accord with this. The only other system among those discussed to show a decrease is the circulatory. All the others show a slight increase, marked only under constitutional diseases. Influenza caused six deaths.

The accumulated figures for the year to date show 1 per cent. increase in the total number of deaths, almost 10 per cent. under constitutional diseases, over 22 per cent. under digestive disorders, nearly 14 per cent. from pneumonia, and 16 per cent. from cancer; the only marked decrease being that from respiratory diseases, rather under 10 per cent.

In November, therefore, characterised by warmth (save during the last week), excess of precipitation, but a normal variability of temperature from day to day, the death-rate was low, deaths from lung disease much below the usual.

The year up to date shows very unusual excess of warmth, absence of cold, and diminished rainfall, along with more numerous deaths, especially from digestive diseases, from cancer, pneumonia, and constitutional ailments. Deaths from all lung diseases (save phthisis) have been few.

EDINBURGH MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

A RETROSPECT OF A FIRST SERIES OF ONE HUNDRED AND TEN ABDOMINAL SECTIONS FOR PELVO-ABDOMINAL DISEASE.[1]

By F. W. N. HAULTAIN, M.D., F.R.C.P.Ed., *Physician for Diseases of Women, Deaconess' Hospital, Edinburgh.*

FROM the recent rapid advance in abdominal surgery, by which interference with the peritoneum has been shorn of its terrors, abdominal operations are now performed so frequently that it is superfluous to describe in detail each individual case. To those,

Table of Operations.

Operation.				Total.	Mortality.
Ovariotomy	Simple pedunculated cysts	Unilateral	18	35	0
		Bilateral	3		
	Simple sessile cysts	Abdominal	3		
		Subperitoneal pelvic	4		
	Papillomata	Unilateral	1		
		Bilateral	2		
	Solid tumours	Fibroma	2		
		Oöphoroma	1		
	Dermoid		1		
Broad ligament cysts				2	0
Hysterectomy	Fibrocystic		2	3	0
	Simple fibromyoma		1		
Myomectomy				3	0
Mixed tumours				2	0
Extra-uterine pregnancy				5	0
Ventro-fixation	Retroflexion		2	5	0
	Procidentia uteri		3		
Porro Cæsarean				1	0
Suprapubic cystotomy				3	0
Exploratory				10	1
Removal of Appendages	Dysmenorrhœa			2	0
	For fixed delusion			1	0
	For fibromyomata			4	0
	Diseased ovaries			8	0
	Matted appendages			15	0
	Simple tubal disease	Hæmatosalpinx	3	11	1
		Hydrosalpinx	1		
		Pyosalpinx	6		
		Tubercle	1		
				110	2

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[1] Read before the Obstetrical Society, Edinburgh, 14th December 1898.

however, who have many opportunities of performing such operations, a small residue of cases remain which are of interest, both clinically and from a surgical standpoint. It is my intention, therefore, to direct your attention to a few such instances, which have in my comparatively small experience presented themselves.

In the first place, it may be well to tabulate in a general manner the conditions for which I have thus operated, and thereafter to consider specially those of interest, under their individual groups. I may further state that this table embraces my entire experience in this branch of gynæcological surgery.

Ovariotomy.—This operation was performed on women who varied in age from 21 to 81 years. The tumour in the latter case was a large intraligamentary cyst, which grew upwards between the layers of the mesocolon, and was thus encircled by the adherent colon. Through the difficulty in enucleating and separating the cyst, the operation was a very prolonged one (one and a quarter hours), yet in spite of the great age of the patient she made a rapid and thorough recovery.

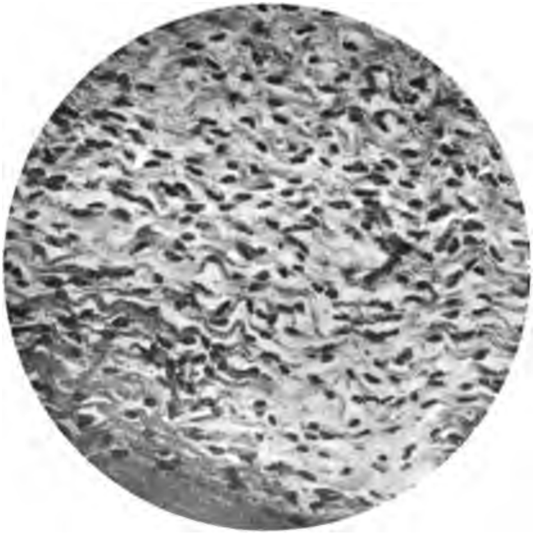

FIG. 1.—Solid ovarian tumour; white fibrous tissue.

The largest ovarian tumour I have removed weighed 83 lb. The patient herself on recovery weighed but 1 lb. more, which solved the previously doubtful question as to whether the patient had been removed from the tumour, or *vice versâ*.

Perhaps the most interesting case in this group was one in which there was a rudimentary uterus, not larger than the terminal phalanx of the little finger, and a total absence of Fallopian tubes. The cyst was intraligamentary, pelvic, and sessile. From the absence of the usual landmarks of the uterus and tubes, the enucleation was considerably complicated; the pedicle, if such it might be termed, having to be formed by the infundibulo-pelvic ligament.

I have been more than usually fortunate in meeting with three solid ovarian tumours. These from their rarity and histological peculiarities are full of interest.

One, a tumour weighing 6 lb., was found to be composed of typical white fibrous tissue (Fig. 1); another weighing 1½ lb., presented all the characteristics of an exaggerated chronic ovaritis; while the third was entirely cellular throughout, and it is yet a matter of controversy whether it more closely simulates an encephaloid cancer or an alveolar sarcoma (Figs. 2 and 3).

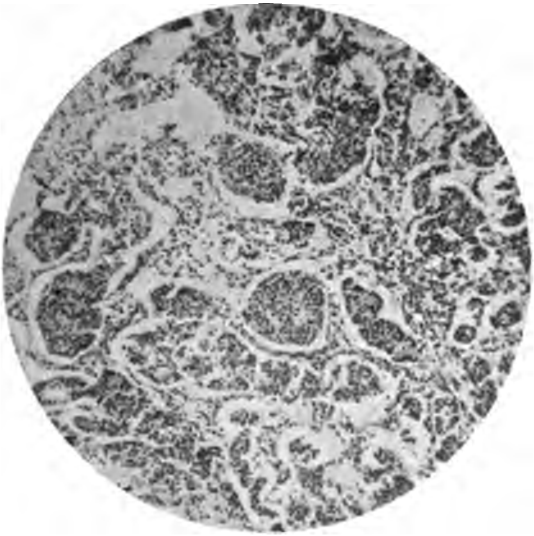

Fig. 2.—Solid ovarian tumour; oöphoroma (low-power).

The two first tumours were associated with rapidly developed and excessive ascites; with the third there was no trace of peritoneal effusion. This at first sight is contrary to what one would expect, as, from the apparently malignant nature of the cellular tumour, peritoneal effusion would seem a more natural consequence. These cellular tumours of the ovary, however, though apparently composed of embryonic cells, must not necessarily be

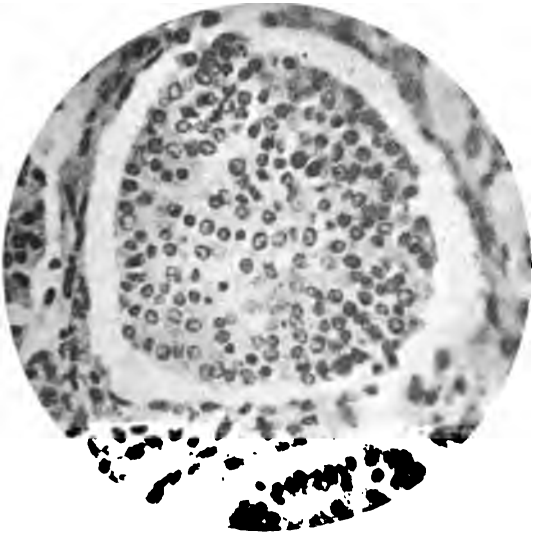

Fig. 3.—Solid ovarian tumour; oöphoroma (high-power).

considered as malignant in character, as they do not tend to give rise to secondary metastases. I am inclined, therefore, to agree with Bland Sutton, that they cannot be designated as sarcomata or carcinomata, and am disposed to consider them as a special variety of new growth, which has ingeniously and aptly been described by him, oöphoromata.

The presence of ascitic fluid in association with solid tumours of the ovary is a matter of considerable interest, and is difficult to account for, especially when one considers that similar tumours of the uterus are not equally complicated. It would seem to me that the difference is to be found in the mode of circulation in the growths. The uterine fibroid is, as we know, entirely nourished through its capsule, in which the circulation is at all times easy, while the ovarian tumour is nourished entirely by means of vessels which enter within a limited area, and ramify throughout its structure. It will thus be evident that the blood return will, in dense fibrous growths, become gradually impeded, and a serous intraperitoneal exudation predisposed to. This was well exemplified in the second of my cases, in which the periphery of the growth was found to be intensely congested, and the peritoneal cavity was at the same time filled with an abundant blood-stained effusion.

That the circulation is difficult in fibrous ovarian tumours, must be conceded, from the frequency with which degenerative cavities are found in their interior, and also from their extremely slow growth and comparatively small size; in fact, if it were not for the accompanying ascites, the presence of many of those operated upon would remain unknown.

The only other ovarian tumours which I have found associated with extensive peritoneal effusion have been fungating papillomata; in both instances the growths were bilateral, and since removal there has been no return of the ascites. As at the operation no evidence could be found of infection of the peritoneum, one has no reason to expect its recurrence.

I may add that one of these patients had been tapped fourteen times in the course of the year previous to the operation; her condition being considered hopeless, and due to malignant abdominal disease. Although reduced to the utmost stages of weakness, I considered an exploratory incision advisable, and by this means I was enabled to remove the cause, with the most satisfactory result. I should like here to strongly impress the importance of exploratory abdominal incisions, where diagnosis is not absolutely certain, and especially in cases of obscure ascitic collections.

Hysterectomy.—As the table shows, this was done twice for fibrocystic myomata; in one of these there was a distinct cyst, in the other the tumour was merely œdematous, which may be considered as the initial stage of cystic formation. In both, the tumours were interstitial and large, and were associated with profuse menorrhagia and metrorrhagia. The remaining hysterectomy

I performed against my will, on a patient whose mind had been poisoned against the electrical treatment which I recommended. It was a simple interstitial fibromyoma, which I have little doubt would have yielded to the conservative method proposed. The operation in each case was similarly performed, and the stump treated intraperitoneally, after ligature of the uterine arteries. Myomectomy was undertaken in each case for pedunculated subserous fibroids; one of these I mistook for an ordinary ovarian cyst, and it was only after opening the abdomen that I became aware of the true condition.

Mixed tumours.—In one of these I found and removed an ovarian tumour on the left side, and a simple broad ligament cyst on the right side. The other case was more complicated, and proved a veritable museum. On the right side there was a large ovarian cyst, and a medium-sized broad ligament cyst (so-called parovarian), on the left side there was a large broad ligament cyst, while in the uterus were numerous fibromyomata, one of which was the size of a cocoa-nut. As the fibroid was not markedly pedunculated, and having already removed both ovaries, I, with Dr. Milne Murray's approbation, left it alone.

Extra-uterine gestation.—In the five cases of this group the operation was performed after the symptoms of so-called rupture had been manifested. In four there had been profuse intraperitoneal hæmorrhage, which had given rise to the usual symptoms of profound anæmia, but in one of these only was there any evidence of rupture. The remaining three showed that the source of the bleeding was through the patent abdominal extremity of the tube, a feature so strongly and ably dwelt upon by Cullingworth. From a close examination of the specimens after removal, it seems to me that, in the majority of cases, separation of the ovum with necessary bleeding is the primary factor in the so-called rupture. Should there be free exit to the blood, this is naturally taken advantage of; hence not only the bleeding from the abdominal ostium into the peritoneal cavity, but also the irregular uterine discharges with which ectopic pregnancy is so constantly associated at the time of the so-called rupture.

The fifth case was of more than ordinary interest, in so far as the tubal pregnancy occurred in a patient who had had an appendicitis seven years previously, and in whom the surrounding structures had evidently been left in a matted and adherent state,—so much so, indeed, that from the time of her appendicitis she had complained of a marked limp with the right leg. After having missed a period, the patient complained of a severe persistent pain in her side for three weeks, at which time she had a profuse uterine hæmorrhage and shed a complete decidual cast, which was thought to be an ordinary miscarriage. As thereafter the pain increased rather than lessened in severity, I was asked by

Surgeon-Captain Watson to see her, and recommended immediate operation. On opening the abdomen, the intestines presented an intensely congested appearance; those in close continuity with the swollen and adherent right uterine appendages were of a purple-black colour, themselves closely simulating a gravid tube, from which indeed Dr. Murray and I had considerable difficulty in distinguishing them. After careful separation of the intestines, the gravid tube was found embedded in the pelvis; this I ligatured at both ends and excised, and subsequently stitched the two cut edges of the broad ligament over the site from which the mass had been enucleated. After recovery from the operation, the patient was surprised to find that the limp from which she had previously suffered had entirely disappeared.

Ventrofixation.—This operation I have found eminently satisfactory. It was performed twice for enlarged congested and fixed retroflexed uteri, which gave rise to marked symptoms, and in which douching and plugging had been futile in so softening the adhesions as to allow of reposition by ordinary methods.

Out of three cases of procedentia for which it was undertaken it has alone been absolutely successful in two, while in the third a secondary colporrhaphy had to be performed to rectify a most pronounced rectocele which remained.

The method of fixation I have adopted and can thoroughly recommend, is by stripping the parietal peritoneum off the edges of the abdominal wound, and thus apposing and fixing the fundus to the extraperitoneal tissues by means of silkworm gut sutures, which include the skin and recti muscles as well as penetrate the anterior uterine wall. So far, none of my cases have subsequently become pregnant, but from the mass of statistics published, where such has subsequently obtained, I do not anticipate more than a thirty to one chance that labour will in any way be unduly complicated.

Porro Cæsarean.—This operation I performed in a country cottage in Perthshire in the middle of the night. The patient was a dwarf, 3 ft. 11 in. in height. When I first saw her she was in the second stage of labour; the uterine contractions were extremely violent, and uncontrolled by deep chloroform anæsthesia, while retraction of the uterus was excessive, with a corresponding alarming thinning of the lower uterine segment. The conjugata vera was below 2 in. As I feared that rupture of the uterus might at any time occur, I recommended immediate operation, even though our armamentarium was incomplete and the surroundings by no means reassuring as regards asepsis. With the co-operation of Dr. R. Stirling, the operation was easily and rapidly performed by the Porro-Muller method, the pedicle was treated extraperitoneally and secured by the elastic-tube method, so ingeniously devised by Lawson Tait. The patient recovered without a hitch, and has now a son, who, though but 6½ years old, is commencing to rival his mother in stature.

Suprapubic cystotomy.—This was undertaken in each instance for new growths in the bladder, all of which were papillomatous and freely hæmorrhagic. In none has there been a reappearance of the bleeding, although it is now more than a year since the last patient was operated on. The age of the patients was 17, 50, and 52 years respectively. The thoroughness and ease with which one can remove these tumours by the suprapubic method as compared with the route by vaginal incision or urethral dilatation, justifies its selection; and as it is well known the special tendency of these simple vesical papillomata is to become malignant, it seems dangerous to tinker with them by haphazard scraping through the dilated urethra. As regards post-operative drainage in these cases, I have tried both the suprapubic (through the wound) and the urethral. The latter is most agreeable and comfortable to the patient, and in the case in which it was adopted proved most efficient. A No. 20 soft rubber catheter was used.

Exploratory incisions.—What exactly is included under the term exploratory incision is difficult, nay impossible, to define. To the experienced abdominal surgeon the frequency of exploratory incisions increases; in other words, the oftener he opens the abdomen the more does he realise the difficulties of the exact diagnosis as regards its morbid contents, and his inability to give that absolutely correct decision which alone can satisfy his conscience. Having opened the abdomen, his opinion is verified or the reverse, and he proceeds to complete the operation by the removal of the previously doubtful quantity, and catalogues the operation under its special designation. The group of exploratory incisions, therefore, must be considered as a sort of "toom" for operations, in which removal of the disease is considered unjustifiable, or where there are no diseased structures to remove; at the same time it may be made to include a number of operations which cannot be grouped under any other heading, as, for instance, freeing adherent healthy ovaries, etc. etc.

As is to be expected, therefore, my operations under this heading, though described as but ten, would have been much increased in number, if the literal translation of the term exploratory incision had been upheld.

Three times I opened the abdomen for ascites and found extensive secondary malignant infiltration of the peritoneum. Once the infiltration was tubercular, and was drained with gratifying result.

In one case I freed apparently healthy though adherent ovaries on account of pain, and once stitched prolapsed apparently healthy ovaries to the pelvic wall. In another instance I freed an adherent pedicle after a previous oöphorectomy, which was causing much pain and discomfort. This I may say is my entire record of conservative abdominal surgery. I as yet do not feel myself sufficiently competent to recognise, by a slight superficial inspection, ovarian disease of such a limited nature as to warrant me in resecting the morbid portion from the organ. Possibly a great future may be expected on these lines, but much yet remains to be explained as regards ovarian pathology, before one can hope to justify its ready performance.

Another case of some interest in this group was an absolutely adherent ovarian cyst, which was so intimately fixed to the anterior abdominal wall that I could not separate it. The patient had been seen some years previously by a very eminent ovariotomist, who then refused to operate, but as the tumour was now giving rise to urgent symptoms in spite of frequently repeated tappings, I decided to attempt its removal. My ambitious endeavours being futile, I made a large incision through the cyst wall, introduced my hand, broke up numerous loculi, and evacuated the contents. I then washed out the cavity with a weak solution of iodine, and drained. For three months the cavity was washed and drained before it became obliterated. The patient is now, however, perfectly recovered, though it is more than nine years since the operation was performed.

This was my first attempt at, and is my only unfinished, ovariotomy; had I been more experienced, doubtless I would have made more energetic attempts towards the complete removal of the tumour, but certainly with no better ultimate result, probably infinitely worse.

In this group I have unfortunately to record one of my fatalities, perhaps one of the simplest operations I have performed. It was undertaken for the removal of a hard swelling situated behind the uterus, which was giving rise to pressure symptoms. The tumour proved to be a subserous cervical fibroid, which from its attachments I considered would be more easily removed through the vagina, and I therefore closed the abdomen with the intention of subsequently operating by that route. The patient, however, developed acute septic peritonitis, and died on the third day.

Removal of appendages.—This operation was undertaken, as can be seen from the table, for a variety of pathological conditions, many of which are of interest.

Though loath to undertake so radical a case for dysmenorrhœa, it appeared to me absolutely necessary to perform it on two young women, in whom all other methods of treatment had failed, and who could not from their social position afford to lie up several days in each month. I may say both are now in active service, and have never regretted the means which were employed for their restoration to health and usefulness.

I also removed the appendages from a lunatic in an asylum in England, at the desire of the medical superintendent, to try and cure a monthly mental aberration (that she was harbouring a man inside her); the result at first gave prospects of success, but unfortunately the unwelcome lodger returned after a few months' holiday.

Three of the cases in which I have removed the appendages for bleeding fibromyomata have enjoyed absolute health since the operation; the other continues to menstruate as before, with great pain and in excessive quantity.

As will be noted, I have had a seemingly large number of removals of simple diseased ovaries. Two of these proved to be hæmatomata; in one, the opposite ovary seeming healthy, I left it. The remaining cases ware operated upon for chronic ovaritis, interstitial and follicular. The ultimate result as regards these and the subsequent group of matted appendages have been to me sufficiently satisfactory to warrant the procedure. In quite 60 per cent. the previous pain and uselessness has been entirely removed, in 30 per cent. a marked improvement has been apparent, but the remainder, I am sorry to say, have remained *in statu quo ante.* In all these instances the patients had previously undergone a prolonged course of treatment without avail. It should also be noted that in many cases the beneficial effects of the operation were not apparent till many months after its performance.

There is no class of case so anxious to the conscientious surgeon. By thus operating he exposes the patient to considerable risk, simply to alleviate pain, which is an uncertain quantity. Her life, though miserable to herself, may be of the utmost value to her friends, children, and others, while the disease she suffers from cannot be said to be dangerous. Under no circumstances, therefore, do I urge the operation, but merely suggest it after anxious deliberation, as a probable means of cure, and explain the risks and possible attendant drawbacks to herself and friends, more from the seamy than the rosy aspect. Within the last decade there has been an exaggerated swing of the pendulum in the opposite direction, from the reckless frequency with which this operation was performed; to this I am no partisan. There is a happy medium; the operation is one of great value, and to it I am sure many women owe much gratitude and thanks.

The cases in which removal of the appendages has been undertaken for simple tubal disease offer two instances of outstanding interest. One a hæmatosalpinx, in which the patient, though suffering from enlarged and distended tubes, had never menstruated, though at the same time she was incapacitated from work by severe monthly pain. The uterus measured 1 in., by the sound.

The other was a case of apparently primary tubercle of the Fallopian tubes developing in an elderly spinster of 54, and giving rise to secondary ascites. The operation was performed to ascertain the probable cause of the peritoneal effusion, which had baffled many physicians to account for. That the disease was primarily in the tubes I am disposed to think, from the history of the case, as the patient dated the commencement of the illness from a free vaginal discharge, which suddenly appeared about five months before the abdominal distension was complained of.

In this group my other fatality is to be found. It occurred as the result of the removal of extremely adherent pus tubes, where, at the time of the operation, a small amount of pus escaped into the peritoneal cavity. Although free irrigation was employed, it failed to prevent infection of the peritoneum; the patient never seemed to rally, and died with acute septic symptoms early on the third day.

Technique of operations.—In the technique of the operations described I have nothing original to suggest; my aim has been entirely directed to three primary essentials—(1) Asepsis, (2) control of bleeding, and (3) precaution against injury to the hollow viscera. As regards the question of antisepsis *versus* asepsis in abdominal surgery, I am at a loss to give a definite opinion, as I have employed, and continue to employ, both methods as circumstances demand, with equally satisfactory results. In my hospital cases, where asepsis can be carried out in a thorough and efficient manner, I entirely confine myself to aseptic methods. In private, however, where the surroundings are necessarily questionable, I have always adopted antiseptic measures. These consist in the free use of 1 in 40 carbolic acid for sponges, instruments, and hands. As far as my experience shows, there is nothing to be said in favour of one method as against the other. In every instance, however, I poultice the skin of the abdomen for forty-eight hours before operation with a compress of 1 in 3000 corrosive. By this means I have certainly lessened, though not entirely removed, the presence of stitch-hole abscesses.

The control of hæmorrhage is undoubtedly one of the most important factors in successful abdominal surgery. By this I do not simply mean the securing of all bleeding points before the abdomen is closed, but more particularly the conservation of blood, or, in other words, the prevention of unnecessary blood loss during the operation. I feel convinced that many of the cases which die shortly after operation with symptoms of shock, so called, are the result of slight venous oozing, which has followed the loss of a large quantity of blood during the operation. The slight venous ooze is, indeed, the last straw, and is equivalent to the apparently trivial post-partum trickle, which accounts for so many deaths after placenta prævia. With this in view, therefore, I make a point, before proceeding to free adherent tumours from the surrounding structures, to attempt to cut off the circulation through the tumour, by ligaturing the main sources of blood supply. In ovarian and tubal cases this is, as a rule, easily accomplished by a stitch through the infundibulo-pelvic ligament which controls the ovarian artery, and another deep stitch through the broad ligament at the angle of the uterus and tube, by which the uterine supply to the ovary is cut off. Hæmorrhage into the cavity of a tapped cyst is also thus avoided. This is very apt to be concealed and overlooked, and must always be considered as a possible complication. For the same reason I never separate large adherent areas at one time, preferring rather to control the bleeding as I slowly proceed.

It will thus be evident that my object is not brilliancy and rapidity; these I consider unnecessary, and more of the nature of playing to the gallery. The benefits claimed for rapidity, if real, may be more than counterbalanced by a short, sharp hæmorrhage, while the dread fear of so-called shock from prolonged operations, as far as my experience goes, gives infinitely less anxiety than a severe blood loss. True shock after abdominal operations I consider a much over-rated danger. I cannot recall a single instance in my own practice which gave me a moment's anxiety, although in difficult cases the operations have at times lasted at least two hours. That shock is a possible cause of death, perhaps one must admit. Its rarity, however, is extreme. Doubtless it is a convenient term for filling up death certificates, impressing sorrowing relatives, and apparently soothing the operator's conscience. It is, however, so closely simulated by acute septic infection and excessive blood loss, that it can only be considered when, by post-mortem examination, these have been proved to be absent.

Where dense adhesions exist between tumours and the hollow viscera, I prefer to leave a portion of the cyst wall attached; this I thoroughly scrape, invaginate the edges, and stitch with a continuous suture.

With regard to the "toilette" of the peritoneum, I am not nearly so energetic in sponging the cavity clean as I at first was. I believe the presence of aseptic blood is less harmful than the irritation of prolonged sponging. In fact, in one of my cases of extra-uterine pregnancy, in which the patient was pulseless and almost moribund, I took advantage of some of the effused peritoneal blood as a restorative, by mixing with it *in situ* a pint of warm sterilised normal salt solution, before closing the abdomen. The result was most satisfactory, and I cannot help thinking that its ready absorption was advantageous to the patient.

When pus or other foreign fluids escape into the peritoneal cavity, I irrigate with 1 to 40 boroglyceride solution, which seems to be better borne by the delicate peritoneal endothelium than simple sterilised water, and it is at the same time antiseptic.

Drainage through the abdominal wound I now never employ, except I fear excessive hæmorrhage from venous oozing; here the tube is used only as an indicator of the presence of bleeding. Should drainage of the peritoneal cavity be necessary, I should be disposed now in every case to drain through the posterior fornix vaginæ, as it appears to me the drainage by this route must be more thorough, a contention which is strongly supported by the almost entire absence of septic peritonitis after vaginal hysterectomy. This method of drainage I had occasion to use in one case, after rupture of a pyosalpinx during removal. An ordinary gauze drain was used, and left in for thirty-six hours; the result was eminently satisfactory.

In suturing the abdominal wound I include all layers with silkworm gut stitches; in some cases I have omitted the peritoneum, dreading its infection should stitch-hole abscesses develop. Both methods seem equally satisfactory. I may say that as yet I have no case of ventral hernia, though my series of operations has extended over a period of ten years.

Post-operative treatment.—In our improved methods of the after-treatment of abdominal sections I fully believe lies the secret of the greatly diminishing mortality in these cases of recent years. I, even during my experience, have seen it almost entirely revolutionised. In my earlier cases I followed as implicitly as I could the starvation methods then adopted. These, however, my humane instincts recoiled from very soon, and led me to follow gradually an almost entirely opposite course. The visions of the torture which patients suffered from the total want of fluid for forty-eight hours after a prolonged operation, remains with me now as a nightmare, and savours more of the treatment of prisoners in a Chinese jail than patients in a British sickroom. No doubt it was adopted, as based on an ingenious physiological theory of peritoneal absorption, which fortunately, as so often happens, has been found clinically to be entirely fallacious.

As in the technique of operations, so in the after-treatment, there are three essentials I aim at—(1) To preserve strength, (2) to promote comfort, and (3) to prevent intestinal paralysis.

As regards the first of these, I never omit the routine custom of a nutrient enema before the patient leaves the operating-table, which is to be repeated every four to eight hours, according to the condition of the patient, should she be unable from chloroform sickness to take nourishment by the mouth. The enema I am in the habit of ordering is—whisky, 1 oz.; yolk of egg, ½ oz.; bovril ad 4 oz.

Should the stomach be able to retain fluid after four to six hours, small quantities of tea, coffee, or dilute meat juices can be given. Should, however, as is usual, chloroform sickness be present, enemata only are employed. In prolonged and excessive chloroform sickness and retching, a large drink of hot water is useful in washing out the stomach; and should vomiting still continue, I have found it frequently stopped by a teaspoonful of white of egg and whisky beaten up together, given at intervals. After twenty-four hours, should there be no inclination to sickness, tea, gruel, soups, and nutrient jellies should be frequently given in small quantities. Milk foods and aerated drinks I have found objectionable. On the third day the patient may partake of ordinary light diet. Should the pulse be at all feeble or quick, cardiac stimulants, preferably strophanthus, should be employed from the commencement. This is to be recommended as routine practice in patients over 50 years.

In the promotion of the patient's comfort our attention is most frequently directed to combating thirst and pain. Thirst I find best alleviated by large normal saline enemata (1 pint), and swabbing the gums, mouth, and lips with a weak solution of glycerin and water.

Pain in the wound is diminished by placing a large pillow below the patient's knees, to remove abdominal strain. For the severe pain in the back which always is present after removal of the appendages, and occasionally after ovariotomy, I never hesitate to give one dose of morphia (½ gr. suppository or ⅙ to ¼ gr. hypodermically). It frequently ensures a quiet and restful first night, which more than compensates for the minor ulterior effects it is supposed to cause. I may add I never repeat the dose.

I also invariably insist on the patient being turned on her side several times during the first forty-eight hours. This not only is the means of giving the patient relief from prolonged lying in the dorsal position, but I feel is of the utmost value in promoting free peritoneal drainage by absorption. By so doing any pool of intraperitoneal fluid which must necessarily, in the dorsal position, lie stagnant in the retro-uterine pouch, is disseminated over the entire peritoneal surface, where, if aseptic, it is readily absorbed, and if it contain organisms they are subjected to a ready and powerful phagocytic action. Much may be gained by attention to details in promoting comfort. Ease and contentment are valuable agents in fostering recuperative power.

Flatulent distension of the intestines is extremely common, and must be treated early to avoid anxiety. It may usually be avoided by an aperient on the third day, but, should it develop, strenuous endeavours to overcome it must be made. The most ready methods of doing so, I have found to be large turpentine enemata, introduced far up into the bowel, as high as the vaginal nozzle of a Higginson syringe will reach, the nozzle being kept in the rectum till the enema is returned. After this, with the nozzle still *in situ*, gentle massage along the colon will usually be found effective; if fruitless, nothing remains but a free purge with calomel (5 grs.) and saline.

Accidental complications.—Before concluding, it may be interesting to relate some rare accidental complications which I have met with. In one instance I had a severe secondary hæmorrhage from the abdominal wound, which nearly resulted fatally. In another, after shifting the patient from one room to another on the seventh day, a pulmonary embolism resulted, probably from the detachment of a small thrombus in the pedicle.

A case of double suppurative parotitis after ovariotomy also may be mentioned, interesting in so far as the night attendant on the patient had recovered from mumps only a fortnight previously. As is well known, parotitis is an occasional complication of ovariotomy, and the case in point may be looked upon merely as a coincidence; on the other hand, it is possible that there may be a special susceptibility to mumps infection after such operations.

The last complication I shall mention is one of such interest, that I may claim your forbearance in relating it more fully. It arose through my carelessness in leaving a pair of catch forceps in the peritoneal cavity.

The case was a double ovariotomy, in which, from the solid nature of the tumour, a large abdominal incision was necessary. There were no adhesions or other complications; the operation, in fact, was as simple as could possibly be.

On returning my instruments to my private hospital, my matron informed me the day afterwards that a pair of forceps were missing. Knowing I had no occasion to use forceps in the peritoneal cavity, I naturally did not consider they were there, and surmised they had been lost in transit. The patient, during the first week after the operation, however, did not appear to be progressing so well as usual, and complained considerably of abdominal discomfort and backache. This continued, and on the sixteenth day a localised abdominal distension appeared. The association of the lost forceps and the abdominal swelling now dawned on me and haunted my thoughts. I was loath, however, to believe in such a probability, and waited for another week. By this time, however, it was evident the patient (not to say myself) was rapidly losing ground, and becoming emaciated and hectic, while her abdomen was becoming irregularly distended. I therefore resolved to sift the matter and make an exploratory incision. This I did on the twenty-third day, with the co-operation of Dr. Alexis Thomson, and to my horror found the forceps embedded in plastic effusion. With the utmost difficulty they were enucleated from their bed, and the resulting vascular cavity stuffed with gauze. After this, fortunately, the patient made a slow but good recovery. From a practical point of view, the case is of interest in showing how easily and apparently unaccountably such an accident may happen, and is a severe object-lesson of the scrupulous care required in attending to the details of abdominal surgery. From a scientific point of view, it is interesting in showing, as the chart demonstrates, how in such a case, with a practically normal temperature and pulse, the most serious active mischief may be present in the peritoneal cavity (see Chart, Fig. 4), which in this instance was apparently rapidly progressing to a fatal issue.

As the table shows, I have had two deaths as the result of my operations, both of which were from acute septic peritonitis. In one of these the reason is not difficult to seek, in so far as it occurred after soiling of the peritoneum with pus from a ruptured pyosalpinx. In the other, however, some faulty procedure must lay the blame on myself. Occurring as it did after one of the very simplest of my cases, it stands prominently out as a warning of the risk and danger (of which I am keenly cognisant) that are associated with even the most trivial interference with the peritoneum.

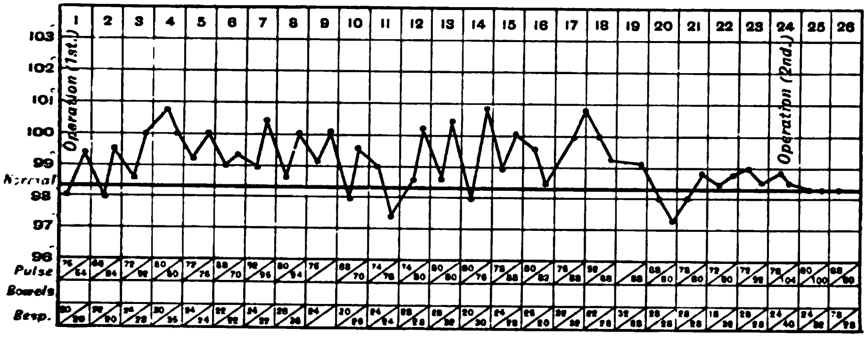

FIG. 4.

I cannot close without expressing that any success I have obtained in my cases has been mainly due to the many opportunities kindly afforded me by Dr. Halliday Croom, whose methods I had the honour for years to watch, and thus became thoroughly conversant with; and further to Dr. Milne Murray, whose co-operation with me in my earlier cases and many of my subsequent severer operations, has given me that much-needed support, which is so essential in the performance of anxious operations when previous experience is more or less wanting.

THE INVOLUTION OF LIFE—A SCHEME OF PATHOLOGY.[1]

By ROBERT MAGUIRE, M.D., F.R.C.P., *Physician to Out-patients and Joint Lecturer on Pathology, St. Mary's Hospital; Physician to the Hospital for Consumption, Brompton, London.*

(*Concluded from page* 12.)

Digestive system.—The law proposed brings a new light to bear upon the puzzling subject of functional dyspepia, that is, dyspepsia in the absence of organic disease; and, I venture to think, assists in the proper diagnosis and treatment of the disorder. The numerous observations made in the last twenty years upon the phenomena of dyspepsia have really given us but little help in the practical treatment of our cases, and this is because the researches have been conducted on too narrow lines. The specialist observers have assumed that they had to deal with a matter of pure chemistry, and have ignored the vital element which lies behind the facts they have discovered. They have dealt only with the results of the disease, and thus have given us subdivisions of hyperchlorosis, achlorosis, acid dyspepsia, flatulent dyspepsia, and so on, divisions which may at times be warranted by exceptional and individual cases. But let us take the greater number of cases of functional dyspepsia which come before us, and see if we cannot get a more general view of the phenomena of the disorder which will be more helpful to us. Empirically, dyspepsia has been divided into classes, according to the kind of food which in each class has seemed to be ill-digested; and while the separation of such classes seems to be inadvisable, there is, nevertheless, a true scientific reason for viewing dyspepsia in this light.

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[1] An Introduction to the course of Special Pathology at St. Mary's Hospital.

The food of the embryo is the contents of the yolk-sac—pure albumin, requiring and receiving no digestion before absorption. The food of the suckling is milk, consisting of proteid, carbohydrate, and fat. Now, while a certain amount of chemical change—namely, saponification—occurs in the digestion of fat, yet its principal and most important preparation for absorption is emulsification, and in milk it is already emulsified in a manner so perfect as to be impossible of imitation by artificial means. Even such a slight departure from this state as the appearance of the fat in the form of colostrum corpuscles, is sufficient to cause fat-dyspepsia in the infant, and diarrhœa, not without use in removing meconium from the bowel. Milk-fat, then, practically requires no digestion. The same must be said of the carbohydrate, lactose, a simple and easily diffusible form of sugar, which can be regarded as starch predigested by the mother. The proteid is, then, the only constituent of the milk which requires digestion. Proteid digestion is the first to be established, and this, for good reason, in that while the body can do without fat and starch for a time, proteid is the food for heart and nerve, and without it starvation ensues. Its digestion is not perfected at first, for even slight mechanical variation of its form, as the substitution of cow's milk for human milk, hinders the process. Starch digestion does not begin till the seventh month of extra-uterine life, and is not perfected till the twelfth month. The digestion of fat—that is, fat in its ordinary edible form, what we may call molar, in distinction from molecular fat—does not begin till much later, probably not in any great degree until puberty approaches; witness the almost universal distaste for fat of children, and, while appetite alone is not an infallible guide, yet so widely spread a distaste as this must have a physiological foundation. Thus the order in which the digestion of food-stuffs is developed is proteid, carbohydrate, and fat; while, from actual observation of dyspeptics, I can assert that, in disease, the last developed is the first to go, and that we have disappearance of the several functions in the reverse order, namely, fat, carbohydrate, and proteid.

(Obviously, the law will not apply in such a manner, when the dyspepsia is due to localised organic disease. For instance, carcinoma of the stomach, lessening or abolishing gastric digestion, will cause at least delay in the digestion of proteid matters, while starches and fats will be disposed of normally. Again, organic or other disease interfering with the salivary glands will first hinder the digestion of starches. Above, I refer only to ordinary functional dyspepsia. Further, the remarks must be considered to apply only to ordinary civilised peoples. Among the Greenlanders and Esquimaux the working of the "survival of the fittest" has led to a special facility for the digestion of fat in the form of blubber, and doubtless equally exceptional cases exist in other peoples, whose surroundings differ from those of ordinary mankind.)

The first thing a dyspeptic abhors is fat; he cannot digest it, but can exist without it. Most of his troubles, and especially his flatulence, come from the non-digestion and fermentation of starchy matters, but even late in his dyspepsia he can still deal with proteids, though the form of these may have to be modified and simplified—especially freed from extraneous indigestible substances. An interesting example of the inability of a dyspeptic to deal with fat, when his other digestive powers were in good order, came under my notice lately. Early in the year a patient consulted me for insomnia, which, I found, was caused by dyspepsia. Under treatment he soon, apparently, recovered, and was greatly pleased with the result. One evening, however, he dined at a French restaurant, and ate spinach, which he greatly enjoyed, digested well, and then passed a good night. The next evening he ordered his own cook to serve him with spinach, after eating which he had severe dyspepsia, a restless night, and came to me the next day, fearing that all his old trouble had returned. But the explanation was this—the French cook, in his national fashion, had served him with *epinards au jus*, that is, cooked in albuminous "stock," while the English cook, in her national fashion, had prepared her spinach with butter, and served it with cream. The patient's digestion was perfect for all other purposes, but he had not yet recovered the power of digesting fat.

I have said above, that to take the view of dyspepsia mentioned is of importance in treatment, and I must here make good this assertion. It is a recognised principle in therapeutics, that as far as possible rest should be given to a diseased part or function during the time that rest, alone or assisted by other measures, is effecting recovery. In dyspepsia, therefore, we should aim at giving the digestive organs rest, and this can be done in four ways—(1) By rectal feeding; (2) by completely predigesting the food; (3) by withdrawing the food-stuffs which cannot be digested; and (4) by supplying the digestive agents which are lacking. Rectal feeding, essential as it is in certain organic diseases, is rarely required in functional dyspepsia, and then only when the stomach obstinately rejects food, usually because of a neurotic condition. To Sir William Roberts the profession owes the perfection of predigestion of food-stuffs, but he himself has told me that he has now a difficulty in checking the abuse of this sometimes admirable remedial measure. The patient's appetite, in all but very obstinate conditions, ought to be considered, and peptonised foods are never very savoury or palatable. Moreover, a patient with functional dyspepsia must usually go about his duties, and then cannot well obtain a peptonised diet.

The third method is followed empirically by the so-called "Salisbury" diet, a system which has been introduced under circumstances which obviously must prejudice educated practitioners against it, and which cannot fail to do harm, if followed blindly without medical advice, as its first non-professional advocates recommended. But it has its merits: it gives relief, and can be, under proper supervision, beneficial and non-harmful. It was brought out as a mere rule-of-thumb procedure, but its *rationale* is clear from what is said above. The fats and starches which the patient cannot digest are withdrawn, and the proteid matters in the form of half-raw minced beef, the power of digestion of which still remains to some extent, constitute the whole of the diet. Rest is given for the disordered functions, and unassisted nature allowed to heal them. The patient can sometimes by such means be cured, "tute," but not "jucunde."

The fourth method of treatment is, I think, the pleasantest and best, and if we remember what I have stated above as to the manner in which the divisions of the digestive functions are affected, we can apply our remedies with precision. We can nearly always leave the albuminous matters to look after themselves, occasionally assisting their digestion by pepsine. Greatly to the comfort of the patient, the starchy foods can still be taken, if at the same time we administer one of the preparations of diastase now on the market. It seems to be demonstrated that the action of diastase is not abolished by a certain degree of acidity in the stomach, and the relief given to the dyspeptic by its administration is often marvellous. The fats cause more difficulty. A patient can very well do without them for a time; but if they are to be taken at all, it has seemed to me that the patient's craving gives the clue to treatment. A man who is "bilious," and abhors fat, asks for nothing better than a "lemon-squash" to refresh him and relieve his sickness; and I think lemon-juice diluted assists a dyspeptic to digest fat. Empirically, the cooks have recognised this principle in mixing vinegar with their salad dressing, and in serving lemon-juice with the liver of oysters. It may be remembered that the bile acids are the principal agents in causing emulsification of fats in the intestine.

Let us now turn to the consideration of alcoholic cirrhosis of the liver. A commonly taught view of this condition is that it is an inflammation of the tissue of the portal canals, bringing about an exudation of leucocytes, which change into fibrous tissue, and this contracting, in fact cicatrising, squeezes and destroys the liver substance. I think this view is wrong. If it were true, then there should be no appreciable destruction of hepatic cells, and consequent great disorder of liver function, in the early stages of the disease, and in fact the liver should go on acting well until its size begins to diminish. But very early in the history of the disease the preliminary over-action of the liver gives place to under-action, while the firmly-contracted small liver of cirrhosis, although undoubtedly it exists, is more often spoken about than seen. By far the majority of cirrhosed livers, even those examined in the post-mortem room, are enlarged—in fact, the destruction of liver function leads to the death of the patient before the liver has time to diminish in size. Again, the fibrous tissue formed in the liver undoubtedly does contract, as shown by the obstruction to the portal circulation, but, in my own opinion, this contraction is not the cause of the destruction of hepatic cells. Cirrhosis may also be produced by other causes than alcohol, such as biliary retention, malaria, and syphilis; and there have been described variations in the distribution of the fibrous tissue, which are supposed to differentiate the cirrhosis due to these causes from that produced by alcohol. Thus we have monolobular, multilobular, multicellular, hypertrophic, and biliary cirrhoses. I have never been able to satisfy myself of these differences, or, at any rate, of their being characteristic of any one kind of cirrhosis. My own observation would lead me to think that the distribution of the fibrous tissue is very similar in all the forms, although in alcoholic cirrhosis it shows a greater tendency to accumulate in the portal canals than in the other forms. The view of the condition which seems best is this. The alcohol absorbed from the stomach is a strong irritant to the liver, and at first stimulates it to increased action, at any rate in regard to its biliary function. Hence the efficacy of our hospital "white-mixture" in removing the effects of a debauch. It purges away the excess of bile. But this irritation soon diminishes the general vitality of the liver, just as the more prolonged action of alcohol diminishes the vitality of every other function of the body, without the production of any inflammation. Then let us apply the law of pathogenesis, and we see that the special hepatic tissue must be the first to fail, since it requires the most vitality for its existence. The framework of the liver, a low tissue, will remain, and the remaining vitality of the organ is expended in producing lowly organised fibrous tissue, instead of highly organised hepatic cells. The minute anatomy of the process also shows that the law is followed. In a cirrhosed liver one always finds a great excess of biliary ducts. This condition has been thought to be due to a mere laying bare of the ducts from atrophy of the other tissue, and again to a proliferation of the ducts, especially in the so-called biliary cirrhosis, but neither view is correct. Professor Dreschfeld, some years ago, showed that the liver cells, in addition to ordinary atrophic changes, altered their form, and, in fact, assumed the appearances of the cells of biliary ducts, clearly a reversion to their embryonic and more lowly vitalised condition. The most recent theory of the pathology of cirrhosis of the liver, that it is due to micro-organisms, is not incompatible with such a view, for Professor Adami supposes that the vitality of the liver cells becomes injured by alcohol before the micro-organism can take effect.

Blood-forming organs.—As regards the blood-forming organs, we may take as an example ordinary chlorosis, a disease found in pubescent girls, and show how this, too, conforms to the law. At the age of puberty, in both sexes, all the organs and functions are undergoing very rapid increase of their actions. The blood-forming organs are subject to great strain to provide blood for the highly-increased metabolism, and a general lack of vitality, from want of fresh air, insufficient exercise or food, overwork, or mental worry, will naturally tell most on the organ or function which has most work to do. The strain upon the blood-forming organs is the greater in girls, because of the loss of blood which accompanies ovulation, although nature so frequently relieves the system somewhat, by suspending menstruation for a time. This is an important point in treatment, for the amenorrhœa should be regarded as a protective condition, and no attempt should be made, as is often done, to force on the menses by emmenagogues. The worst cases of chlorosis to treat are those which are accompanied by menorrhagia. Here, then, we have an instance of what is, at the time, the most highly specialised system failing from general lack of vitality, while other systems suffer in only a minor degree. Such patients will recover, if you give them iron alone, that is, the material with which to form blood, but much more rapidly if you combine the iron with arsenic, a drug which stimulates the action of the blood-forming organs. But, at the same time, one must not forget the original cause—general lack of vitality—and remove this by good food, especially meat, fresh air, and general hygienic measures.

Urinary system.—Numerous researches have been made upon the chemistry of gout; but valuable as all these are, and however important it be to thoroughly understand the relationships of uric acid, biurates, quadriurates, guanin, xanthin, and so on, and the effects of diet, and the presence of other salts upon the forms of these compounds, yet behind all there is the main fact that gout and its congeners are due at one time to the precipitation of uric acid in one or other of its forms from solution, and at another to the accumulation, either from excessive production or deficient elimination, of uric acid in solution. For clinical purposes, one must separate the precipitation of uric acid out of the urine into the urinary passages, from the same precipitation into the tissues out of the blood or lymph. But the precipitation, the insolubility of the uric acid form, is the same in each case. Here, again, I would maintain, we have an instance of involution. The serpent of low vitality has an animal diet, and passes as its urine a solid mass of uric acid (using the term in its general sense). Its normal urine is a calculus. The fowl of higher vitality, feeding on grain, passes a pultaceous mass of uric acid, that is, a normal urine consisting of uric acid gravel. The native of India, a man of low general vitality, succumbing easily to acute diseases, old at 40 years of age, and feeding on grain, suffers greatly from stone, as our army surgeons can tell us. The Londoner, a man of considerable vitality, living usually to 65 or so, the most highly meat-fed civilised person on the earth, as far as statistics can show, does not suffer greatly from stone. The poor Irish peasant of enormous vitality, living till he is 90 (in the absence of the national accidents), not infrequently reaching 100 years of age, and rearing a numerous family, lives on potatoes and buttermilk (he cannot afford to eat the butter), only seldom drinks alcohol, and hardly ever suffers from stone. Obviously, it would be wrong to say that this is the whole of the argument, or even a fair statement of it, but it serves to show that the deposit of uric acid from the urine is a normal condition in the animals of low organisation, and occurs, as a reversion to such a state, in the higher animal, when his vitality, general or local, becomes deficient. The known facts at our disposal in comparative physiology are not yet sufficient to warrant one in applying the same reasoning to the phenomenon of precipitation of uric acid from the tissue fluids, or its undue accumulation in them, but the analogy is very pointed, and might well be borne in mind by those who are engaged in the investigation of gout and other toxæmias. An autogenic toxæmia may be a reversion to a more primitive state, while a toxæmia introduced from without is a noxious agent, leading to a diseased state by destroying vitality, with or without the intervention of chemiotaxis.

The granular kidney of Bright's disease, and its associated circulatory and tissue changes, is essentially due to a lack of vitality, at first not affecting the whole body, but leading to the growth of a lowly organised tissue, and the decay of the highly organised. In this country, at least, we no longer consider the granular kidney to be an interstitial nephritis, bringing in its train the circulatory phenomena. We know that an obscure blood change is the primary condition, and that this brings about a growth of lowly organised fibrous tissue in the blood vessels, heart, kidneys, and other organs, and the disappearance of the highly specialised tissues of these parts. The kidney change is not essential, and, as I have more than once found, death may occur from the general condition before the kidneys are affected at all. We have in fact a lack of vitality neither altogether general nor altogether local, and bringing retrogression in its train.

But the overgrowth of fibrous tissue may be purely local in some forms of Bright's disease. We have, for instance, the white contracted or granular kidney, and students have difficulty in understanding the relationship of this to the form above mentioned. But for its explanation, we must turn to what has been said above on the subject of general pathology. Here we have a definite cause, nephritis, which has injured the vitality of the kidney, and led to the growth of a lowly organised fibrous tissue instead of the highly organised kidney structure. We have evidence, too, of the manner in which the vitality of the kidney cells is affected, in the known method of production of casts of the tubules. In the early stages these are hyaline, and formed, at any rate in part, by hyaline globules, which can be seen in process of formation in the cells of the convoluted tubes, as described by Cornil. They show that the metabolism of the renal cellular protoplasm has been injured, and less specialised hyaline material is found instead of healthy protoplasm. Further, in both these forms of kidney change, we can frequently see in advanced cases, that the highly specialised and active cells of the convoluted tubes, having lost the amount of vitality necessary to maintain them in such condition, assume a small cubical form, and come to resemble, in fact, the epithelium of the almost inactive and more lowly organised collecting tubes,—a statement which may be compared with what has been said above of the involution of hepatic cells in cirrhosis to the type of those of the biliary ducts.

Respiratory system.—Nearly all the diseases of the respiratory system with which we have to deal, have their starting-point in an inflammation. Whatever may be the idea of inflammation in other parts, there can be no two opinions on this point—that here the process of inflammation is accompanied by lack of vitality of the part, and also, because of the peculiar dynamic relations of the organs, leads to further changes. It may be that the noxious agent, if left alone, would cause still more serious damage, but nevertheless the inflammatory process itself causes injury. Nevertheless, we have some diseases of the lung which occur in the absence of inflammation, of which emphysema is one. Here, as the result of undue strain, or deficient nutrition, the vitality of the lung tissue is so much injured that it is unable to bear the pressure produced by voluntary or involuntary expiratory efforts, and so expands at the points which are least supported. The same thing occurs when the vitality of the tissue is injured by inflammation, as in the emphysema which follows pneumonia and the bronchiectasis, an analogous condition, which follows repeated attacks of bronchitis.

The accumulations caused by inflammatory exudation, either in the substance of the mucous membrane of the bronchial tubes, or in the lumen of the air passages, obstruct the free entrance of air, and so cause dyspnœa. I have elsewhere endeavoured to show that such a mechanism forms really the pathology of asthma.

The various lesions of tuberculosis are due to the fact that the tubercle bacillus injures the vitality of the lung itself directly, and also indirectly by the inflammation which it causes, or, if we adopt the newer view, which is called forth by chemiotaxis to resist the bacillus. The substances which are produced by such lack of vitality are pus, caseous material, calcareous deposit, or fibrous tissue, all of lower organisation than the highly developed lung tissue whose place they take, while at the same time the chemical substances produced by the tubercle bacillus, or other germs which accompany it, being absorbed, injure the vitality of most parts of the body, causing the fever, emaciation, and asthenia of the disease. The lesions produced in the lungs by other germs, such as the Bacillus Anthracis, Actinomyces, and Aspergillus, can be viewed in the same light.

Circulatory system.—The diseases of the circulatory system in their clinical aspect are nearly all the consequences of inflammation or degeneration of the circulatory organs, which renders them incapable of performing their work, or unable to resist the internal strain involved by that work. The inflammation or degeneration itself obeys the laws already laid down in the paragraph on general pathology, and has, as there mentioned, as its basis, a lack of vitality of the specialised tissue, leading to the substitution of a more lowly organised fibrous or fatty tissue. Instances of this we see in valvular inflammation, leading to destruction of special tissue, with or without cicatricial contraction, and fatty or calcareous degeneration of the inflammatory accumulation. In consequence of this, we have stenosis of the cardiac orifices, or regurgitation through them. Pure degeneration (using the term in its wide sense), as in the fibrous overgrowth in the cardiac muscle in Bright's disease along the course of the cardiac vessels, and again the pure substitution of fibrous tissue for cardiac muscular tissue in Martin and Huchard's dystrophic sclerosis of the papillary muscles, are further instances of the results produced by deficient vitality, in destroying the true tissue of the part. Similarly, atheroma of the arteries is a fatty degeneration, and endarteritis obliterans an inflammation of the vessels, both bringing about the substitution of an inferior tissue for the true tissue of the vessel wall. Both in the heart and in the vessels the inferior tissue is not only unable to do the work of the part, but is unable to stand the pressure of the blood stream and to maintain its tension. Consequently, we have a giving way of the walls, taking the form of dilatation of the heart, aneurysm of the arteries, and varicosity of the veins, but all due ultimately to lack of vitality of the original tissue of the walls. Even without organic disease, a very slight diminution of the vitality of the cardiac and arterial muscles brings about a similar result, as in the acute dilatation of the heart of fevers and general malnutrition; while mere functional weakness of the arterial coats can cause the low arterial tension of convalescence and similar states. The influence of stenosis and regurgitation through the orifices, in causing dilatation of the cardiac cavities and hypertrophy of their walls, will, of course, be obvious. Then we have widely spread diminution of vitality, produced by deficiency in the supply of blood and its nutrient constituents, and consequently symptoms are caused by failure of other organs, in accordance with the scheme of the law.

In congenital affections of the heart, particularly the most common clinical form of these—imperfect septum ventriculorum—we find the lack of vitality working in two ways. Thus, the cause of an opening in the ventricular septum may be either intra-uterine disease or deficient developmental power. In the first, the lack of vitality is shown by an inflammation and consequent stenosis of the pulmonary orifice, the disease attacking the side of the heart which is the more highly developed—the right side and not the left side, as in extra-uterine life. Consequent upon this we have increased tension in the right ventricle while the septum is forming, and a hindrance to its perfect growth. But the same defect is sometimes produced without intra-uterine disease, by simple lack of vitality, which allows of the development of the septum from the apex upwards only to a certain point.

Cutaneous system.—There is so little known at present of the pathology of skin diseases, that it is impossible to generalise upon them. Two instances, however, may be given, in which this law of pathogenesis holds good, and probably the specialists in skin disease may be able to add to these. Seborrhœa sicca of the scalp is due to lack of vitality of the sebaceous glands, leading to diminution of sebaceous fat and the formation of dry imperfect scales, instead of the more highly developed hair. Psoriasis, too, appears to me to be a similar instance of lack of vitality, even though the result is a heaping up of cells. It may be, and is often, accompanied by inflammation, but this is a secondary result. In the first instance, there is a dryness of the cells and diminution in the amount of their fatty matter. The accumulation of the cells is in itself an evidence of lack of vitality, as showing that the cells have no longer the power of full development, and of casting off their effete companions. Such a view, too, explains the primary localisation of psoriasis on extensor surfaces. It is a necessity provided for by nature, probably through the principle of the survival of the fittest, that the parts on the extensor surfaces of the body should be less vital than those on the flexor surfaces, since they are, in the lower animals at least, the more exposed to injury. Consequently, a condition caused by lack of vitality would be more likely to present itself first in those parts whose energy was the less, and spread later to those whose energy was greater. An inflammatory affection like eczema, which is more common on the flexor and more vital surfaces, would have a different pathology (as discussed above), but still involving a primary loss of vitality. There is nothing, in the view of the pathology of psoriasis mentioned, inconsistent with the well-known fact that psoriasis is often at its worst when the patient is otherwise in the best of health, and may disappear when the general health begins to fail, for, as stated in the law, the lack of vitality may be general or local.

Such, then, are instances, selected almost as widely as possible, which seem to conform to this law, and they could be greatly multiplied. It must not be ignored, however, that other cases present themselves, whereof the explanation is not so easy. This, however, may be due to our deficient knowledge of the details of their pathology, and such a view becomes the more probable, the greater the number of cases which can be shown to conform to the law. I submit that the view stated above, even if provisional, may at least usefully serve as a peg upon which to hang the details of our knowledge, until some better generalisation appears.

THE VIS MEDICATRIX NATURÆ.[1]

By W. B. Ransom, M.A., M.D., F.R.C.P., *Physician to the General Hospital, Nottingham.*

(Concluded from page 24.)

We have now passed in review a series of facts, showing how far the highest animal organism (man) retains the adaptability and power of recuperation, functional and anatomical, which we found to be characteristic of living beings generally.

But there is one other factor influencing growth whose importance claims our attention. I refer to the peculiar, almost mysterious, influences exerted on the whole body by the functional activity of certain organs, such as the genital and thyroid glands, a group of phenomena sometimes discussed under the head of "Correlation of Organs," or somewhat fancifully by the German pathologist Hansemann as "Altruism of Cells," but of recent years partly explained by the hypothesis of "internal secretions."

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[1] An Address delivered before the Medical Society of University College, London.

That some organs are necessary to the well-being of all the others, is obvious: without the stomach to absorb food, and the heart to distribute it, growth and activity must cease. But it is not so obvious why the growth of the hair on the face or the pitch of the voice should depend on the integrity of the testes; or why the whole development of body and mind should remain permanently infantile when the thyroid is absent and undergo degeneration when it atrophies; why strength should ebb away and pigment darken the skin when the suprarenal bodies are diseased; why the body should melt into sugar when the pancreas is excised, or into urea when three-fourths of the kidneys are removed.

It is clear that the various members of our body influence one another, though possibly in different ways.

In the case of the thyroid gland, we know that it secretes a substance or substances powerfully affecting metabolism, the administration of which causes cretins to develop in body and mind, dissolves the mucin that clogs the tissues of patients with myxœdema, increases animal warmth, stimulates the hair and skin, reduces fat, and generally excites to active metabolic changes. We have also some evidence that it prevents the formation of or destroys certain poisons in the blood,[1] which accumulate when the thyroid is removed, and cause cramps and acute nervous disturbances, and that the antidote to this poison (thyreo-antitoxin, Fränkel) is distinct from the substance (thyro-iodine, Baumann) which prevents myxœdema. It is further possible that the thyroid exerts an antitoxic effect on other poisons; for instance, it shows signs of increased activity in certain forms of grave jaundice (Hürthle, Lindemann) (*e.g.* also caffeine poisoning).

Similarly, there appears to be a poison in the blood when the suprarenal bodies are removed, and, as Schäfer and Oliver have shown, these glands contain a substance which raises the blood pressure by its powerful vasoconstrictor influence. What, however, is their normal action, and what the change in Addison's disease, is still uncertain. We can only surmise that some internal secretion helps to maintain the body's health.

In the case of the pancreas, we know that removal of the whole organ almost invariably causes diabetes, which is prevented by grafting a piece under the skin; and clinically diabetes may be associated with diseases of this organ. Again, some internal secretion seems to be postulated, though its nature and mode of action are still matters of speculation.

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[1] Notkin, *Virchow's Archiv*, Bd. cxliv.

The same may be said of the kidneys, since the important researches in which Bradford showed that removal of the greater part of the renal substance of an animal leads to a vastly increased nitrogenous metabolism, as evidenced by wasting, polyuria, and great increase in the excretion of urea. Here we have an organ whose beneficial action may be twofold—on the one hand, an influence moderating nitrogenous metabolism of the tissues generally, and, on the other, the power of excreting from the body poisons manufactured within or introduced from without.

In the liver we have also probably an internal secretion of sugar and of urea, if not of other bodies, as well as the elimination of various effete products by the bile.

But the liver is of especial importance as a neutraliser or checker of poisons introduced from the digestive tract, and, to some extent, of those circulating in the systemic blood. Thus it is well known that morphia acts much more powerfully injected hypodermically than when swallowed, and it has been shown experimentally that curare can be injected into a mesenteric vein without ill effect in a much larger dose than into the jugular; that if the portal vein be connected with the inferior vena cava (Eck's fistula), and the portal blood sent through the latter, a much smaller dose of atropine will kill than when the vena cava is tied above the fistula and all the blood sent through the liver. The effect of simply throwing the liver out of the circulation by the establishment of such a fistula and tying the portal vein in dogs, is to cause the gradual onset (ten to thirty-five days) of nervous excitement, fits, ataxy and coma, these symptoms being increased by a meat diet. Urea then nearly vanishes from the urine, being replaced by carbamic and other amido-acids, which the liver should have caught and turned into urea. In advanced cirrhosis of the liver in man, strychnia swallowed passes much more readily into the urine than in a healthy person.

According to Roger, the liver actually destroys alkaloidal poisons, while it stores up metals, such as iron (as may be seen after hypodermic injection), copper, arsenic, and mercury, and to some extent excretes all kinds.

It is clear, then, that the liver is of great importance, both from its internal secretions and its antitoxic power. We may truly answer the query, "Is life worth living?" with "That depends on the liver," or "Question de foie."

One need not go wholly with that modern school which views the alimentary canal mainly as a laboratory for the production of poisons, and seems to think "auto-intoxication" the chief business of our bodies; but it must be admitted that unwholesome substances do at times get into our portal and systemic circulations, and it should be recognised that the liver is an important means of neutralising or getting rid of such *materies morbi*.

Its internal secretion of urea probably also directly stimulates the kidneys to excrete poison, since, as Klemperer and others have shown, this substance has a powerfully diuretic effect.

Of the internal secretions of the stomach and bowels we know practically nothing, though, judging by the psychological effects of gastric derangement, we may surmise that they are not unimportant.

As excretory organs, however, they play their part. Witness the excretion of morphia by the stomach and of iron by the bowels (Gottlieb, Jacobi), as well as the daily domestic experiment of the salutary influence of a dose of salts.

We may also note here the antiseptic effect of the digestive secretion, more especially of the gastric juice, shown by the fact that cholera bacilli only affect certain animals when its acidity has been neutralised. Of the definitely antiseptic effect of the bile and intestinal juices there is little evidence, but they may be more or less favourable to bacterial growth. The bowel contents are always full of varied germs, some of which flourish normally, others only during enfeebled conditions of the mucous membranes and their secretions. Rats may swallow typhoid bacilli with but little harm, till their system has been exposed to the volatile poison of sewer air, when they succumb. In many bowels live for a time the bacilli of typhoid, cholera, and tuberculosis, without producing those diseases, just as clover seeds abound in the soil of every field of grass. When the farmer wants a clover crop, he may sow not clover, but phosphates, and the hitherto latent seeds shoot above earth. The clover with its symbiotic root microbes catches the free nitrogen of the air, enriches the soil with nitrates, and prepares the way for another crop which before would not grow. Similarly, but reversely, with the bowels, the physician cannot extirpate the clover seeds, he cannot kill the germs in 26 ft. of intestines without killing the owner of those intestines—intestinal antisepsis is a figment—but he can influence the soil, and by food or drugs and mode of life maintain a vigorous epithelium and a healthy secretion. He can withhold the manure which fits the soil for a series of abnormal vegetations. In regard to no organs is their influence on the whole body more marked than in the case of the genital glands—testes and ovaries. In youth the growth of hair, the change in the vocal organs, muscular and mental activity, depending on the development of the one; the enlargement of the mammary glands, the alterations in the bones, and the deposition of subcutaneous fat, associated with the activity of the other, are alike wonderful. Towards the other end of life, the removal or atrophy of either leads to associated atrophies of enlarged prostate or uterus and of the normal female breast. From recent observations, it would seem probable (Beatson, Stanley Boyd) that removal or atrophy of the ovaries tends sometimes to cause arrest or even atrophy of carcinoma of the breast, showing an influence even on cells which are half independent of and almost parasitic on the general bodily structure. No wonder that "vitality" is popularly associated with sexual activity, and that even a man of science like Brown-Séquard sought the elixir vitæ in the juices of the testicles. Beyond the facts mentioned we can go but little further. Brown-Séquard's startling theories have passed away with him into the void. In spite of enthusiastic pupils and of melodramatic drug-dealers, the proof is lacking that testicular substance has distinct physiological effect. It neither gives a beard to the eunuch, nor enables the tabetic to dance. With regard to the ovaries, the evidence is likewise lacking, though, thanks to the enterprising chemist, the docile doctor dispenses regularly "ovarian tabloids." One remarkable fact, however, deserves mention, namely, that removal of the ovaries appears to arrest and cure that grave softening of the bones known as osteomalacia.

To sum up, we may say that the genital glands exert a powerful influence on bodily vigour, and that their atrophy may for some diseases be a natural cure; but the hypothesis of an internal secretion, though probable, is as yet for them not proved.

Viewing this part of our subject as a whole, we find it proven that the functional activity of individual organs influences the metabolism of the whole, sometimes stimulating to growth, and probably sometimes antagonising or removing poisons, whether of endo- or exogenetic origin; and we may take it as certain that in the combat of the organism with living agents of disease these influences play a part.

To the study of this combat we now turn.

That living organisms, mostly vegetable, cause a large number of diseases, is now a familiar fact. That practically all inflammations and most febrile conditions are due to the invasion of our bodies by bacteria, though a recent, has become a fundamental, doctrine of the medicine of to-day. Inflammation and fever, then, are the common sequels of microbic attack. What do they mean?

I need hardly remind you that fever, though occasionally due to damage of the heat-regulating mechanism in the brain, usually indicates increased combustion of the tissues, caused by a poison circulating in the blood. Fever usually means increased destructive metabolism. Similarly, you are aware that an inflamed part is warmer and redder from an increased blood supply, caused by dilatation of all the smaller blood vessels; that liquor sanguinis, leucocytes, and often some red corpuscles, exude from the capillaries and cause swelling and pain. Chemical and anatomical changes, then, are the result of, and may be interpreted as the response to, the entrance of microbes into the system. Each is a vital reaction, not a passive effect. Each must have its influence on the invading organisms. But to what end? Does it help or hinder? Is it part of the *Vis medicatrix naturæ*, or is it life running riot?

It has ever been the tendency of man to see an object or aim in all things, to discover a final cause, and assume an obvious usefulness in all structures and in all functions. So has it been with fever, which Hippocrates looked upon as merely the increase of the "vital principle" (animal heat), Sydenham as the "instrument by which nature separates the impure from the pure parts" (though he admitted that nature might overdo it), and Stahl as the chief agent of the vital spark or soul, carrying his doctrine so far as to say that animals having no souls could have no fever. These pious beliefs suffered a rude shock when Leibermeister showed the serious damage done to the system by the mere rise in temperature, and the striking benefits produced by the artificial reduction thereof, so that it had to be admitted that fever was at least not an unmixed blessing.

In this post-Darwinian age we do not adopt the teleological point of view of preceding centuries, and we see no more *necessity* for a useful purpose in the fact that fibrin ferment or bacterial toxins produce fever, than in the fact that phosphorus may produce degeneration of the liver; but we are prepared to admit the *possibility* that a rise in temperature may under certain conditions be of use to the organism, and that the law of survival of the fittest may have led to the existence of a race of readily feverish beings. It is a matter not for *a priori* speculation, but for investigation and experiment.

Now, it has been abundantly proved, by experiment and clinical observation, that a continued very high temperature is injurious to the system, but it has not been shown that fever is altogether harmful, though it has not been definitely proved to be productive of good.

We see daily in hospital the benefit of reducing a febrile temperature by the application of cold, but, on the other hand, there are grounds for believing that some pathogenic germs are weakened by temperatures which the animal bodies may reach (*e.g.* pneumococci, tubercle bacilli); and experiments on dogs,[1] in whom fever had been produced by puncture of the corpus striatum, showed that these animals offered more resistance to bacteric infection (pneumococci, fowl-cholera, swine-erysipelas) and to bacterial toxin (diphtheria) than did normal animals. It may therefore be that, while in very great or prolonged rise of temperature the injurious effects of the pyrexial process may be disastrous to the body, yet fever may be helpful in counteracting the activity of pathogenic germs.

Inflammation also has been assumed to be a beneficial reaction,

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[1] Lœwyt and Richter, *Virchow's Archiv*, Bd. cxlv.

"as the local attempt at repair of injury" (Adami); a definition which clearly includes too much, as it includes regeneration of tissues, just as do the definitions which include the necrotic or destructive changes following an injury.

We shall see, however, that there is great force in this conception, which we owe largely to Metchnikoff, at any rate as regards animals; but it is not universally true. Thus in plants the bodies well known as galls, of various kinds, are inflammatory swellings produced by the irritation of developing ova of insects, or by acari or parasitic fungi, in the growing tissues of bud or leaf or root. But the whole result of the cellular hyperplasia is beneficial to the parasite, and not to the host. The former is provided with food and home, each adapted to his particular needs, growing as he wants it, and finally, in some cases, dehiscing of its own accord, to let him out when sufficiently developed.

As my father pointed out to the British Medical Association in 1892, in these cases the reaction to irritation is a cellular proliferation directed by the irritant, is specific, and is not beneficial to the organism itself, in the sense of destroying or removing the irritant. In other cases, fungoid growths pervade the tissues of the tree or plant, and lead to its decay without producing any cellular reaction. It may be that the hyperplasiæ described as galls do benefit the plant by supplying abundant nutrition to the parasite at the point of attack, and thus preventing invasion of the whole system.

There seems, however, to be a broad difference between plants and animals, in that the former do not possess regular excretory organs for the removal of poisons and effete products, and are not provided with a mechanism for expelling a living virus.

Turning, however, to the animal kingdom, we find from the lowest to the highest a very constant series of phenomena following the attack of bacteria. We find the amœba engulfing them, surrounding them with a vacuole, and finally digesting them; we find in sponges, worms, molluscs, and arthropods, the amœboid cells of the blood or lymph acting like so many amœbæ and digesting or encapsuling the invader.

In Vertebrata, in non-vascular areas, such as the rabbit's cornea or the tail-fin of the embryo axolotl, we find wandering cells crowding to the point of attack and ingesting foreign particles, while the fixed cells of the tissue come but later into action in the process of regeneration.

In vascular tissues, such as the tail of the frog tadpole, we find the same process, only with mobilisation more complete. Dilated arteries and capillaries bring up hosts of leucocytes, which with some blood plasma pour out of the capillaries and eat up the microbes or die in the attempt. The title *phagocyte*, or cell-eaters, given to these wandering cells by Metchnikoff, is thus justified by a broad basis of fact.

We cannot now dwell upon the different forms of leucocytes met with in the higher animals, nor discuss in detail their several functions, but may note that some do and some do not appear to act as phagocytes, and that also certain fixed cells, such as the endothelial cells of capillaries, have the power of ingesting microbes.

The congregation of leucocytes to a spot invaded by bacteria is comparable to the movements of myxomycetes towards a decoction of dead leaves, and appears to be due to the influence of chemical products exuded by the germs either when living or dead. To this phenomenon the term positive chemiotaxis has been applied, and it has been found that various substances produced in cultures of bacteria, or extracted from their dead bodies, exert this effect when introduced into the tissues, or cause a general leucocytosis when injected into the blood.

Other substances which do not attract, or even repel, the leucocytes are said to be negatively chemiotactic. Thus very virulent bacteria or their products may not attract the leucocytes; and the further remarkable fact appears to be established, that though in animals highly susceptible to certain germs, the leucocytes may not eat them up, yet when rendered artificially immune they may do so. Thus if we inject the vibrio Metchnikovii into the ear of a guinea-pig, though redness and swelling occur, there is hardly any diapedesis of leucocytes, and the animal dies; but if it has been previously vaccinated, diapedesis and phagocytosis occur, and life is saved.

These and other facts have led to the well-known theory of Metchnikoff, that animals resist infective disease chiefly by means of their phagocytes, which enclose the microbes, and either kill or digest, or at least encapsule, and thus keep them harmless. Some observers also think that some of the leucocytes (*i.e.* coarsely granular oxyphile, or eosinophilous) excrete substances which destroy the bacteria without actually ingesting them (Kanthack and Hardy).

The case is, however, by no means so simple, as further facts will show. Thus anthrax bacilli, injected into a rabbit's ear, (Nuttall) can be seen to degenerate while lying bathed in lymph and before their ingestion by phagocytes, and the serum of blood outside the body has been found to be destructive to many microbes. The idea, therefore, arose that immunity, natural or acquired, was due to the bactericidal action of the blood and body fluids. A humoral as opposed to a cellular theory was established. It was, however, soon found that the antiseptic power of the living blood plasma was vastly less than that of the serum, and also that this power by no means coincided with immunity, natural or acquired. Thus rabbits are very sensitive to anthrax, though their serum readily kills the bacilli, while diphtheria bacilli grow well in the serum of animals rendered immune to this disease.

Neither is it probable that the "defensive proteids" of Hankin, nor the "alexins" of Buchner, supposed to be secreted by the leucocytes, play an important part in the defence. Their potency is but slight, and their quantity depends on the death rather than on the life of the cells. Thus an aseptic leucocytic exudation may become only distinctly bactericidal after repeated freezing and thawing.[1] The substance in typhoid blood (not alexins) which paralyses the bacilli is as abundant in the cell-free plasma, separated by filtration (after addition of leech extract), as in the part containing the corpuscles.[2] Anthrax bacilli will grow on a jelly made of thymus or lymphatic gland. It is asserted, however, by other observers,[3] that serum to which leucocytes from a staphylococcus abscess have been added, is much more destructive to staphylococci than untreated serum. That the fluids of the blood do, however, exert an important influence, is proven by the group of facts brought into prominence by the so-called "antitoxic" treatment.

It had long been known that one attack of certain diseases renders an animal more or less immune to subsequent attacks, and it was found that in many instances a similar immunity is produced by the repeated injection of minute but gradually increasing quantities of pathogenic bacteria, or of bacteria of gradually increasing virulence, or of the toxins produced in their growth. Animals so treated may become resistant both to the bacteria and to their toxins, though not always in an equal degree (active immunity). Now the further remarkable fact was discovered, that the blood or serum of animals thus treated, when injected into another animal, confers a similar immunity on that animal (passive immunity of Ehrlich). And as it was found that mixing such a serum with a bacterial toxin (*e.g.* of tetanus or diphtheria) renders the toxin harmless, Behring concluded that it acted as a chemical antidote, and spoke of it as containing "antitoxin." This view has received support from the experiments of Ehrlich with ricin, the toxalbumin of ricinous seeds, who by repeated injection of small doses immunised animals against ricin, and then found their serum protect others against that poison. He further found that ricin added to blood outside the body causes "balling" and precipitation of the red corpuscles, and that the antitoxic serum, added in proper proportions, prevented this effect, and that the poisonous effect was at the same time stopped. He therefore concludes that we have here to do with a definite chemical antidote. The same view has been adopted by Fraser in regard to snake poison and antivenomous serum.

Further facts, however, indicate the insufficiency of this theory, and show that immunity does not necessarily depend on antitoxic blood.

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[1] Schattenfroh, *Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, March 1897.
[2] Achard, *ibid.*, Jan. 1897.
[3] Van der Welde, *Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, Jan. 1897.

First, the blood of naturally immune animals has no such power, until the toxins have been injected into it.

Secondly, acquired immunity may persist after the antitoxin has disappeared from the blood.

Thirdly, a mixture of toxin and antitoxic serum which is inert to one animal may kill another (mouse, inert; guinea-pig, lethal).

Calmette also found that if a harmless mixture of snake venom and antivenin be heated to 80° C., the toxic power returns. From which he infers that the venom cannot have been neutralised chemically. The force of this last observation has, however, been weakened by the lately published experiments of Martin and Cherry, who find that if the venom and antivenin be left a sufficiently long time in contact with each other, it is no longer possible to separate them by filtration through gelatin, a substance which allows the passage through it of toxin and venom but not of antitoxin or antivenom.

The first three facts are, however, sufficient to show that blood or serum of an immunised animal does not necessarily protect by acting as a chemical antidote to the bacterial poison, any more than it necessarily acts as a killer of the germs themselves. The blood need be neither antitoxic nor bactericidal.

On the other hand, a series of researches on cholera vibrios, the typhoid and the coli communis bacilli, have pointed to a paralysing and probably destructive action of the body fluids on these germs under certain conditions. Thus, if virulent cholera germs be mixed with the serum of an immunised animal, and then injected into the peritoneal cavity of a guinea-pig, they are found in a very short time to be massed together into clumps and to be breaking up into granules; and it seems to be proved by a number of observers, that this disintegration of bacilli (cholera, typhoid, or coli communis) occurs independently of a leucocytic exudation, and at a time when the wandering cells of the peritoneal fluid are actually fewer than normal. Metchnikoff himself has to admit the presence here of a bactericidal fluid, although he holds it to be secreted by the leucocytes. And it has further been shown that the blood serum of animals immunised against cholera or typhoid bacilli by the intraperitoneal injection of the dead bodies of these germs, or of patients suffering from typhoid fever, has the power outside the body of inhibiting the movements of the bacilli, and making them run together into motionless clumps—the so-called "agglutinative action." The bacilli appear not to be killed, but are certainly paralysed and weakened. In this agglutinated state, in the peritoneum, they are readily taken up by the phagocytes of the omentum, and when in the peritoneal cavity also die in the fluids. Gruber and Durham therefore hold that by the process of immunisation a specific "protective substance" or "agglutinin" is formed, which renders the microbe susceptible to the destructive but non-specific action of the alexins of the body, and they hold that both active and passive immunity against cholera and typhoid depend on these agglutinins. Pfeiffer, on the other hand, thinks that an actually specifically bactericidal anti-body is in each case developed.

All observers, however, find that both anti-cholera and anti-typhoid serum is not antitoxic; it does not protect against the toxins of these germs—a marked contrast to diphtheria serum.

Nor does the existence of a marked agglutinating power in the blood at all indicate immunity, as is frequently found in severe and fatal cases of typhoid fever. As Wassermann[1] has shown, bactericidal serum may be injected and remain present in large quantity in animals previously infected with *B. pyocyaneus*, and yet the animals die.

At present we by no means fully understand the action of the body fluids on typhoid and cholera bacilli, nor their power in combating those diseases or conferring immunity, but the facts do indicate the existence in them of substances harmful to the germs, though their origin and nature are as yet obscure.

Summing up, we find the animal body responding to the varied methods of attack of microbic foes with a varied and complicated mechanism of defence. We find a number of wandering cells under certain conditions attacking and slaying the invading germs; we find under other conditions the body juices paralysing and disintegrating them; and we find, under yet other circumstances, the blood rendering the bacteric poisons of no effect, although it does not appear to act as a chemical antidote. And yet we have found immunity occur without phagocytosis and without antitoxic or bactericidal action of the body fluids. Clearly something more is needed. Disease is not combated by leucocytes or blood alone.

An interesting research of Cobbett and Melsome has shown the possibility of a local, as opposed to a general immunity. The ear of a rabbit which has once been infected with erysipelas makes short work of a second attack of the streptococci, although the rest of the body has no increased resistance. Here the tissue cells must have undergone change, become hardened to the toxic influence, acquired more vigorous reactive power. And it is to the cells of the body generally that we must look for a complete explanation of immunity to the attacks of pathogenic germs. Upon their insusceptibility and upon their power of accommodation and vital reaction will recovery depend. We have already considered the marvellous adaptability of living matter and the extent to which it can accustom itself to influences at first injurious. Toleration is acquired of substances previously poisonous (*e.g.* arsenic, opium), and the reserve or potential energy of the living cells develops new force.

The case of muscular exertion and fatigue illustrates this point.

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[1] *Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, 9th June 1897.

To perform a certain amount of hard work, say a thirty-mile march, the tissue combustion actually needed is the same whether the subject be in good or in bad training. The effect, however, on the subject is notoriously different. At first the marcher is poisoned by waste products to which he later becomes indifferent, and (as Dunlop, Paton, and others have shown), to get through the same amount of severe exertion, the untrained man breaks down more proteid than the trained, he excretes more uric acid, more extractive nitrogen and more phosphorus. As in the course of training functional hypertrophy of muscles develops, the work is distributed over a larger amount of good contractile substance, and although the same essential katabolic changes occur, there is not now a concomitant wasting of nucleo-proteids. Moreover, the combustion products produce less effect on the system, and probably are more efficiently removed by the excretory organs. We have here an acquired insensitiveness and an increase of functional activity to meet increased need.

Something like this we may conceive to occur during the attack of the organisms of disease. The body cells become less sensitive, their reactive powers stronger. Special mechanisms, such as the army of phagocytes, share in this reaction, but it is not confined to them, nor can an altered composition of the blood be due to the lymphoid organs alone. Probably the internal secretions we have before discussed play their part in neutralising toxins and in rendering the body fluids less suitable for bacteric growth.[1] The excretory organs also are important helps in getting rid of toxic substances. The increased toxicity of febrile urine is a well-known fact.

Upon the potential energy and reactive power, then, of the whole body depends recovery from infective disease. Rarely can we aid with drugs. The surgeon may empty an abscess and remove the infective agent, but the best physician is he who does no harm. Scarlet fever, pneumonia, typhoid fever, tuberculosis, all are inaccessible to drugs; all may get well by the healing power of nature.

The *Vis medicatrix naturæ* is not, as has been recently said, a figment of the imagination, but it is one aspect of the *Vis vitæ*, one ingredient of life. Let us then beware how we meddle with or hamper the manifold development of life. It is not by protecting from external conditions that we strengthen vitality, the essence of which is adaptability to change. Change is life-giving, uniformity brings decay. We are, physically at least, both in the world and of the world, and by nature responsive to its varying phases, by nature endowed with a vast reserve of energy for unwonted struggles. To live healthily and to live long we must live habitually within our reserve, but we must also keep our reactive powers in training.

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[1] Kondratieff has protected mice from tetanus by the injection of a substance (enzyme ?) extracted from the suprarenals and spleen of normal horses (*Centralbl. f. Bakteriol. u. Parasitenk.*, Jena, March 1897).

Facing, then, fearlessly the elements among which we are born, we raise our resistance even to violent changes, converting them into healthful stimuli, and we train the living matter of our bodies in that reaction and regeneration on which we must ultimately depend, and which we have called the *Vis medicatrix naturæ.*

A RESEARCH INTO THE BACTERIOLOGY OF TYPHUS FEVER: PRELIMINARY NOTICE.

By ANDREW BALFOUR, M.D., D.P.H., and CHARLES PORTER, M.B., C.M.

THE bacteriology of typhus fever is a subject which has received but scant attention. Within late years only four researches in this direction have been conducted, that by Thoinot and Calmette, those by Hlava and Lewaschew in 1891 and 1892 respectively, and that by Dubief and Bruhl in 1893. The first-named investigators described bodies which they found in the blood of typhus patients, and which somewhat resembled in shape human spermatozoa. Their observation has never been confirmed, although Dubief and Bruhl in their research paid special attention to this point.

These latter made a careful study of a small epidemic which occurred in certain Parisian prisons in 1893. Their results may be very briefly stated as follows:—

In several of the post mortems which they performed they found a diplococcus in the lungs and bronchial secretions. They did not find it in any other organ save when infarcts were present, in which cases they succeeded in obtaining it from the wedge-shaped areas. Microscopically, the organism appeared as an undoubted diplococcus, which they describe as small and fragile, apparently provided with a capsule and non-motile. It stained readily by the ordinary methods, and also by the process of Gram. They were able to cultivate it, and found that it grew well in broth and milk and on agar and potato. It would not grow at all well on nutrient jelly even at a room temperature of 20° C., growth appearing only after eight days and being very poor. They also examined the blood of living patients, and in some cases found a similar organism, cultures of which, however, remained sterile. They injected broth cultures of this diplococcus into rabbits and guinea-pigs, with, as a rule, fatal results, a condition closely resembling pneumonia being produced. Cultures of the diplococcus were obtained from the blood of the inoculated animals during life or immediately after death.

Hlava of Prague described a strepto bacillus in the blood, and Lewaschew, small motile bodies of varying shapes.

The recent epidemic of typhus with which Edinburgh has been visited has given an excellent opportunity for an investigation of this important subject. Results have been obtained sufficiently interesting and remarkable to justify a preliminary notice of what has been done, and we hope at a later period, when the research has been concluded, to give a full account of the completed investigation.

The experiments, which were begun before we knew anything of the work of Dubief and Bruhl, have been conducted upon post mortem cases and upon the living, and it is here proposed to state in the first place what was found in the cadaver, and in the second to describe what was discovered during life.

1. *Post mortem cases.*—In all, seven fatal cases of undoubted typhus fever have been examined, and in every case a diplococcus has been found. In six of these cases, an examination, with the usual precautions, was made of blood from the heart, the spleen, and the lung. In four of the six cases the diplococcus was present in each organ. Of the remaining two, one showed the organism in both spleen and lung but not in the heart, while the other exhibited it in the lung only. In the seventh case the examination was limited to the spleen.

We intend to give a full detail of the methods employed in a later article, but they were devised in order to preclude any possibility of error. It was found that, as a rule, if bodies were examined later than five hours after death, other organisms, such as the bacillus coli communis, were also present, but by the ordinary cultural methods we were able to isolate and study the diplococcus.

Microscopically, under the ordinary high power (obj. 7, oc. No. 3, Leitz), it appeared as a well-marked diplococcus, apparently about 2 μ in transverse diameter, though this measurement requires confirmation. There was at no time any appearance of a capsule, and it possessed Brownian movement alone. It stained well with carbol fuchsin, gentian-violet, and retained the stain by Gram's method. In some instances it tended to occur in chains, tetrad formations, and small clusters, but generally it was found as a separate diplococcus (Figs. 1 and 2). It grows rapidly in broth and milk, and as far as at present ascertained it seems to possess the power of coagulating the latter in from twenty-four to forty-eight hours. It is easily cultivated on agar, blood serum, Löffler's blood serum, and potato. Both in jelly streak and stab preparations its colonies appear generally within three days, when kept at room temperature, *i.e.* 18° C. Liquefaction takes place in from five to eight days. In broth, incubated at 37° C., it forms a cloudiness within twenty-four hours, and the broth eventually tends to clear by deposit. On agar the appearance is characteristic, copious, white, and irregular at the margins. On blood serum and streak jelly the growth is somewhat similar; in stab jelly faint, confined as small dots to the needle track; on potato, copious and cream-coloured. It grows readily when plated on agar jelly, the growth being diffuse and spreading, and more slowly when plated on nutrient jelly, in which it appears as small pin-head colonies. There is no gas formation in glucose jelly. The most favourable temperature for its development and growth appears to be 37° C.

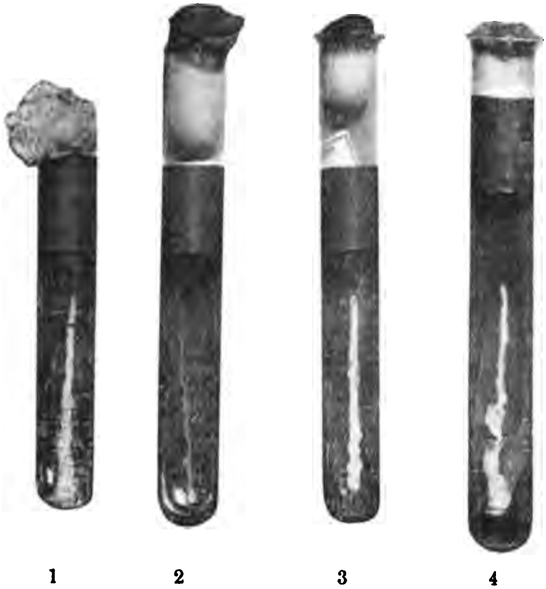

FIG. 1.—Agar cultures, Cases 1-4. Case 2 presented some special features.

A doubtful case, which proved to be one of purulent meningitis, was examined, and nothing resembling a diplococcus was found. A negative result was also obtained in a case of typhoid fever.

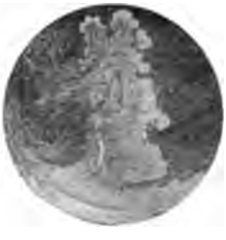

FIG. 2. — Agar plate from Case 1.

2. *Examination of blood in living cases.*—At a later period it was considered advisable, on Dr. Ker's suggestion, to examine the blood. At first this was done by means of films and hanging drops, and by broth cultures made direct, but the first two methods were abandoned, as they were considered unnecessary. The blood in all cases was taken from the thumb, the skin having been thoroughly purified. The needle was carefully sterilised by heat, and the drop of blood transferred by means of a sterilised platinum loop directly to

the broth, samples of which had been previously incubated, tested, and found to be sterile.

Briefly the results were as follow:—

In all, nineteen cases of undoubted typhus were examined. Of these, fifteen were acute cases, ranging from the fourth to the fourteenth day of the fever, and in every case large numbers of diplococci were found, after the broth had been incubated at 37° C. for from twelve to forty-eight hours. No other organism was present, and the diplococcus presented the same characters as the diplococcus found post mortem. Several of these cases ended fatally, and were amongst those examined after death.

Where films and hanging drop specimens were examined, the organism was usually found, separate and free in the plasma. The remaining four cases were convalescents, ranging from a few days after the crisis to five weeks, and, with one doubtful exception, the same result was obtained.

The diplococcus presents, as far as ascertained, the same microscopic and cultural characteristics as that obtained from the dead body. Many of the cases were examined on several occasions, and at no time was the diplococcus absent. In addition, up to the present, seven control cases have been examined, one being a case of scarlet fever, one of measles, the others, cases exposed to the infection of typhus, but showing no symptoms of the disease. In only one of these, a quarantine case, was the diplococcus present, and Dr. Ker suggests that the patient, a boy, may have had a slight unnoticed attack of typhus. Nothing resembling the bodies described by Thoinot and Calmette, Hlava, or Lewaschew has been encountered.

It is not intended at present to make any comments on this research, nor to put forward as yet any theory regarding it. It may be stated, however, that there are marked differences as well as resemblances between the organism found and that described by Dubief and Bruhl, and we trust to be able to compare them fully later.

It seemed advisable, however, to give a brief résumé of what has so far been done, and we would take this early opportunity of thanking Dr. Claude Ker, Medical Superintendent of the City Hospital, where all our cases were examined, for the facilities he has afforded us in the carrying out of this research, and the great help he has given us on many occasions. We would also express our deep obligation to Professor Hunter Stewart, in whose laboratory the work has been done, for his kind advice and helpful suggestions.

REPEATED ECTOPIC PREGNANCY IN THE SAME PATIENT, WITH ACCOUNT OF A CASE WHERE TWO GESTATION SACS (ONE A LITHOPÆDION) WERE SUCCESSFULLY REMOVED FROM EITHER SIDE, AT ONE OPERATION.[1]

By JAMES HAIG FERGUSON, M.D., F.R.C.P.Ed., M.R.C.S. Eng., *Lecturer on Midwifery and Diseases of Women, School of Medicine, Edinburgh, etc.*

CASES of extra-uterine pregnancy occurring on both sides of the same patient, either at the same time or at intervals, are sufficiently rare to merit special note being taken of them; and the case which I am about to record has this unique characteristic, that it is the first, so far as I can find, which has been actually demonstrated in Scotland, though Reid of Glasgow described a case last year, which in all likelihood belongs to the same category.

Olshausen was the first to give statistical observations regarding extra-uterine pregnancy occurring twice in the same patient in connection with a case of his own, and Abel thinks such cases much more common than is generally thought, and that we are only just beginning to observe them. There has lately been a considerable number of cases reported of repeated tubal pregnancy in the same woman, but laparotomy performed twice on the same case is mentioned by only fourteen operators —Abel, Veit, Herman, Olshausen, Ross, Beck, Bennington, Harrington, Frankenthal, Brosin, Kokmann, Schoolfield, Falk, and Zangemeister, the latter recording two cases of this nature. Kokmann's is the only case where a normal pregnancy and delivery occurred between the two laparotomies. In Lawson Tait's case a normal pregnancy and labour occurred between the two extra-uterine gestations, but there was no second operation. As regards the frequency of the condition, few statistics are available. Puech, in 1879, gives eight cases of repeated extra-uterine gestation out of 180 cases of extra-uterine pregnancy, and Dührssen records two cases from a total of thirty-seven, making an average for Puech of 4·4 per cent. of repeated tubal pregnancies, and for Dührssen of 5·4 per cent. in their series of cases of ectopic gestation. From these figures we would therefore deduce that in every hundred cases of extra-uterine pregnancy we should expect to find practically five examples of both tubes being involved at the same time, or of having been involved at intervals more or less prolonged. According to Zangemeister, the shortest interval between two repeated tubal pregnancies is six weeks, and the longest is five years. The case of the six weeks' interval was a case of Zangemeister's, and the example of five years' interval is Kokmann's. In Coe's case, however, there was an interval of twelve years between two tubal pregnancies on the *same* side.

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[1] Read before the Obstetrical Society, Edinburgh, 14th December 1898.

Cases are recorded where ova were found either in the same tube at different places, or an ovum in each tube at the same time.[1] The references I append include only those cases where ectopic pregnancy has occurred on both sides, or, as in Taylor's and Coe's cases, twice on one side. The cases of twin tubal pregnancy, and the examples of combined extra-uterine and uterine pregnancy, are not referred to here. Some of the cases recorded as repeated extra-uterine pregnancies are certainly doubtful, as, for instance, one of Herman's cases and Van Henkelom's case. Haydon's well-known case is apparently one of pregnancy in a uterus with rudimentary second horn on the second occasion, the first being a tubal pregnancy.

The following is the account of my own case:—

Mrs. M. is an apparently healthy woman, æt. 29. Her first child was born in May 1891. It was a cross-birth, and the child, though born alive, only survived one hour. A second child was born at the seventh month in February 1892, which likewise only survived one hour. After this her menstruation was quite regular till January 1894, when she missed her period. After four months' amenorrhœa she was suddenly seized with violent abdominal pain, chiefly on the right side, and shivering. At that time she resided in Ireland. She can give no history of discharge of blood-clot or membrane of any kind on that occasion. For these symptoms she was treated with poultices and hot applications by her medical attendant, and she was confined to bed for many weeks, and felt very ill for the first week or two. After her recovery the periods returned as usual, and she was quite regular till February 1898, when she again missed two periods. An interval of about four years thus elapsed since the previous amenorrhœa. I am indebted to Dr. D. J. Graham for these notes of the patient's history.

I saw the patient first on 11th April 1898; she was then suffering from pelvic pain and retention of urine. After the bladder was catheterised, there was found on examination a fluctuating tender swelling about the size of a goose egg, on the left side of the uterus, the uterus itself being enlarged and pushed to the right side. To the right and slightly above the level of the fundus uteri a fairly hard, somewhat irregular, body was felt, which gave the impression of being ovarian in its origin or possibly a pedunculated fibroid. A small shred of membrane was passed per vaginam after the examination, and on microscopical examination it proved to be a piece of typical decidua. The case was clearly a left tubal pregnancy, about the third month of gestation; the size of the sac corresponding pretty closely to the period of amenorrhœa which had occurred. I thought it probable that there had been some hæmorrhage into the mesometrium, partly from the fact of the sudden pain and sudden attack of retention of urine, which were the cause of my being sent for, and partly from the physical signs on examination.

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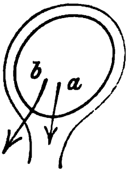
[1] See Rowan, *Austral. Med. Journ.*, 1890, p. 265; and Boehmer, "Observat. Anatom. Rar.," pars 2.

The retention of urine did not last for more than a couple of days. On the 19th April (eight days after first seeing the patient) I performed abdominal section, as the patient's symptoms called for active interference, and the tumour was steadily increasing in size. On the left side was found the characteristic appearance of a tubal pregnancy. Just as I touched it (without the least rough handling) the sac ruptured posteriorly, and blood-clot and fresh blood escaped into the peritoneal cavity. At the time it looked as if the rupture had taken place, not through the tube but through the broad ligament, but more careful examination shows that the rupture took place through the tube wall close to its attachment to the broad ligament (see Plate I.). The sac was comparatively easily removed, along with the corresponding ovary, which is closely attached to it, and the hæmorrhage was soon controlled. There were blood-clots in the cavity of the broad ligament. On the right side an irregular nodulated swelling was discovered, attached by a narrow pedicle (close to the right cornu of the uterus) to a very much dilated and tortuous tube, which was adherent to the fundus uteri. The tube closely resembled small intestine in appearance and size, and was filled with serum, a well-marked hydrosalpinx. This sac proved to be a lithopædion (see Plate I.), and was filled with the bones of a four months' fœtus. The lithopædion sac lay in the peritoneal cavity, attached to the tube by a narrow fibrous pedicle, and looked as if it had been originally extruded through the peritoneal covering of the tube. The right ovary was cystic, and was removed at the same time. The patient has made an uneventful recovery.

It is interesting in this case to see that the presence of the lithopædion on the right side corresponds exactly to the patient's history of four months' amenorrhœa in 1894, ending with the symptoms of pelvic inflammation on the right side, the result of a ruptured tubal pregnancy.

On referring to the illustration (Plate I.), the appearance of the specimens is well seen. The lower figures represent a vertical section of the left tubal gestation, the two halves being folded back and laid side by side to show the contents of the sac. The thinned-out tube wall is well shown, and at one part there is a distinct patch of sclerosis. Effused blood is seen in the mesosalpinx, showing that rupture had occurred there to a limited extent. The point of rupture of the sac, partially and mainly through the tube wall into the peritoneal cavity, and partially into the broad ligament, can be seen. Placental tissue is well marked in the section, but the fœtus is lying to one or other side, and is not seen. The illustration shows the specimens much shrunken by the action of the spirit. The action of the spirit, however, has been to render more distinct the fœtal bones through the sac wall of the lithopædion, and by causing the sac contents of the left tubal gestation to shrink in somewhat, the attenuation of the wall of the tube is better seen.

It is of interest to notice that rupture of the tube must have occurred first into the broad ligament. There was, however, no great separation of the layers of the mesometrium, and clearly there must have been some resistance to effusion occurring to any great extent in that direction. This is to be accounted for, I think, by the fact that there is evidence of old inflammatory mischief in the broad ligament, which prevented any great separation and distension. The result was, that very little relief of tension occurred in the tube after the first rupture, and this accounted for the tube rupturing into the abdomen when I first touched it. The condition at the operation to all intents and purposes resembled an unruptured tube. The attempt at the formation of a broad ligament (extraperitoneal) gestation failed, owing to the inflammatory exudation, and rupture eventually occurred into the abdominal cavity.



In the diagram, which represents the tube with its peritoneal covering, the arrow (*a*) shows the direction of the first rupture between the layers of the broad ligament; the arrow (*b*) represents the direction of the second rupture, which occurred posteriorly into the abdominal cavity, and which communicated with the cavity of the broad ligament as well.

There can be no doubt but that extra-uterine pregnancy is due to a great variety of possible causes, the chief of which is some mechanical obstruction, most likely from perisalpingitic or perimetric adhesions causing obstruction of the tube by the formation of peritoneal bands (Virchow, Speigelberg, Schroeder, Martin, Scanzoni, Webster, Rumpf, Falk). In this case there can be no question of the existence of old-standing perisalpingitis and cellulitis, especially on the left side. The inflammatory thickening of part of the tube wall, and of the broad ligament, is not recent but of old standing.

This leads one to consider, in view of the possible recurrence of extra-uterine pregnancy in the same patient, what should be done with regard to the other tube and ovary, when one operates for a single ectopic gestation. Clearly, if there is the least sign of disease, they should be removed also. But this unfortunately is not always possible to recognise, as changes in the mucous membrane of the tube or obstructions in its lumen may exist without any external evidence of their presence. There seems to be little doubt but that the proper course in cases of operation for extra-uterine pregnancy is to remove the appendage of the opposite side as well, when this can be safely done. In all such cases the operator should get the *written* consent of the patient or her husband, giving him full discretionary power as regards the removal of the other appendage, even should there be no macroscopic evidence of disease; should this be refused, the responsibility rests with them. Reed[1] advises removal of the other

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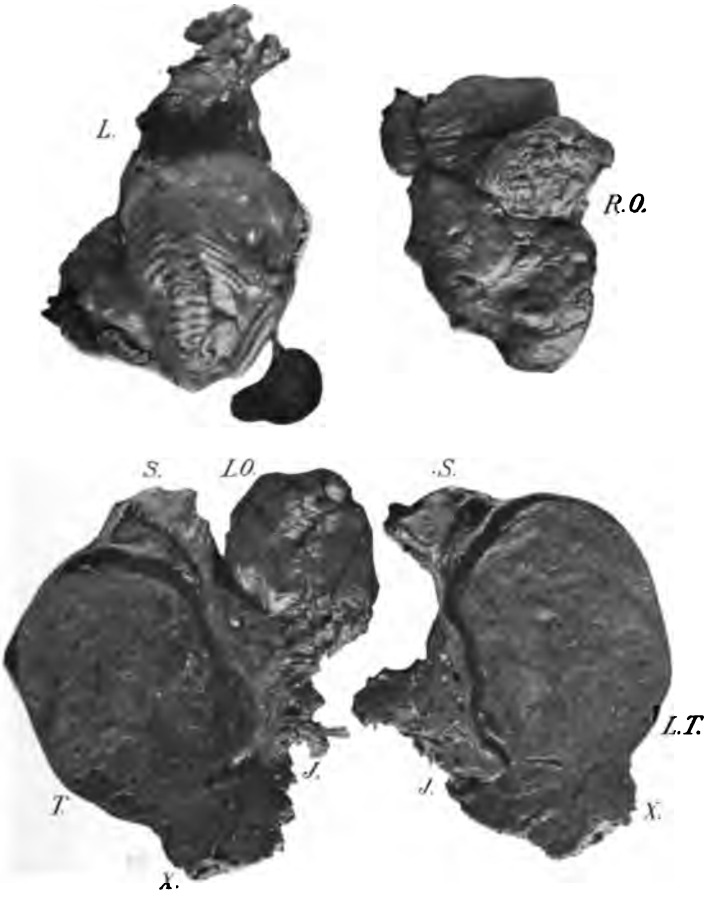
[1] *New York Med. Journ.*, 6th June 1891.



From a Photograph (natural size) of a right lithopædion and a left tubal pregnancy removed from the same patient at one operation. Specimens much shrunk from action of spirit.

UPPER FIGURES.—*L.* Lithopædion sac from right side, showing spinal column and ribs of 4 months' fœtus with pedunculated tubal cyst attached. *R.O.* Right ovary (cystic), and part of dilated tube.

LOWER FIGURES.—*L.T.* and *T.* Vertical section through tubal gestation sac on left side laid open and showing shrunken contents. Placental tissue is well seen, but the fœtus is not in the section. Note (1) great thinning of wall of tube except at sclerosed portions *S. S.*; (2) effused blood in mesosalpinx; (3) point of rupture of sac between the points *X* and *J*, partially through tube wall into peritoneal cavity, and partially into the broad ligament. The thin tube wall terminates at *X* in the figures, and between *X* and *J* lies the partly extruded portion of the sac contents. *L.O.* left ovary.

ovary and tube in cases of ectopic gestation, "owing to the frequency of recurrence, for the desquamative salpingitis, which is the cause of the extra-uterine gestation in one, without doubt exists in both tubes."

This case further enables us to draw an interesting comparison in the same patient between an ectopic gestation left to nature (and where recovery has fortunately taken place), and one treated by prompt surgical interference. The patient apparently just managed to struggle through on the first occasion, and she had a most tedious and painful convalescence. On the second occasion she escaped all this, made a rapid and painless recovery, and no dangerous symptom manifested itself at any time after the operation.

Lastly, as regards the method of operation, it is very seldom that the vaginal method would be indicated. The main indications for vaginal operation are—(1) Where suppuration has occurred in the sac, and (2) in some cases of a second recurrence of ectopic gestation where possibly extensive adhesions exist above, and when the tumour is mainly pelvic, and easily accessible per vaginam. Even in the latter class of cases, however, cœliotomy would be the preferable operation, unless under very exceptional circumstances.

In a mesometric gestation, that is when the tube bursts between the layers of he broad ligament, operative interference is rarely necessary, as the hæmorrhage usually ceases, and the blood and products of conception gradually become absorbed. In this case, however, in spite of the mesometric rupture, the sac continued to grow, and the ovum was continuing to develop, not in the broad ligament but in the tube, for the reasons already explained. It was well, therefore, for the patient that she underwent an operation before further rupture took place, this time into the abdominal cavity, which, as the event showed, would certainly have occurred at no distant date.

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ON THE RELATION OF THE NERVOUS SYSTEM TO DISEASE AND DISORDER OF THE VISCERA: THE DISORDERS OF VISCERAL MOTION.[1]

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(*Continued from page* 42.)

It is considered by some physiologists that tactile and muscle sense sensations travel up the cord to the brain by way of the vesicular column and the gracile and cuneate nuclei, while sensations of heat and cold pass up through the grey matter to the optic thalamus. If this be so, it may explain the alterations in the temperature sense in lesions of the cord involving the grey matter, such as syringomyelia, but the degree to which such lesions influence the temperature sense in the viscera is for many reasons difficult to determine. It is difficult indeed to test the temperature sense in syringomyelia, even of the stomach, much beyond its cardiac end, and even here we have the complicating condition of nerve supply from another source above the seat of lesion. The temperature of hot and cold drinks may be appreciated in the stomach, but beyond it, the warmth of the body in the case of cold drinks, natural loss of temperature in the case of hot drinks, and the comparative and normal insensibility of viscera generally, soon seem to render the temperature indifferent, and its perception impossible. It may be shown, however, in syringomyelia, that while the surface exhibits abnormalities and loss of temperature sensation, the temperature of imbibed material is still felt by the stomach, and this retention of sensibility is probably due in some cases to the pneumogastric innervation of the viscera. In the record of such cases, however, I have not been able to discover that the internal temperature sense has been investigated with the same care as the external, and the point seems worthy of more attention.

The association of cephalic paræsthesia with visceral disorders is a subject of interest. The situation and characters of such sensitiveness have been carefully studied by Head,[2] who has been the most recent observer to point out the situation of hyperæsthesiæ on the surface of the head in connection with visceral disorder, and has given anatomical reasons for the concurrent implication of the sensory nerves of the scalp. Such sensory points in connection with visceral disorder undoubtedly occur, as also do hyperæsthetic points on the thorax and trunks in connection with cardiac and other visceral disease and disorder, but in a large proportion of headaches, chestaches, and trunkaches, pain with little or no tactile hyperæsthèsia, or hyperæsthesia difficult to detect and delimit, is, so far as my experience goes, the rule. This association of visceral disorder with cephalic pain has long been known, and some forms of it carefully described. Robert Whytt of Bennochy, sometime President of this College, and Professor of Medicine in the University, was as good a judge of a headache as his late representative, the amiable author of "Katerfelto," was of a horse. In discussing the causes of periodical headaches, Whytt writes[1] among these of "Sympathy with the stomach, by which the nerves chiefly of the fore part of the head suffer; and the small vessels to which they are distributed are either affected with a continued spasm, or agitated with alternate contractions and relaxations; in consequence of which the patient feels a pain, straitness, fulness, and pulsation about the forehead and temples" This excellent description of migraine, now 130 years old, emphasises the vasomotor and neuralgic elements in these visceral headaches, a certain proportion of which are probably meningeal, not cutaneous, and from their intracranial situation not accessible to tactile investigation, and therefore not objectively hyperæsthetic.

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[1] The Morison Lectures, delivered before the Royal College of Physicians, Edinburgh.

[2] *Brain*, London, 1893-94.

While searching for the innervation of the pial vessels, I likewise investigated the innervation of the pia mater, and found that the nerves which have long been known to course through it end in it by plexus, as I have stated in this Journal.[2] Some headaches, therefore, and the pain of meningitis, are explicable by the copious innervation of the membranes of the brain, and the neural pathology of the investment of that organ is thus brought into line with painful affections of the pleura and peritoneum. The severe headache of tertiary specific disease may in some cases be meningeal; but as the brain differs from other organs in being the seat of perception and the focus for the sensory channels of the body, it is probable that in the case of this viscus, disease implicating the structure of its centres and nerves may cause pain in a manner in which a similar lesion would not do in the case of other viscera. The determining factor for local pain in the latter I have argued is a simultaneous tension which increases the force or degree of impression, and in the case of visceral muscle a reflex cramp which squeezes nervous end organs.

In speaking of the general trophic influence of disease of the central nervous system, it was pointed out that pain and mental anxiety were the chief determining factors under these circumstances, and that mental anxiety might be regarded as having the same relation to the intellectorium that pain had to the sensorium.

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[1] "Observations on the Nature, Causes, and Cure of those Disorders which have been commonly called Nervous, Hypochondriac, etc.," Edinburgh, 1767, p. 298.

[2] *Edin. Med. Journ.*, 1898, N.S., vol. iv. p. 413.

That they were, in short, both painful emotions. Laycock, indeed, called mental anxiety phrenalgia, and considered depression of spirits and hypochondria varieties of that species of pain.[1] As the brain, moreover, is compact of many organs, or, as I shall term them later, organules, which concur to produce that multi-unity the mind, which can distinguish the effects of the exercise or want of exercise of one organ or centre from those of another, such as the different effect of the various special sense centres, so it is probable that the direct effect of the disorder of one viscus may impress the mind differently from the disorder of another, the harmonious and painless operation of all being necessary for that unconsciousness of the existence of a particular viscus which is characteristic of vigour and health. Some organs, moreover, like the liver and kidney, the functional activity of which influences the character of the nutrient fluid for the whole body, have when diseased a more depressant general influence than disorders or diseases in those organs which are chiefly mechanical or more limited in function and effect. The attitude of the patient to disease is also, no doubt, modified in a measure by the condition of the mental organ itself. The naturally blunt and stupid, who fail to appreciate the general gravity of their situation, have also probably a minor power for the appreciation of discomfort, and may exhibit a callousness in physical distress only attained in the more sensitive and gifted after a philosophical estimate of their general relation to things and time. But, putting aside stupidity and philosophy, the hope to live or desire to die as influenced by disease or disorder of particular organs is largely dependent upon the circumstances, whether such disease or disorder saps energy or produces pain beyond a certain point, which is open to individual variation. A pronounced and protracted sense of feebleness is little if at all more endurable than actual pain, and disease which engenders such induces a mental depression and despair of vigorous life more certainly than many more serious diseases which have not that specific effect. The suicide of the so-called convalescent from influenza—not an uncommon event—is an instance of this; and prolonged enfeeblement in general from any cause, with its concomitant discomforts and disillusionments, is productive of a misery which the serious disorder of a particular organ which does not rapidly and profoundly enfeeble or cause pain to a certain degree does not induce. Thus the cardiac sufferer, whose natural vigour is good, and whose diseased heart is well developed and has been well compensated, may evince much less and experience less distress than the patient who has experienced angina vera for the first time. Nothing has struck me as being more pathognomic than the expression and general attitude of some patients on their first experiencing that form of cardiac anguish. It is like "a bolt from the blue" in their lives; they have never experienced anything similar before; the cloud-curtains of the unknown seem suddenly about to be raised, and fills them with an anxious astonishment which frequently, on a repetition of the phenomenon, plunges them into an acute despair, until the innate bravery of man at his best enables them to face that, as any other contingency, with indifference, and with such aid as it is the privilege of the physician to be able to render.

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[1] "Medical Observation and Research," 2nd edition, p. 352.

Chronic dyspeptics again, with their minor but constant dysæsthesiæ with periodical exacerbations and imperfect hæmopoiesis, frequently develop a mental habit which renders them, like one of the most distinguished of them, to whom we can forgive much, "gey ill to live wi'."

On the other hand, chronic disease which attacks youth, is not associated with much pain, and only deteriorates vigour so that the perceptive power of the individual does not appreciate great differences in this respect, may be coupled with all the sanguineness and natural expectation of life and usefulness which is proper to that period. The expectation of recovery of the phthisical, even in the last stages of the disease, is so well known and common a phenomenon as to have become stereotyped in clinical language.

There is no doubt, under all these circumstances, except in the case of the stupid, a so-called metaphysical element in the manner in which the mind is affected by organic disease, but it may, I think, be regarded as based upon clinical facts, that the mind appreciates the necessity for the exercise of philosophy least readily, as is most reasonable, when visceral pain and rapid enfeeblement are not dominant characteristics of the type of disease from which a given patient is suffering. This conclusion may appear after all a "surface-worm" to dig up, and the labour of finding it rather incommensurate with the value of the result, but the subject has appeared to me of some interest in its bearing upon the relation of visceral disease to mental emotion, and I trust I may not seem to have emphasised the point too tediously.

The separation of sensation from motion is relatively to the mind a distinction in what may be termed objective consciousness. We feel pain and can localise it, and we can observe movement. The separation of sensibility from motility is a finer distinction, and is largely in what we may term subjective consciousness or reasoning. Essentially, the transmutation of one form of motion into another merely emphasises the common property by which a living universe is conditioned. Clinically, the distinction is convenient, and no circumstance reveals more instructively the relation of the nervous system to visceral motion than the influence one organ has upon another. The displaced or twisted kidney which induces a tachycardia, the diminished thyroid secretion which causes a cardiac stagger or a bradycardia, the retained excretion, or the peripheral excitation which induces a convulsion, or the functional inactivity generally which disturbs metabolism, all show how intimately one form of motion is related to another; and when we emphasise motility in a given clinical condition, we might rationally regard it in some instances as the motor feature of an essentially sensory and causal disorder.

In touching upon some points illustrative of the relation of the nervous system to viscero-motor disease and disorder, I shall follow the plan already adopted in discussing sensory disorders, and take as the type of disordered visceral motion abnormalities in the action of the heart, making reference incidentally to analogous conditions in other organs.

The motor innervation of the heart, we know, is accelerant and augmentor-motor, inhibitory and depressor-motor, and trophic or pneumogastric proper, all these movements having necessarily their sensory concomitants or equivalents. Gaskell and his successors, anatomical and physiological, have revolutionised, as we learned last year, our conceptions of the visceral nervous outflow in general, including the innervation of the heart. For whereas we formerly regarded the pneumogastric as a primary, and the cervical sympathetic as a secondary source of cardio-motor innervation, we now believe that the pneumogastric is a secondary and the cervical sympathetic a tertiary source of such innervation.

Whether all the suprathoracic vasomotor outflow originates as low down as the second dorsal spinal nerve, appears still to be somewhat doubtful.

Hill and Bayliss[1] failed to evoke vasomotor action in the intracranial circulation by electrical stimulation from this region, but in the performance of their experiment they split the thorax and sewed it up again. Whether in doing so they disturbed the intracranial circulatory conditions by affecting the power of thoracic aspiration, which I endeavoured on clinical and anatomical grounds to show, in a paper on the treatment of aortic valvular disease read before the British Medical Association in 1895, has an important influence in maintaining the intracranial circulation, I cannot say. In any case, they failed to evoke the anticipated result. The source of the nerves in question still appears to be doubtful, and it may be that their origin is somewhat higher.

Without the aid of experimental physiology, and merely using the microscope for anatomical research, I have met with conditions bearing on this point. It will be seen that what appear to be the sources both of the pia matral and pia vascular innervation are certain tassel-shaped bodies which are arranged centripetally to the vessels, and of which I gave a short account in this Journal.[2] The precise locality of these vessels I have not yet had an opportunity of determining, but they appear to me to be sufficiently characteristic in design to be capable of identification by injected specimens. Thus identified, the position of the bodies in question, whether they be the source of vasomotor nerves or no, should also be determinable.[1] In the meantime we may assume, notwithstanding Hill's and Bayliss's failure to elicit response, that the outflow of the indubitably existent vasomotor nerves in the pia mater is from the same region as the general vasomotor outflow, that is, below the level of the first dorsal nerve, and thus bring the vasomotor phenomena of all the viscera, including the brain, into line.

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[1] *Journ. Physiol.*, Cambridge and London, 1895.

[2] *Loc. cit.*

Cardiac motion, like secretory and other motion, may be average or normal and plus or minus. A mid-point or equilibrium is a balance of extremes, and vital equilibrium is maintained by a certain variation around a centre of equipoise. Acceleration and retardation, augmentation and depression, constantly correct one another with the sensitiveness of a species of tentacula to surrounding circumstances, these tentacles being the nerves and the properties with which they are endowed.

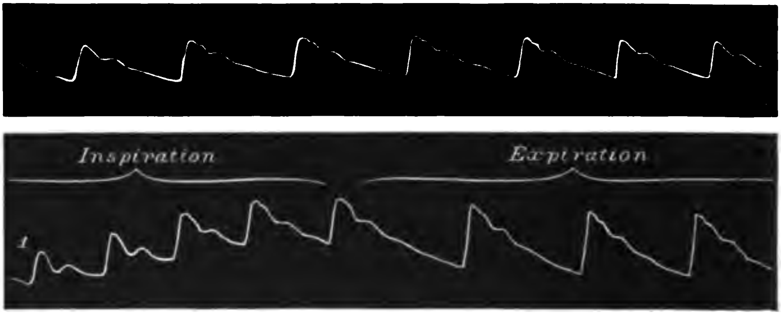

Fig. 1, Tracing 1.—Sphygmogram showing the influence of forced respiration upon the radial pulse wave. Tracing 2.—Sphygmogram of the same pulse during quiet breathing.

This is well exemplified by the influence of respiration on the character and rate of the radial pulsation. The sphygmogram (Fig. 1, Tracing 1) shows the small waves of forced inspiration on the ascending portion of the curve, followed by the large waves of prolonged expiration on the descending portion. These are no more the evidences of a mere pneumatic suction and propulsion than the cerebral circulation is the passive arterio-venous balance which Monro and his disciples conceived it to be. The pneumatic conditions play a part in the one, as the comparative resistance to atmospheric pressure does in the other, but the character and rate of the waves in the two portions of the curve indicate a vasomotor regulation, the peripheral incentive to which is, I believe, to be found in the increased pressure in the dextral chambers of the heart and pulmonary circuit during inspiration. For comparison with the tracing of forced respiration, I show one of the pulse of the same subject taken at the same time during quiet breathing, in which the pneumatic factor is conspicuous by its absence, although in a minor degree doubtless still operative (Fig. 1, Tracing 2).

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[1] Since writing this, I have examined these peculiar bodies with some friends, one of whom, an expert microscopist, for whose opinion I have much regard, disputed their neural nature, and considered some ganglion cells in connection with similar structures to be from another source. On careful consideration of the point, however, I feel myself justified in maintaining the view I have already expressed.

The antagonistic action of the accelerant and retardant nerves of the heart has been disputed. Retardation can be induced when artificial acceleration is in progress. It is none the less true that the safety of an abnormally inhibited heart consists in some acceleration of cardiac pulsation; and, as was mentioned last year, in the sphere of secretory motion, there appears to be a certain compensatory independence in the secretory influence of the cranial and sympathetic nerves of the sub-maxillary gland. It is probable that the same rule applies to other forms of vital motion. William Hunter,[1] referring to the fact that intense and even fatal jaundice may rapidly follow emotional shock, accounts for the phenomenon by suggesting a spasm or reversed peristalsis of the bile ducts while the bile is in full flood, as the probable cause. He admits, however (p. 81), that diminished quantity and increased viscidity of bile may be a cause of its absorption. The viscidity he attributes to catarrh of the bile ducts. Can a more directly neurotic hypothesis be advanced?

The main secretory nerve of the liver is the pneumogastric, and its sympathetic endowment is from the splanchnic nerve, while vasomotor nerves for the portal vein are derived, according to Bayliss and Starling, quoted by Halliburton,[2] from nerves ranging from the third to the eleventh dorsal. It appears possible, therefore, that the sequence of events may be somewhat as follows:—In sudden, severe, and mental cases shock is followed by pneumogastric inhibition and arrest of its secretory-motor function. This entails an attempt on the part of the sympathetic to come to the rescue, which it does by producing a more potent but also a more viscid bile—the nature of such potency not being at present to the point—which, like a blundering but well-meaning friend, stands in the way, just when the pneumogastric, having recovered from its inhibition, throws out by augmented activity too large an amount of bile for rapid escape, hence absorption and rapid toxæmia in some cases, and slow poisoning in others. I do not for a moment presume to indicate this solution as correct, but it appears to me as probable as other theories suggested, and brings the motor activities of the liver into line with those of the heart, which it is my present purpose to maintain.

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[1] Allbutt, "System of Medicine," vol. iv. p. 68 *et seq.*

[2] *Op. cit.*, p. 670.

Again, the striking phenomena, on the one hand, of Graves' disease, which I prefer on this occasion to call Begbie's disease, and, on the other, of myxœdema, appear to suggest a parallel on similar lines. The thyroid gland is innervated by the pneumogastric and visceral nerves which reach it by way of the inferior and middle ganglia of the cervical sympathetic. Of these its most active secretory nerve is probably the pneumogastric, although I am not aware that the fact has been experimentally proved. If we regard Begbie's disease as in the first instance a cardio-vascular neurosis, to employ a term expressive of our ignorance, we can conceive an increased though ineffectual effort upon the part of the pneumogastric system, that is, of motor fibres probably derived from the spinal accessory nerve and running in the pneumogastric, to restrain the cardio-vascular organs in excessive visceral motion, and although unsuccessful in that effort, so stimulated by it as to provoke increased activity in its own secretory motor fibres. This would result in an excessive secretion, among others, of the thyroid juice, the effect of which upon the circulation is to accelerate it. Thus the increased activity of that gland may add fuel to the fire which burns so lustily as vasomotor commotion under the circumstances in question. This secretory increase, as we know, is at present regarded as one of the most probable causes of excessive pulsation in Begbie's disease, the etiology of which, however, is not yet by any means clear.

It is conceivable, moreover, that a period of persistent hypersecretion may result in one of hyposecretion, the retardant influence of which may be slow in showing itself, for the following reason:—The sympathetic system, besides its general vasomotor over-action, may also induce a supplementary sympathetic juice as in the case of the submaxillary gland, and thus maintain for a time the waning accelerant influence of pneumogastric secretion. We know, however, that ultimately in a large number of cases the excessive excitement of the cardio-vascular system subsides more or less, that in some cases bradycardia succeeds tachycardia, and that in yet others myxœdema takes the place of Begbie's disease. The neuro-motor activity of the gland, in short, and its associated organs, is worn out, and we may expect some day to have demonstrable pathological changes in the controlling centres, which, together with peripheral degeneration, would account satisfactorily for the striking complex of symptoms referred to, and still unexplained. Action and supplementary action, endeavour and support, overaction and reaction, error and correction, seem to be general laws of life, and there does not seem to be any just reason why they should not apply also to the life manifest in visceral motion in its various forms.

To study the matter a little more closely, and taking cardiac action as the type, let us examine the motor effects of shock upon the heart, and in the first instance of the shock of physical pain, with which Laycock's "phenalgia" must necessarily be associated, and often is in a high degree.

As has already been remarked in a previous section, pain or excessive sensibility in any organ may influence the heart's action in association with various degrees of concomitant mental emotion, proportionately to the amount of the stimulus. The tachycardia associated with a displaced kidney may be present without much associated pain, and therefore without great mental distress. With more pain the same conditions will be emphasised. The reflex effects of nephroptosis may affect many organs besides the heart, and the severity of these secondary motor disturbances is not proportionate, as Macalister has stated, to the amount of mere displacement.[1]

Calculi, we have seen, may induce an extreme of agony which may terminate in collapse and death, the associated cardio-vascular phenomena being those of rapid and enfeebled action of the heart and a general vaso-paresis accompanied by profuse perspiration and depression of temperature. These extreme phenomena differ from those associated with angina pectoris chiefly in the situation of referred pain and in the uninterrupted duration of the agony. With this distinction these cases might be termed cases of angina pectoris hepatica, renalis, *et hæc species omnes.*

The first effect of sudden visceral pain, whether cardiac or general, on the movement of the heart is to arrest its action, and probably, could we at the moment of the onset of pain auscultate that organ, we should detect an inhibition. This, however, like peripheral spasm, which is usually regarded as the cause of angina pectoris, is so early a phenomenon that it must very rarely be indubitably observed, and when observed and present it is not necessarily the essential cause of the general complex of symptoms. The conditions usually first observed by the ear and hand are the phenomena of depression which follow inhibition, associated in many cases, shortly afterwards, with those of a rescuing augmentation and acceleration of the heart's beat.

Lauder Brunton, whose many-sided scientific activity has recently been recognised by his and our Alma Mater, and with whose name one of the most beneficent agents in the treatment of angina pectoris is indissolubly associated, was the first, so far as I know, to note graphically the cardio-vascular movements connected with valvular angina. He published a case[2] of aortic valvular disease with sphygmograms, in which he showed the quick small pulse of the agony and the larger and slower pulse of the interval free from pain. Brunton considered the pulse of the agony to indicate peripheral spasm, and was at a loss to reconcile its rate with its assumed condition. Since Brunton's original observations, however, opinion has modified a good deal on this point, and there are those who feel more disposed to regard the peripheral signs usually observed as those of empty collapse rather than of active spasm—of profound collapse; in other words, such as might be caused by other visceral and even somatic pain of a certain degree of severity.

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[1] Allbutt, "System of Medicine," vol. iv. p. 344.

[2] *Trans. Clin. Soc. London*, vol. iii.

Sir Richard Douglas Powell, however, in his recent Lumleian Lectures before the Royal College of Physicians, London,[1] also describing what he terms vasomotor angina in the case of a young man suffering from aortic regurgitation, spoke as follows: "It was curious to note the absolute extinction of all the aortic characters in the pulse, which became contracted to a small hard pulsating thread; whilst, as the patient sat forward, leaning against a chair in great pain, the bed shook with the violence of the cardiac beats, and his neck and subclavian vessels could be seen pulsating with responsive violence. A dose of trinitrine quickly brought back the aortic features of the pulse and dissolved the painful scene. One could not observe the labouring beat of the heart, the strongly pulsating large vessels contrasted with the almost effaced small vessels, without appreciating the power of vasomotor contraction to cause cramp or paralysis of a healthy ventricle; yet there are physicians who still doubt the efficacy of vasomotor spasm in the mechanism of angina."

I presume it is because of the humane desire not to disturb unnecessarily the patient in the throes of cardiac agony, that we find more frequent record of the condition of the radial pulse in these cases, than of the heart itself. If, however, the heart of such a patient be examined, it will be found that the extinction of "aortic character," which Powell noted in the radial pulse, is also very distinctly appreciable at the aortic orifice itself. The usually very considerably hypertrophied left ventricle will be found to pulsate with increased frequency, the bruits significant of the valvular defect being almost or quite hushed, and increased tension in the pulmonary circuit indicated by a well-marked accentuation of the second sound at the pulmonary arterial orifice. There is repletion of the ventricular cavity, and comparative emptiness of the extra-cardiac systemic arteries. The full and hypertrophied heart labours violently to rid itself of the residual, stimulating, and dilative burden of blood, not because it cannot jerk some of its contents into the larger comparatively empty vessels with more than "aortic" emphasis, but because its own intrinsic and momentary failure has cast upon it a paralysing weight which must be rapidly reduced if the organ is not to come to a standstill. The small peripheral pulse which is associated with such valvular angina during the period of cardiac failure, is not on this showing one of spastic narrowing, but of empty collapse. The following sphygmograms (Figs. 2–5), which I published to illustrate a paper on the blood pressure in angina pectoris,[1] may render my meaning clearer. The first shows the attack at its height, with a lowering of the predicrotic wave and a flat drag in the diastolic interval, meaning, as I think, that the collapsed and comparatively empty artery has failed to reach the lever of the instrument. In the next the cardiac force is increasing, its action becoming slower, and, although blood still fails to reach the periphery in normal abundance, there is a rise in the position of the predicrotic wave and an indication of dicrotism; pain is still present under these circumstances, but the patient is easier. The next sphygmogram represents the refilled and retarded pulse and the cessation of pain. The fourth and last tracing shows the

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[1] *Lancet*, London, March 26, 1898.

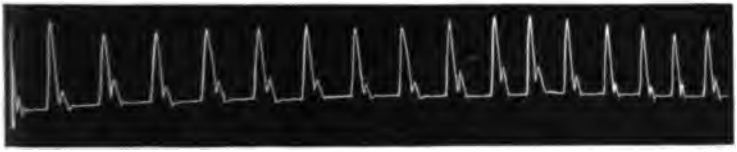

FIG. 2.

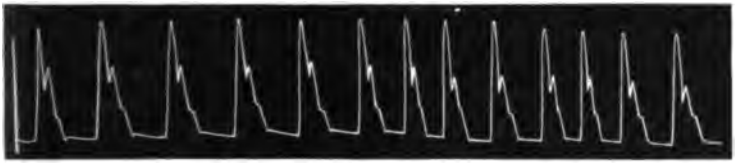

FIG. 3.

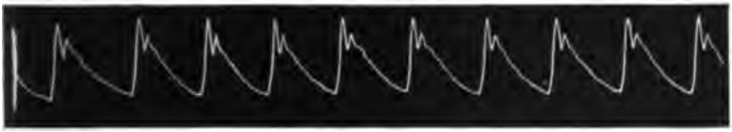

FIG. 4.

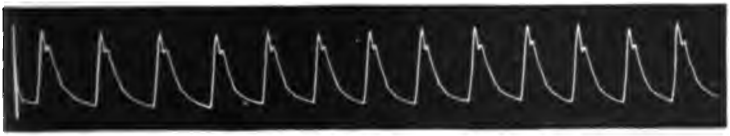

FIG. 5.

[1] *Edin. Hosp. Rep.*, 1895, vol. iii.

typical aortic regurgitant pulse of the patient during a period of temporarily re-established compensation.

The conditions are somewhat different in the angina of arterio-sclerosis without valvular disease, and also in those mainly associated with fatty degeneration of the heart. In the former, while the initial conditions of the attack are to my mind indubitably central, not peripheral, there may at times be noted a short period of simultaneous arterial hardening in association with a quickly pulsating heart. How much of this is spasm, and how much the induration of an arterio-sclerotic and imperfectly filled vessel, I have not been able, so far, to convince myself.

The angina of the fatty heart, again, may be associated with a slow, full, and soft pulse without suspicion of spasticity, the irresponsive and degenerated ventricle throwing its contents into the systemic arteries with bradycardial sluggishness, and in imminent danger of that syncope which so frequently overtakes it.

It has been mentioned that shock which so markedly affects cardio-vascular movement may likewise influence secretory motion, and I have suggested the presence of a supplementary or balancing secretion in such cases. Shock to the liver may, indeed, prove fatal to hepatic motion, as shock to the heart may to cardiac motion, but it is probable, for the reasons stated, that minor degrees of shock may have their consequences averted by a supplementary mechanism in such a way as would not be possible but for that double innervation which is a fact in the anatomy of the viscera, and but for that bilateral and double innervation which appears to play an important part in the innervation of unified or single viscera such as the liver and heart. This bilateral action is probably secured by an incomplete peripheral and plexal decussation which has not yet been demonstrated anatomically, but of which there is good experimental evidence. Thus, as has been stated, section of one pneumogastric nerve has little effect upon cardiac action, while section of both nerves permanently disables it, just as a similar lesion abolishes the glycogenic function of the liver.[1]

In addition to the transient phenomena of disturbed or excited, but not necessarily disordered, cardiac action to which I have referred in connection with angina pectoris, changes of cardiac rhythm of a more persistent character may be met with, and on the nervous relations of these, taken as a type of viscero-motor irregularity in general, I desire to comment very briefly.

I trust enough has been said to justify the thesis that in the regulation and maintenance of sustained visceral motion the nervous system always plays an important part. Its regulating faculty is more generally conceded, however, than its sustaining power. I assume, therefore, for the reasons stated, that in this respect also it plays essentially as important a part as the blood and the inherent irritability of the cell manifesting any particular form of motion. As I have stated, the functional unit is not one but triune in every form of visceral motion,—mobile, secretory, metabolic, and excretory,—and that, differ as these may from one another in details of manifestation, they are subject to essentially the same order or regularity of action, and to the same disorder or irregularity of action, as the more objectively mobile form which we associate most readily with muscular tissue. With this, perhaps unnecessary, reiteration I proceed to say a few words concerning abnormal variations in cardiac action and their relation to the nervous system, regarding such as typical of other forms of disordered visceral motion.

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[1] Halliburton, *op. cit.*, p. 670.

The heart's action, undisturbed by forced respiration, bodily exertion, or mental emotion, is, normally, quite regular. The eviscerated heart of an animal recently killed is also usually quite regular. Auricular systole precedes ventricular systole with automatic precision. Circumstances, in other words, which induce variation in stimulation are removed, and rhythmical cellular action, one of the factors in the triune partnership, for a short period, alone or assisted by some residual storage of nervous energy, or some remnant of stimulation by blood, continues to act with the unvarying rhythm which blood, nerve, and muscle in common contribute towards during life, when the persistently isochronic and isodynamic organ is undisturbed by extraneous stimulation. The first observable departure from this isochronia and isodynamia is a variation in rate rather than in force. The heart beats are all complete in themselves, and if, when more rapid, they are individually less powerful than a single slower beat, they collectively carry on the circulation with much the same efficiency as when the heart's action is more regular, provided no call be made upon the heart for additional exertion. This condition, when not due to mental excitement, as it may be (in which case it may be characterised either by augmentation or by depression, or by both these states alternately), is usually due to variation in the stimulation of the cardio-vascular nervous system caused by variation in the tone of the chambers of the heart, and in the amount of their contents. This simplest form of abnormality may be termed anisochronia, as variation in rate is more easily appreciable than variation in power. If the condition be present for any length of time, the variation in force becomes more noticeable, and to irregularity in rate is added irregularity in the size of the pulse wave. The systole may have half its normal force or less than half, a condition which may in general terms be called hyposystole. Now, inasmuch as the empty and eviscerated heart may have a perfectly regular rate (Fig. 7, Tracing 6) and amount of systole, this hyposystolic condition cannot be attributed to the amount of blood in the chambers of the heart. It must therefore be due either to the muscular or neural factor. The muscular factor, we have seen, with the nervous and hæmic factor at a minimum, may act perfectly regularly, and irregular post mortem tremor of muscular tissue is most frequently observed in somatic, not visceral, muscle. On the other hand, there is abundant evidence, clinical, physiological, and pharmaceutical, to prove that stimulation of the nervous system may profoundly influence the action of visceral muscle in augmenting or depressing it, and in hastening or retarding it. It seems, therefore, rational to conclude that variations in cardiac action during life, whatever the special character of the rhythm, must be largely, although not altogether, due to the combined or alternate and always associable play of the nervous mechanism we have examined. Such nervous action, even under abnormal circumstances, may be rhythmically or arrhythmically exerted.

Reduplication of the second sound of the heart may at present be excluded from discussion, as it may be wholly due to hydraulic variation in the pulmonary artery and aorta. Triplication, however, otherwise termed *bruit de galop*, cantering rhythm and reduplication of the first sound of the heart, while it is capable of being influenced by gravitational conditions and modified by posture, is essentially a retarded systole, or in some cases a systole followed by a very small supplementary systole, which for definiteness may be termed a hypo-hemisystole, or something less than a hemisystole. Like variation induced in the pulse wave by respiration, the neural factor is necessary to its production, as we may convince ourselves comparatively satisfactorily by a study of the more pronounced condition to which it is related, namely, quadruplication, or one of the forms of rhythmically dropped beat. Reduplication of the first sound has been variously accounted for. Sibson[1] considered it to be due to asynchronous action of the two ventricles, and his disciple, Sir William Broadbent, has recently expressed the same opinion.[2] Physiologists, however, such as François Franck,[3] dispute the possibility of dissociated ventricular action, and clinical examination and sphygmography appear to me to support the physiologists. Two ventricular systoles would, of course, mean two diastoles, and reduplication of the first sound in Sibson's hypothesis should usually be associated with a reduplication of the second sound, which is not the case. But were the double systole situated in the left ventricle, it would not necessarily imply a reduplication of the second sound. Now, sphygmography shows that cases of quadruple pulsation consist of a long systole followed by a short diastole, and this by a short systole followed by a long diastole. Systole, in other words, is followed by hemi-diastole, this by hemi-systole, and this finally by a long diastolic pause. The tracings emphasise these points. It has been observed that digitalis induces this rhythm in some cases, and this fact, I think, supports a neural view of its causation. When the beat is triple, and the first sound reduplicated, these events are crowded up, the first short diastole is omitted, and the triple rhythm with anacrotism results (Fig. 7, Tracing 2, and Fig. 6, Tracings 1 and 2). Instead of the bigeminal pulse of quadruplication, a trigeminal pulse may be present which consists of three systoles and two diastoles (Fig. 6, Tracing 3, and Fig. 7, Tracing 5). It is a quintuplication. In all these varieties the ventricles act together, not apart, and the regulating factor in the rhythm is, I believe, neural. The cardiac cycle may, we know, be altogether or almost suppressed, and this acyclia, it may fairly be argued, is a variety of inhibition, and probably not only neural but more precisely pneumogastric in origin, and due to local cardiac or distant reflex stimuli (Fig. 7, Tracing 7).

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[1] *Lancet*, London, 1874.

[2] "Heart Disease," London, 1897, p. 34.

[3] *Arch. de physiol. norm. et path.*, Paris, 1895, No. 3.

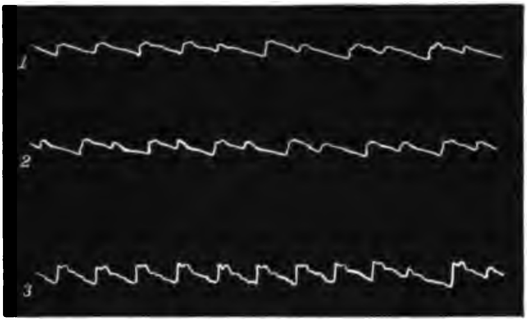

FIG. 6, Tracings 1 and 2.—Sphygmogram of dropped beat, with quadruplication of the heart sounds best marked in 2, when the patient was recumbent. Tracing 3.—Sphygmogram from the same case standing, when the quadruplication by change of rate was converted into triplication, the anacrotic summit being the representative of the hemisystole in Tracing 2.

Some of the varieties of abnormal cardiac rhythm mentioned may be of long duration, but they are essentially an adaptation of the organ to certain circumstances, with a view to carrying on the circulation; and while they may at times be significant of cardiac failure, they are not necessarily so, and cannot therefore be regarded as pathological. It is not so in some cases of persistent *bradycardia* and *tachycardia.* In the former, as we know, the heart's action is abnormally retarded, the neural cause of such retardation being, it would appear, either *in* the heart or *outside* the heart (Fig. 7, Tracing 1). Dehio showed[1] that extracardial bradycardia could be accelerated by the administration of atropin, and that what he terms cardial bradycardia could not be so affected. It is the latter kind which constitutes the graver danger to life. The precise neural mechanism of the condition can only be surmised, not having yet been demonstrated. Retardation should normally provoke acceleration; and as it does not do so, it would appear, in the absence of myocardial degeneration, or some general depressant, such as a poison, whether auto- or heterogenetic, that the seat of the disorder is in the spinal visceral innervation of the heart, an examination of which will probably ultimately reward the labour of some one. The transient bradycardia which follows fevers such as pneumonia may be attributable to exhaustion of the spinal visceral nerves, and Goldscheider and Flatau[1] have recently shown that high temperature has a very distinct effect upon the appearance of central nerve-cells. We know, however, as little, positively,

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[1] *St. Petersb. med. Wchnschr.*, 1892, No. 1.

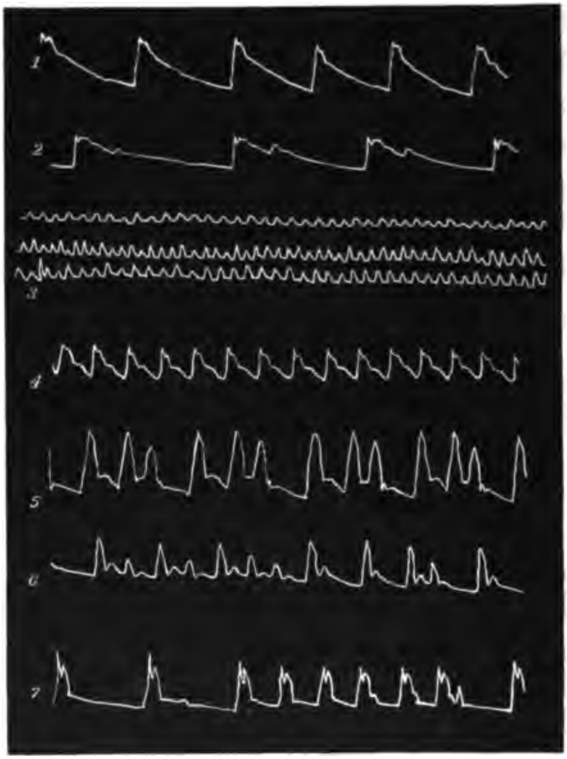

FIG. 7, Tracing 1.—Sphygmogram of persistent bradycardia. Tracing 2. —Sphygmogram of bradycardia with dropped beat. Tracing 3.—Sphygmograms of paroxysmal tachycardia. Tracing 4.—Sphygmogram from the same case of paroxysmal tachycardia, after subsidence of the attack. Tracing 5.—Sphygmogram from a case of quintuplication of the heart sounds in aortic valvular disease. Tracing 6.—Irregularity of the pulse, with hyposystole from a case of double mitral disease. Tracing 7.—Acyclia, with complete and partial intermission of the pulse.

about the essential neural causes of bradycardia at present as we do of those of persistent *tachycardia.* These very terms reveal our ignorance. There is some reason to believe, however, that persistent tachycardia, like bradycardia, may be of cardial and extracardial origin. My former colleague at the Great Northern Central Hospital—Mr. Spencer Watson—cured a case of persistent tachycardia by the removal of post nasal growths. Martius also, in his monograph, has collected a number of cases in which a degree of persistent tachycardia was associated with disease of the pneumogastric nerve or nerves. I have already suggested that in some cases of cardiac dilatation, associated with persistent tachycardia, a neural stimulation of the rich nerve supply of the heart may be active in producing the condition, and it is a common experience that the removal of dilatation also reduces the heart-rate to more leisurely and regular action. In essential or paroxysmal tachycardia, however, we have an interesting nervous condition, at present unexplained, but usually associated with cardiac dilatation, in which the delirious heart appears to escape from all nerve control, inhibitory and accelerant, depressant and augmentative, and to revel for a time in the fœtal tic-tac of emancipated visceral rhythmicality. This form of runaway heart is usually pulled up sharp by what appears to be an augmented inhibitory action of the vagus, which asserts its authority with surprising abruptness. The sphygmograms illustrate these phenomena (Fig. 7, Tracings 3 and 4).

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[1] *Op. cit.*

This sudden restitution to normal visceral motion is not unknown in other organs. The defective biliary secretion of non-catarrhal inhibited hepatic motion may be rapidly followed by normal secretion. The oliguria of an actively podagral state may as quickly be succeeded by copious urination, just as the mental irritability of suppressed gout may suddenly lessen when the disorder finds localisation in a joint. That these rapid changes may be due to neural conditions there is good reason to believe. It is related of vom Stein, the patriotic and able opponent of the first Napoleon, that when he was dismissed by the King of Prussia at the dictation of the ill-mannered Corsican, he developed a bad attack of gout, which resisted all treatment, balneological and other, until Queen Louisa, overruling the weak complaisance of her not too heroic Consort, procured vom Stein's recall to office. The gout disappeared forthwith, and the metabolic and excretory cripple of yesterday, restored to mental and bodily vigour, successfully organised the overthrow of the insolent invader of his country.

Just as visceral disorder may be the stimulus underlying somatic sensory phenomena, so also somatic motor phenomena may be due to visceral disorder. Irritative, septic, and stagnant conditions in the gastro-intestinal canal may apparently be the efficient cause of clonic and tonic disturbances of the somatic muscular system. In the case of the myoclonus multiplex, which affects some neurotic and neurasthenic dyspeptics, it is probable that these painless spasms are due to some general irritant in the blood, frequently aggravated by a certain amount of tabagism. But the question of a reflex or unabsorptive and non-toxic origin of widespread tetany is more difficult to decide. The bilateral tetany of the infant, for example, with carpo-pedal contracture, so frequently observed at our children's hospitals, and usually in association with gastro-intestinal irritation of a pronounced type, or with the recent history of such, might very plausibly be argued to be a reflex due to bilateral stimulation of the bilateral innervation of such a single tube as the alimentary canal. This view is, moreover, still more plausible in those cases in which the higher centres are quite unaffected, and the patient grins complacently at his carpo-pedal condition. It is, however, possible that there are toxic agents which in certain quantities act like strychnine chiefly upon the spinal cord, and the alcoholic extract obtained from the gastric juice by Bouveret and Devic [1] seems to have been capable of producing tetanic convulsions on intravenous injection. The motor disturbances, moreover, need not be limited to the lower segment of the nervous system, but may, according to the authors quoted, become generalised and even attended by epileptoid phenomena and unconsciousness. Under these circumstances, however plausible at first sight a reflex explanation of tetany may appear, it is probable that the toxic view now more generally entertained is the correct one. In minor degrees of the affection, it is just possible that reflex action may also play a part. In any case, the intimate connection between the visceral disorder and the neuro-muscular phenomena in question is indubitable. Whether some cases of somatic contracture of a so-called "hysterical" nature are of visceral origin cannot be stated positively, notwithstanding the large rôle previously assigned to the sexual system in these disorders. Renal calculus may, however, provoke a lateral contraction of the trunk on the affected side, which must be regarded as a true reflex; and I have observed the same condition in a neurotic patient, the subject of consolidation of the base of the left lung due to aortic valvular disease. The length of time for which "hysterical" contracture may persist removes auto-intoxicant influences from the purview of causes, and the final discovery in some cases of central sclerotic changes indicates the possibility either of original disease there, or of so-called functional changes becoming organic lesions, whether the original stimulus were splanchnic or not. These somatic hysterical contractures bear a very strong resemblance to an interesting group of cases which Mr. Clinton Dent brought before the Medical Society of London in 1897,[2] in which a spastic condition of portions of the alimentary canal may stimulate tumour, cause severe pain on the principles laid down in a previous lecture, and even justify the operation of laparotomy from the urgency of discomfort. This intestinal neurosis or contracture appears, like its somatic equivalent, to be in the first instance reflex, rather than due to a more general poisoning of the centres.

(*To be continued.*)

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[1] *Rev. de méd.*, Paris, January 1892, p. 48; February, p. 97.

[2] *Trans. Med. Soc. London*, 1897, vol. xx. p. 257.

REVIEWS OF BRITISH AND FOREIGN LITERATURE.

A Treatise on Human Anatomy. Edited by HENRY MORRIS, M.A., M.B. Second Edition. London: J. & A. Churchill. 1898.

THE early appearance of a second edition of this work is ample evidence of its popularity—a popularity no doubt largely due to its handy size and concise form. The editor has been able to retain the services of the former contributors, with the exception of Dr. H. St. John Brooks, and the late Mr. Arthur Hensman, whose articles, by the way, have been revised by Dr. Arthur Robinson of the Middlesex Hospital.

The aim of the book, as stated in the preface, is to provide a complete and systematic description of every part and organ of the human body, so far as it is studied in the dissecting-room. This necessarily involves the omission of all histological details—a rule which, however, has not been rigidly adhered to. Whilst sympathising with the editor in his efforts to exclude such details, we cannot help thinking that some account of the relation of nerve-cells and their associated fibres to each other, would have helped the student to understand what is after all one of the most complex subjects he has to study. A brief description of the minute anatomy of the kidney has been given, and we think, with equal advantage, a more detailed account of the minute structure of the brain and cord might have been added. A diagram or two would indeed have sufficed.

A new feature in the present edition is the account of the skin by Mr. W. Anderson. Here details of a microscopic character are more or less dealt with, such for instance as the structure of hair and hair follicles. Though stress is laid on the fact that wavy, straight, and curly hair vary in the form of their section, as pointed out by Pruner Bey, no mention is made of the fact, long since established by Professor Chas. Stewart, that the curliness of the hair depends on the curves of its follicle. Dr. Arthur Robinson has added a section on vestigial and abnormal structures. No doubt the author was handicapped by the limits of space, but the facts recorded, it seems to us, would have been much better split up and described in connection with the structures to which they refer.

Most of the slips which occurred in the first edition have now been corrected, but we note the persistence of an error on page 906, where, in the figure showing the superior view of the cartilages of the larynx, the facet on the upper surface of the cricoid, for articulation with the arytenoid cartilage, is wrongly named the crico-thyroid articulation. There are a few points on which we differ from the authors. Thus, in Fig. 120, the area mapped out for the insertion of the pectoralis minor is open to criticism, whilst the line indicating the attachment of the trapezoid ligament in Fig. 121 is decidedly misleading. Nor can we agree with the area defined for the insertion of the pronator quadratus in Fig. 129, the deeper fibres of which muscle have an extensive attachment to the triangular surface on the inner side of the shaft of the radius, immediately above the sigmoid notch.

Some new figures have been added. Amongst others we note a few diagrams of the lymphatics, which are instructive, and serve to emphasise what is, we fear, an often neglected subject. In the section on the nervous system, colour has been employed to render more distinct the distribution of the peripheral nerves, and additional diagrams have been added to illustrate the paths traversed by sensory and motor nerve-fibres, and their course to and from the cortex of the brain.

We must congratulate the authors on the success which has attended their efforts, and can heartily commend the work as one not overburdened with unnecessary details, but which places before the student in a concise form those facts which it is essential he should be familiar with. It is, what it affects to be, a standard text-book of macroscopic anatomy.

Arthur Thomson.

The Nervous System and its Diseases: a Practical Treatise on Neurology, for the Use of Physicians and Students. By Charles K. Mills, M.D. Edinburgh and London: Young J. Pentland. 1898.

The author of this volume has taken great pains to collect and to put into readable form a vast amount of information relating to diseases of the brain and the cranial nerves, derived in part from a large and varied experience. We welcome such an exposition of the nervous system and its diseases by the distinguished American neurologist, who has also made a special study of mental diseases and medical jurisprudence. He states in the preface that, should circumstances permit, this volume will be followed by another, which shall include the remaining diseases of the nervous system, insanity, and the medical jurisprudence of both nervous and mental diseases. All through the present work, however, we find the influence that this combined knowledge and teaching has had on Professor Mills in writing this book, resulting in an excellent, practical, and comprehensive treatise.

The first chapter, comprising 121 pages, is occupied with a sketch of the nervous system, its tissues, development, anatomy, physiology, nomenclature, and chemistry. It is always a debatable point whether too much, and yet not enough, is contained in these introductory chapters on anatomy and physiology; for our own part we are in favour of the plan adopted by the author, for the simple reason that the physician who treats disease will put before the reader especially all those essential points in structure and function which he has found useful in elucidating problems of pathogenesis and diagnosis. We are, however, somewhat disappointed with the chapter on the chemistry of the nervous system, about which certainly little is known; and we venture to express the opinion that it is hardly brought up to date. Moreover, the valuable work of Nissl, and others who have adopted his method, upon the changes in the nerve cells in disease, is not mentioned.

The adoption of a new nomenclature by the use of mononyms is an American idea, and needless to say therefore of practical utility. We fear, however, it cannot be introduced into English neurological literature until it is accepted by the standard text-books of anatomy and physiology. We are, however, of opinion that Professor Mills is to be commended in leading the way to a ready means of saving time and trouble by adopting this new nomenclature of Professor Wilder.

A chapter on symptomatology and methods of investigation, including electro-physics, the electro-medical examination, Weir Mitchell treatment, and the properties and action of drugs used in neurological practice will be found to be of great practical value to the practitioner and student.

We note with satisfaction in chapter iii., on diseases of the membranes, sinuses, and veins of the brain, how much the author has drawn upon the admirable work of Macewan and other British surgeons.

In chapter iv., the author devotes 100 pages to the important subject of the anatomy and physiology of the brain, as studied by experiments upon animals, and by the association of symptoms observed clinically during life, with lesions found post-mortem. This chapter will be especially useful, as it contains a large amount of information not usually found in text-books, *e.g.* cranio-cerebral topography and the various methods of examining the brain post mortem.

Chapter v., upon the cerebral circulation, vascular disturbances, and diseases arising therefrom, will be found instructive and practical. We presume that this chapter had already been in the press before Leonard Hill's work on the physiology and pathology of the cerebral circulation had appeared, as no reference to this valuable contribution to our knowledge is made by the author.

In the chapter on tumours we are glad to note that the author states that gummata may develop *from a few months* to a score of years after the primary infection. Our experience quite coincides with this statement. We believe the writer could have also said that the most severe and intractable cases (the gummata being frequently multiple) are those that develop early after infection.

This chapter on tumours contains a great deal of original matter, as well as comprehensive references to many published cases, showing that the author has given special attention to this important subject.

The chapter on cortical localisation, upon which Professor Mills is a recognised authority, is a valuable and comprehensive epitome of the subject. We do not agree with the author, however, in assigning sensory functions, cutaneous and muscular, to the limbic lobe. There is no reliable evidence forthcoming, from observations on man, that this region has this particular function, nor do we think experimental evidence is sufficiently safe to support this view. We consider that the author's single case of tumour of the temporal lobe, involving especially the posterior part of the medi-temporal convolutions, is suggestive, but hardly sufficient to warrant the foundation of a *naming* centre in that region. In connection with the subject of aphasia, we are somewhat disappointed to find no mention of the admirable work of Bastian.

On page 564 the author states that Tooth has traced secondary degeneration in the antero-lateral tract of Gowers to the exit of the sixth and seventh cranial nerves. It is generally acknowledged that its course can be traced to the vermis. On page 615 the author attributes birth palsies to prolonged and difficult labour, and the use of instruments. Gowers and many other eminent authorities have pointed out that labour has been in these cases often rapid and free from difficulty.

The chapters on the special senses are particularly full and interesting.

In the first edition of a large treatise such as this, which has no doubt occupied the author's attention for many years, errors of omission to recent advances must necessarily have crept in here and there. These, however, are few and far between. The book is extremely well illustrated, the photographs of clinical cases being well executed. There is a useful bibliographic index; and altogether we can strongly recommend the work to all who are interested in neurology. We hope that the success of this volume will induce Professor Mills to complete the work by bringing out the promised second volume.

F. W. Mott.

Diseases of the Nervous System. By C. E. Beevor, M.D. (Lond.), F.R.C.P. London: H. K. Lewis. 1898.

In writing a text-book the author must decide what he wishes to accomplish—a full and yet concise compendium of what a senior student and a well-read practitioner ought to know, or an encyclopædia on the subjects under consideration. Dr. Beevor has aimed at the former, and has done an extremely difficult task ably and well.

His description of the anatomy and physiology of the cord and brain may be a little abstruse over points of special interest to the author, but no individual theories are stated dogmatically. Most of the diagrams are both original and good, although it seems a pity that a pictorial sketch of the cerebral circulation has not been included. The book contains admirable short accounts of the more important diseases of the nervous system, and care has been taken to help the memory by grouping together diseases similar in clinical features. The outstanding feature of a great teacher is his power of impressing upon the mind the salient points of a disease, and perhaps in a very few instances the author might have kept this more in view. In describing, for instance, locomotor ataxia, primary spastic paralysis, etc., a preliminary reference to the sites of the lesions, with their probable functions, would have greatly aided the student in remembering many of the typical clinical features otherwise so puzzling. With these exceptions, it is difficult to particularise any individual part of the book which is better than another; all is alike good, and well worth the careful perusal of practitioner and student alike. Certainly the short chapter on aphasia is admirably clear and concise, and the functional diseases of the brain deserve special praise. One omission, perhaps intentional, is a reference to or discussion of the possible infective nature of poliomyelitis anterior acuta. A concise index adds to the value of a handbook that fills a niche in the medical library which, if not quite empty, is by no means overstocked. Robert A. Fleming.

The Living Organism: An Introduction to the Problems of Biology. By **Alfred Earl**, M.A. London: MacMillan & Co. 1898.

As Mr. Earl points out in his preface, the conceptions derived from biological research exert a far-reaching influence on many departments of thought, so that inaccurate conclusions and unsound speculation have more serious issues in the case of biology than in any other of the natural sciences. Many of the doctrines have an obvious relevancy to human affairs, many of them lend themselves very readily to popular discussion, and there are never wanting enthusiastic spirits who, with the best of intentions, "give to the ignorant as gospel in the name of science," as a cautious thinker phrases it, "the rough guesses of yesterday which to-morrow will forget." It is to be feared, moreover, that there is little in the biological education of the schools which leads the student to a criticism of his categories; in other words, to a philosophical consideration of his problems. With the utmost *naïveté*, and with the most complacent self-satisfaction, such words as "organism," "species," "protoplasm," "sensation," "adaptation," are bandied about, as if all was plain sailing, as if the possibility of intellectual shipwreck was inconceivable, as if each of these words did not cover an unsolved problem. The value of Mr. Earl's book, which is critical rather than constructive, is that, in an intelligible and forcible manner, it disturbs the complacency of our craft. Its keynote is "the unity of the organism," as was that of Mr. Sandeman's "Problems of Biology," with which this work has much in common; and though he does not indeed reveal what the unity is, he will win conviction from the unprejudiced as to this at least, that "the addition of all the chemical and physical changes does not yield the organism as a whole." J. Arthur Thomson.

Degeneracy: its Causes, Signs, and Results. By **Eugene S. Talbot**, M.D., D.D.S. London: Walter Scott, Ltd. 1898.

The fact expressed by the term degeneracy is, that an organism may change in a direction opposite to that of evolution, towards the indefinite and homogeneous instead of towards the definite and heterogeneous—that it may, in other words, fail to attain the normal level of the stock. The degeneration may come about in various ways—(*a*) It may be the result of a germinal variation of whose nature nothing is known, as when a "reversion" follows an unsuitable cross; (*b*) it may be the result of a germinal variation which can with more or less probability be traced to disturbed nutritive conditions in the parent; and (*c*) it may be the result of individual "modification," affecting the body of the offspring at some period of its development and growth. These are the three possibilities of origin which are recognised by all biologists, which the author, however, fails to distinguish with sufficient clearness. Not that he is unaware of them, but simply that he mixes them up. He also recognises, with a confidence which would be irritating if it were not amusing, a fourth possibility—the inheritance of acquired degenerate modifications as such, basing his belief partly on a series of inconclusive results in regard to circumcision and mutilations, partly on a number of cases whose irrelevant nature has been repeatedly pointed out, and partly on a misunderstanding of one of Weismann's concessions. He bluffs his readers by his short and easy method of dealing with Weismann's position, which he fancies has been abandoned by the admission that maternal nutrition may play a part in determining variation—a fancy which shows that the critic does not understand what he is criticising. In fact, the whole treatment of heredity in this volume is lax and unsatisfactory.

Dr. Talbot's particular thesis is, that the factors inducing degeneracy especially affect those checks on excessive activity which have been acquired during the evolution of the race, and thus produce a state of nervous exhaustion, in consequence of which the offspring fail to rise to the normal level of development. As it is admitted by all that the reproductive cells may be affected by the nutritive conditions of the parents, and as nervous disturbances tend to induce nutritive disorders (and *vice versâ*), there is no difficulty, from Weismann's point of view, in believing that the neurasthenia of the ancestor may be followed by the neurosis of the descendants. The book contains an eminently readable exposition of the various forms of degeneracy, both bodily and mental, and is a treasury of vivid facts, some of which, however, seem barely credible. We should like exceedingly, for instance, to hear more about the experiments on white mice made by Mr. George Roe Lockwood of New York.

Many parts of the book appear to us to be admirably done, and there is a refreshing breadth and tolerance in the author's practical conclusions on the prophylaxis of degeneracy, but we are bound to say that the work is very unequal, and that even the words often betray that the writer's reach has exceeded his grasp, as when he refers to the experiments of Maupas on Infusorians as bearing on *parthenogenesis.* One might as well speak of the parthenogenesis of potatoes.

Dr. Talbot is strangely uncritical and rash in regard to many points, *e.g.* the evidence of maternal impressions afforded by the deformed chicks whose mother had been frightened by a parrot, or the interpretation of human cyclopean monstrosities as reversions to the single-eyed sea-squirts, or the statement that all albinos are degenerate. It is a pity, too, that a work which is "the result of more than twenty years' labour" should be blemished by frequent carelessness, for what is *Artemsia salina*, and who is Moquia-Tanden, or Moquia-Tandea as the index has it, what evidence is there that "a worker bee may be an offspring of an unimpregnated queen bee," or that "as a rule the parthenogenetic offspring are themselves incapable of further procreating their kind"? and, alas, instances are all too numerous. We notice also with regret that the author does not always sufficiently indicate where his excerpts from books end. At the same time, his work has much merit, and should be of much value to educators and parents, for whom it is specially intended. Moreover, if the author has not always succeeded in separating the chaff from his wheat, he has produced a book which has no nonsense or gas in it, and this separates it markedly from some other recent works dealing with degeneracy.

J. ARTHUR THOMSON.

Die Lehre von der Pellagra: ætiologische, klinische, und prophylaktische Untersuchungen. Von CESARE LOMBROSO. Unter Mitwirkung des Verfassers deutsch herausgegeben, von Dr. HANS KURELLA, Nervenarzt in Breslau. Berlin: Oscar Coblentz. 1898.

ALTHOUGH pellagra is a disease still unknown in this country, yet it possesses more than a mere scientific interest, for it is related to other ailments which own an analogous causation, and it is not improbable that minor forms of such have been, through ignorance, overlooked or misinterpreted in the past. Lombroso has for nearly thirty years striven to obtain recognition of his view of its origin, and has at last been successful. History and statistics alike indicate its association with the use, as food, taken habitually made of unsound maize. It is conceded that the disease is due, not to any one micro-organism in particular, though some may be more potent for evil than others, but to the toxins which arise from their influence or in their presence. This conclusion can no longer be rejected, when the most characteristic phenomena of pellagra can be experimentally evoked by the exhibition of the toxins, but not by the bacteria of decayed maize. Thus the *Bacterium maidis* develops slowly in white bread, rapidly in polenta. Fresh cultures of this and of *Oidium lactis* are harmless, but the extract of polenta which has been infected with these is lethal. This goes to prove that micro-organisms, innocent in themselves, can give rise to noxious products; but this entirely depends on the constitution of the medium in which the development takes place, and from the deeper or superficial alterations which occur therein. The organisms may indeed be the indices, perhaps the associates, of the chemical processes which bring about the formation of the toxins.

It is somewhat curious that the name attached to the disease refers to one of the least serious of its features, one which is occasionally absent, while the grave nervous disturbances, which include not merely motor but mental disorganisation, have scarcely received the attention they merit. The wrecks of humanity to which those who have long suffered from it are reduced, are in evidence in all the lunatic asylums of North Italy. Pellagra has points of contact, not only with ergotism and lathyrism, but also with strychnia poisoning and alcoholism, and these have been diligently worked out by Lombroso in support of his thesis. Cooking, even if efficiently carried out, does not wholly neutralise the poison, and possibly alcohol distilled from such maize may retain toxic qualities, and thus account for the exceptional instances quoted by the opponents of the theory, where apparently no maize has entered into the dietary.

The treatise furnishes a most complete account of pellagra. Starting with its distribution in Italy, it shows how it follows in the track of the musty grain, whether that be the ill-ripened product of an inferior kind in the place where it is grown, imperfectly dried and badly stored, or where imported maize of bad quality has replaced a good local crop. In describing the features of the complaint, it is observed that many symptoms prominent in certain districts are entirely absent in others. A very important type, which has not hitherto attracted sufficient notice, is what Lombroso calls pellagra typhus, a peculiar and dangerous febrile state, supervening in some cases. This is plainly not a mere complication with enteric fever, but is more probably an acute form of the customary spinal disturbance. This acute exacerbation may at one time be referred to uræmia, at another to the presence of infective pneumonia, but chiefly, perhaps, to a more intense action of the maize poison on the spinal cord, either ascribable to accumulation or to increased conveyance thither of a substance of possibly greater toxic efficacy. A large section is devoted to the pathological anatomy, deduced from 113 autopsies, but this could hardly be summarised here. Pigmentation is noticeable, and this is explained as due to the premature senility of those affected, which is also expressed in the atheroma and brown atrophy of the heart.

The prevention consists in more careful treatment of the maize during and after harvest, so as to obtain a well-ripened and well-dried grain of good quality; in unsuitable localities, the substitution of other crops, such as potatoes, for maize. The establishment of exsiccators for artificially drying the maize has done much good where the government has introduced them. When it has attacked an individual, the unwholesome food must be abandoned, an improved dietary put in its place, and arsenic administered, which appears to exert a distinctly controlling influence. In course of his investigations, Lombroso tried the tincture of the mouldy maize in various skin diseases. The most marked results occurred in thirteen cases of old-standing psoriasis. Of these, four were cured and six materially improved. In almost all, more or less severe digestive and nerve symptoms appeared, and with the advance of the phenomena of intoxication did an improvement in the cutaneous affection manifest itself. He regarded this as an instance of cure by substitution, in so far as by bettering the vasomotor innervation of the disordered tissue, the disturbance of nutrition was equalised.

W. ALLAN JAMIESON.

Hypnotism and its Application to Practical Medicine. By OTTO GEORGE WETTERSTRAND, M.D. Authorised translation (from the German edition) by HENRIK G. PETERSEN, M.D.; together with *Medical Letters on Hypno-Suggestion, etc.*, by HENRIK PETERSEN, M.D. London and New York: G. P. Putnam's Sons. 1897.

THE above is not, as the title seems to imply, a text-book of hypnotism, but simply a record of cases. One hundred and twenty-eight are cited, many of them interesting and valuable; but, taken as a whole, they compare unfavourably with Wetterstrand's later publications. It is obvious that a work written in 1890 must now be out of date, more especially in a science which has made such rapid strides as hypnotism. Little is said of prolonged sleep, a method of treatment which Wetterstrand has made peculiarly his own; while the comments on theory and phenomena are scanty and misleading Thus: "It is often very easy," Wetterstrand says, "to get in *rapport* with sleeping persons." . . . "This fact particularly is of great theoretic interest, as it demonstrates that hypnotic sleep is nothing but natural sleep with the additional *rapport*." To this it might be objected that it is still more easy to get in *rapport* with waking persons, and, further, that the fact that one state can be changed into another, does not prove that they are identical.

From beginning to end the translation is faulty. The German construction is followed as far as possible, and often with startling results. Thus: "She was short-breathed when going upstairs." . . . "The head felt to her ice-cold." . . . "She called me in as a last resort." . . . "I have cured by hypno-suggestion a condition of weakness in a lady's right arm, which previously had been unable to lift a chair." . . .

"One can give either the medicine during the sleep, or say to it in the sleep that it is to take the medicine voluntarily."

A nervous child is thus described by her mother: "She is like that the whole day and gives me not a moment's peace; if I leave her she begins to shriek loudly." . . . "She always spit out the medicine." No wonder Wetterstrand despairingly asks, "How then to treat the the child under such circumstances?"

In reference to a somnambule, Wetterstrand is made to say, "Nose-bleed could be produced, fire-blisters raised on his skin; the number of heart-beats could also be reduced; but this I do not intend to describe with any detail, as I hope a physiologist, Professor Tigerstedt, who lives here, will conduct experiments with him in that direction." Nose-bleeds and fire-blisters are good. It is to be regretted, however, that the result of the Professor's researches have not been added to this edition. It is doubtful whether, in any cases of this kind hitherto published, the experiments have been conducted in so rigidly scientific a manner as to exclude all sources of error.

Some of the mistakes in translation alter the sense. Thus, Wetterstrand is made to say: "The majority of people, it seems, can be brought into any of these conditions (hypnotic stages)." As only a small proportion become somnambules, this is obviously incorrect. In turning to the original, however, we find that the true rendering is "one or another" ("in den einen oder andern der genannten Zustände").

The latter part, amounting to a third, consists of the translator's "Medical Letters." These have been written at irregular intervals since 1891, and are entirely out of place here. The following, taken from the preface, fairly represents style and matter: "We ought not, however, to forget that theories must necessarily be experimented with, before their intrinsic value in relation to facts can be ascertained. Nevertheless it is also true that from facts have often been evolved theories, but this reversed position may perhaps be looked upon as a more fortunate one." . . . "Intelligent and lucid dissemination of psychological medicine is needed, we must confess, in this country, more than elsewhere, where, perhaps, such knowledge has long been riotous or apathetic."

Dr. Petersen believes in telepathy. "Suggestion as projected will," he says, "is powerful enough to break down intervening obstacles whenever encountered." . . . "To us there seems to be a logical sequence and similarity between wireless electric light and speech and wordless suggestion." The experiments cited in proof of these statements are unworthy of serious consideration.

The fallacies of the Nancy school are reproduced. Hypnotism is explained by automatism, loss of consciousness, concentration of attention, etc., while the possibility of suggesting crime to an honest man is asserted to have been demonstrated by both schools. Now, the Salpêtrière school has always denied the possibility of suggested crime, and no one has ever demonstrated that an honest man could be transformed into a criminal by means of hypnotism. Bernheim's experiments only show that hypnotised subjects frequently perform simple harmless acts, even when told that these were criminal ones. Recent scientific researches, as to the mental states involved in hypnosis, have clearly shown the erroneous nature of the Nancy theories, especially in reference to so-called automatism and crime. The phenomena of hypnotism are now supposed to be due to the intelligent action of a secondary consciousness.

According to Dr. Petersen, "American contribution to this science (hypnotism) has hitherto come almost exclusively from non-medical circles, the papers being often well written and clearly defined, and on many occasions the writer has been singularly impressed by noticing that the Old World's scientists adopt and adapt transatlantic notions in this direction, although not sanctioning them in the original form or acknowledging their primary authority." Such plagiarism is, of course, disgraceful, and it is to be regretted that Dr. Petersen omits to supply us with the names of the sinning "Old World's scientists," along with a description of the "transatlantic notions" which they have so unblushingly appropriated.

Wetterstrand justly occupies a high place amongst those who employ hypnotism, and it is to be regretted that the English-speaking public should only be allowed to judge of him through the medium of a book which ought never to have been published in its present form.

J. Milne Bramwell.

Die Functionsstörungen des Grosshirnes. By Prof. Albert Adamkiewicz, Vienna. Hannover, 1898.

The book before us is essentially an original and clever method of treating in a systematic way the so-called functional disturbances of the cerebrum. The manner of grouping the subjects is ingenious, their consideration systematic and comprehensive, while the descriptions are short, concise, and to the point, in which respects the treatise, if such a term may be fairly applied, differs from many similar ones published in the German language. Although the book appears to have been written from a chiefly neurological standpoint, it may prove of value to the psychologist as well as to the alienist physician. We have not before seen an endeavour to bring the subject of cerebral disease, in its varying aspects, pyschological and neurological, organic and functional, into harmony, such as has been here attempted. Let us for one moment extract the contents of a chapter, by way of illustration. In that upon stimulation, irritation, and paralysis of the cerebral cortex, we notice the following subjects discussed. As examples of simple irritative phenomena, we have headache, elevation of the temperature, trophic disturbances, and polyuria; as periodic irritation conditions, eclampsia and epilepsy; and as continued irritation states, chorea, paralysis agitans, stammering, and tremor. Hyperstimulation of the cortex is manifested in neurasthenia, hysteria, and the traumatic neuroses; paralysis of the cortex in stupor, coma, shock, and apoplexy. Or again, under the functional disturbances of the cerebral cortex, we find described, anomalies of sleep, insomnia, and allied states, hypnosis and lethargy. And here also are considered normal and pathological dreaming, somnambulism, and post-epileptic states.

From this it is evident that a contribution of great interest and much originality has been added to current neurological literature. The author appears to have had large experience in the so-called functional nervous disorders. The work before us is entirely a philosophic study, no attempt being made to consider the treatment of the various conditions which are described.

W. Aldren Turner.

RECENT ADVANCES IN MEDICAL SCIENCE.

MEDICINE.

UNDER THE CHARGE OF

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Artificial Food Preparations.

II. Derivatives of Proteid Digestion in a Dry State.—

Somatose.—Hildebrandt (*Centralbl. f. innere Med.*, Leipzig, 1898, Bd. xix. No. 20); Taube (*Wien. klin. Rundschau*, 1895, No. 52); Taube (*ibid.*, 1896, No. 16); Drews (*Wien. med. Presse*, 1897, Nos. 5–17; *Centralbl. f. innere Med.*, Leipzig, 6th June 1896; *Allg. med. Centr.-Ztg.*, Berlin, 1894, No. 34); Kuhn and Völker (*Deutsche med. Wchnschr.*, Leipzig, 1894, No. 41); Metall (*Wien. klin. Rundschau*, 1897, No. 36); Bartley (*Med. and Surg. Reporter*, 1894, No. 6); Neumann (*Munchen. med. Wchnschr.*, 26th January 1898); Bornstein (*loc. cit.*); Neumeister (*Deutsche med. Wchnschr.*, Leipzig, 1893, No. 36); Wolff (*Allg. Wien. med. Ztg.*, 1896); Hause (*Arch. f. Verdauungskr.*, Berlin, Bd. iv. S. 248); Maasen (*Wien. med. Wchnschr.*, 1898, No. 1).

Hildebrandt is no enthusiastic supporter of the benefits frequently ascribed to somatose as an easily absorbable and nourishing food. He states that the small portion of somatose absorbed from the bowel is only equal in value to the nitrogen obtainable from flesh. In amounts of 20 grms., or more, it is badly borne, much is lost in the fæces, and its cost is much too great. He doubts the accuracy of many of the reported observations made upon it.

Kuhn and Völker regard somatose, from their experimental researches, as a nutrient of good value, unless given in large doses; especially of benefit in phthisical cases, with tubercular affections of the bowel; but of no value in the form of enemata;—while Bornstein says, "Small quantities of somatose, a 'Kaffeelöffel' thrice daily, are borne well, at least for a time; but it does not form a sufficient diet when taken without additional food." Somatose is more of the nature of a stomachic than a nutritive substance, capable of carrying on the metabolic changes.

Taube employed somatose in 20 grms. at first, later in 10 grm. doses daily, in a case of gastric ulcer and one of gastric cancer with extreme cachexia. Various other nutritious and artificial foods had led to no good results. The first patient gained 18 lb. in ten weeks, the second felt better and put on some flesh. He recommends somatose as the most valuable of such preparations, and especially as valuable in mercurial cachexia. Taube also, in another paper, details the successful results following its use in mercurial cachexia and acute rheumatism, and draws attention to its power of increasing the milk flow, or even of starting the flow again, should it have disappeared. Drews, as well, treats of this property of somatose, alleging that it has a specific action upon the mammary glands in nursing mothers, causing a free flow of milk, and removing any local pains or discomfort; recurring in his later papers to the subject, and adding, that it increases the strength of the heart-beat and of the blood tension.

Maasen considers somatose as equal in proteid value to many times its weight of lean beef, possessing a total nutritive value which is not all available in actual practice. He finds that 8 grms. daily, in addition to the usual food, very shortly brings about a marked increase in the proportion of hæmoglobin in the blood of anæmic patients; causing better appetite, loss of pallor, and greater feeling of strength, in such cases.

Weiss (*Die Heilkunde*, 1898, Heft 9) describes clearly the heat, food, and nutritive value of the common articles of diet; and, as well, of many artificial food products, especially of the proteid class. He places somatose at the top of the series. Wolff praises its addition to milk for children, to render it more digestible and nourishing, while Hause gives a warning against the danger which may arise of setting up albumosuria. He observed this symptom in a boy receiving 60 grs. of somatose daily. He also notes that it is insoluble in red wine.

Iron-somatose.—Goldmann gave iron-somatose to about thirty patients, for a period of from four to six weeks in each case; all were chlorotic girls (17 to 22). They did not cease working, nor change their diet. Although the actual increase in hæmoglobin, as shown by Fleischl's apparatus, was by no means marked in several of them, all testified to a pronounced improvement in well-being. The most obstinate cases of previous constipation reported daily evacuations while under treatment. The appetite was much improved. Thrice daily before meals, 2·5 grms. (38·5 grs.) were given, dissolved in a little warm water, and then added to milk, coffee, or soup, as fancied. No dislike to the iron-somatose was shown; no teeth blackened; and every indication afforded that it is a valuable iron compound for use in chlorosis, especially as it tends to remove constipation rather than cause it.—Goldmann (*Allg. med. Centr.-Ztg.*, Berlin, 1898, No. 49).

Milk-somatose with tannin.—Schmidt (*München. med. Wchnschr.*, 1897, No. 47) has found that the addition of tannic acid to milk-somatose entirely removes all the tendency to irritate the bowel, possessed by somatose when given in considerable doses alone. The tannin is added to 5 per cent. of the total, and in his hands it has proved a bland, non-irritating preparation, given in doses of a teaspoonful, thrice daily, even to patients with easily irritated bowels. It may, however, be given up to 50 grs. (770·0 grs.) per diem; and a useful form for administration consists of dissolving 2 drms in boiling water along with some meat extract, forming a bouillon.

Alcarnose.—Brandenburg and Hupperz (*Deutsche med. Wchnschr.*, Leipzig, 1898, No. 20, Bd. xxiv.).—Alcarnose is a mixture of albumose and maltose, and has been employed by the authors mentioned above, as the basis of nutritive enemata, with very encouraging results. A large proportion was absorbed, no increase of intestinal putrefaction was caused, and no signs of local irritation could be detected.

Knauer (*Allg. med. Centr.-Ztg.*, Berlin, 1898, No. 53) regards alcarnose, from practical experience, as of great value for the support of patients suffering from metabolic disturbance, because it affords an easily digested and completely assimilable proteid food substance to the organism. He has been able to support patients almost solely upon alcarnose for a number of days; while the metabolic balance under such circumstances was upon the positive side.

Hiller (*Ztschr. f. Krankenpfl.*, Berlin, Bd. xix. S. 54 and 92) appears to have devised alcarnose. It consists of albumoses, 24·8 per cent.; extractives and muscle salts, 2·3 per cent.; predigested carbohydrates, *i.e.* dextrin and maltose, 67·1 per cent.; and of inorganic salts, 6·8 per cent. It has no smell, and a faintly sweet taste.

Sanose.—Schreiber and Waldvogel (*Deutsche med. Wchnschr.*, Leipzig, 1897, No. 41) discuss the properties of sanose, a compound of 80 per cent. casein and 20 per cent. albumose. It reacts much like human milk, is digested at 40° C. by peptic and tryptic ferments, and appears to be of more value than nutrose, and almost equal to eucasin. It is a white powder without taste or smell, best given suspended in milk.

Rose's Milk Preparations.

Hartkopf (quoted *Ztschr. f. diätet. u. physikal. Therap.*, Bd. ii. S. 57) describes Rose's artificially made milk. It contains casein from cows' milk—dried, finely powdered, and mixed with the exact amounts of milk sugar, salts, fats, and extractives necessary to resemble human milk. It is aseptic. For diabetics, the milk-sugar is omitted and saccharin added. Of the ordinary form two strengths are made, one with more proteid for very young infants, the other, with somewhat less, for older ones. Hartkopf in his book reports a great measure of success from the use of these preparations, and values them highly.

Liebig's Meat-Peptone (Kemmerich's Meat-Peptone).

Zinn (*München. med. Wchnschr.*, 1896, No. 46) notes that this preparation is really a mixture of albumoses and peptone. It is manufactured on the lines suggested by Kemmerich, wherein the proteids in meat are simplified by the action of steam at a high pressure. Zinn, from his own experience, regards it from its appearance, taste, stability, and constant composition as the best preparation of the kind hitherto offered.

Kohlenberger (*ibid.*, 1896, No. 47) gives a 10 per cent. solution of this substance (30–40 c.c.) in enemata, and finds that the proteids in it are completely absorbed by the bowel wall. Zinn gives 20–50 grms. per diem; Kohlenberger's dose is equal to about 46–61 grs. in each enema.

Meat-peptone—a heart tonic.—Kemmerich (*Berl. klin. Wchnschr.*, 1894, No. 10) found that the ingestion of Kemmerich's meat-peptone caused the pulse to become fuller and stronger, an effect which was enhanced after some weeks, owing to the stimulating action of the food upon the heart muscle, the appetite, and nutrition.

MEAT EXTRACTS.

VOIT (*München. med. Wchnschr.*, 2nd March 1897).—Commercial meat extracts represent only 3·1 per cent. of the original meat, or 10·5 per cent. of the same when dried. They are rather appetisers and stimulants than foods. Extractives undoubtedly aid digestion and stimulate the bodily functions, and, as such, are invariably included in the diet of both animals and man. Meat extract, therefore, is of undoubted utility as an addition to the food of patients, forming the best form of "pick-me-up" attainable.

In many of the recent extracts, certain bodies of organic origin are to be found non-nitrogenous, as glycogen; nitrogenous, as soluble products of proteid digestion :—

	Kemmerich.		König and Bömer.		Slutzer.	
Water			...		...	
Glycogen	0·5 – 1·3 per cent.		...		...	
Gelatin	6·0	33·2 ,,	2 per cent.	6·5 p. cent.	...	
Albumose	10·0		4·5 ,,		3·5 per cent.	20·5
Peptone	12·3		Trace		17·0 ,,	
Other soluble proteids	4·9		...		...	

PEPTONE.

CAHN (*Berl. klin. Wchnschr.*, 1893, No. 24) considers the question of the value of peptone as an article of diet very fully, concluding the preparations containing peptone (and albumoses) are of use in cases of diminished peristaltic movements, especially in anæmia, chlorosis, and convalescence from acute illness.

MEAT POWDERS AND EXTRACTS, PEPTONES AND ALBUMOSE.

BARDET (*Bull. gén. de therap.*, Paris, 30th March 1898) divides these preparations into two classes—(1) Those still undigested, or only in part; (2) those fully predigested. The substances in the first class may further be separated into those of the nature of a food, and those of more artificial form. The first of these subdivisions forms the class common in France, the second that found in Germany.

Preparations of casein of French manufacture, biscuits of legumin, scones made from vegetable albumins, are recommended for diabetes and gastric fermentation. The German casein preparations do not impress him as of being nearly so useful as the casein present in ordinary foods. The calcium-casein of Röhmann, and the artificial vegetable milk of Lahmann, present no very good reason for their use. He draws attention to the effect of the national habits upon the form of artificial food most approved of by the French and the Germans. The French prepare most of their food in a fresh unaltered form; the Germans alter the properties and characters of theirs in a marked way. He especially regards Adrian's meat-powder, made from roast flesh, with favour, and says a good word for somatose.

Albumose-milk.—Schreiber and Waldvogel (*Deutsche med. Wchnschr.*, Leipzig, 1898, No. 32). The authors have taken advantage of the result obtained when casein is precipitated in the presence of casein, albumose (caseose), whereby it forms much finer flocculi, and by adding 0·4 grm. caseose to 125 c.c. of milk prepared, or milk of the proteid in which one-half can be digested *in vitro*, compared with one-ninth of pure milk.—Hamburg (*Berl. klin. Wchnschr.*, 1896, No. 35; *Centralbl. f. d. med. Wissensch.*, Berlin, 1897, S. 31).

HYGIAMA (Stüve, *loc. cit.*) is in the form of a powder, easily soluble in milk, hot or cold water. It is prepared from concentrated milk, grain after special treatment, and cocoa which has been deprived of part of its fat. It contains 63·4 per cent. of carbohydrates, 10 per cent. of fat, and 20·4 per cent. of proteids. Given to patients on an exclusively liquid diet, Stüve found that it gave very satisfactory results.

ENTEROROSE.—Grünfeld (*Wien. med. Bl.*, No. 6, 1898) describes the properties and characters of enterorose. It is a vegetable product, consisting mainly of wheat and oatmeal dextrins (formed both by diastatic action and through heat), which, when meat-extract, milk-butter, and salts have been added, constitute a non-irritating and easily absorbable mixture, similar in composition to natural foods. Grünfeld also assigns to it an antiseptic action in the bowels, in virtue of which it checks diarrhœa, although even here it is absorbed to an appreciable amount. For adults with diarrhœa, a tablespoonful; for children over two years of age, one dessert-spoonful; and for those under two, a teaspoonful, every one or two hours,—are the quantities recommended, the dose corresponding to the severity of the symptoms.

FOOD SALTS.—Blandberg (*Arch. f. Hyg.*, München u. Leipzig, 1897 (2), Bd. xxx. S. 95) could not substantiate the statements made by the maker in favour of the benefits to the body caused by administration of Gericke's food salt, and cannot recommend the use of Lohmann's extract of vegetable food salts.

NUTRITIVE SALT SOLUTION.—Morris (*Med. and Surg. Reporter*, Philadelphia, 1894, No. 3), in six cases of refusal to take food among lunatics, obtained striking results by injecting into the gluteal region half a litre of sterilised water, in which the whites of two eggs and about 2 grms. of common salt were dissolved.

SURGERY.

UNDER THE CHARGE OF

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SEPTIC INFECTION OF THE URINARY BLADDER.

AN authoritative report on this subject was presented to the French Association of Urology in October 1898 by MM. Albarran, Hallé, and Legrain. It includes a critical résumé of the recent literature on the bacteriology of the urinary tract. The following is a brief extract of the paper, which extends to forty pages of the *Ann. d. mal. d. org. génito-urin.*, Paris, November 1898.

After certain preliminary and historical considerations, the report deals—(1) With the agents concerned in infection of the bladder, (2) with the methods of origin and channels of infection, (3) with the results of the entrance of infective agents into the bladder, and (4) with their diagnosis, prognosis, and treatment.

1. THE AGENTS CONCERNED IN INFECTION OF THE BLADDER.—So far as these are known at present, they may be grouped into those met with (*a*) commonly, (*b*) rarely, (*c*) exceptionally. Among those commonly present, the first place is given to the *Bacterium coli*. Out of 304 cases, reported upon by ten different observers, it was met with in 131 (or nearly 50 per cent.), and in eighty-nine of these it was present in a state of purity. The *Staphylococcus pyogenes* (*aureus* and *albus*) comes next in frequency, seventy out of 304 observations. In twenty-six cases, the *proteus* of Hauser was the chief infective agent. The relative frequency of the *Streptococcus pyogenes* has not yet been determined with certainty, but it would appear to rank next in order. The gonococcus was found in ten cases, but more recent observations tend to show that it is more often the sole infective agent than is generally accepted. It should be noted that the tubercle bacillus has been excluded from the present inquiry.

The rarer organisms include the typhoid bacillus, Fränkel's pneumococcus, Friedländer's diplobacillus, the *Bacillus longus ureæ*, the *Diplococcus ureæ liquefaciens*, etc. If the infection of the bladder were always due to a single genus in a state of purity, the bacteriology of vesical infection would be comparatively simple, but in a great number of cases the organ is infected simultaneously or consecutively by several kinds of organism; in other words, the infection is often a mixed one, especially in cases of long standing which have been under treatment.

2. METHODS OF ORIGIN AND CHANNELS OF INFECTION.—The organisms may reach the bladder either *directly* through one of its normal openings or breach in its wall, or *indirectly* by way of the blood or lymphatic vessels.

Direct infection through the urethra.—The atmospheric bacteria are unable to travel upwards along the healthy urethra. It is possible that the organisms normally present in the urethra may be pushed onwards into the bladder by instruments, but the frequency of such infection is difficult to estimate. When there is disease of the urethra, the bacteria lodging in its walls are very likely to give rise to infection of the bladder. When the urethra, for example, contains any secretion rich in pathogenic microbes, vesical infection is easy and frequent, especially if instruments are passed, whether these be sterilised or not, and whether the anterior urethra is disinfected or not. In stricture of the urethra there are recesses in its walls in which organisms lodge and flourish, and these latter are impossible to eradicate. Under such conditions, the passage of a catheter infects the bladder with great facility, and spontaneous infection apart from instruments may be regarded as quite within the range of possibility. In the male there are many lesions which favour infection by way of the urethra, *e.g.* dilatation of the neck of the bladder and prostatic urethra, incontinence, phimosis, balanitis, hypospadias, etc. In the female, the anatomy of the urethra, the septic surroundings of the meatus, vulvo-vaginitis, weakness of the sphincter in prolapse of the vagina, the conditions attending the puerperal state, all favour spontaneous infection of the bladder by the same route. It may be stated generally that the organisms of the diseased urethra are the most frequent agents of bladder infection, that the latter may take place spontaneously, but that it follows much more commonly upon instrumental interference.

Direct infections through the ureter.—Under certain conditions it may be assumed that the ureteral orifices pour into the bladder a urine which is more or less highly charged with microbes. This is the so-called renal or descending infection of the bladder. The predisposition of the urinary organs to suffer from the secondary results of general infections is generally recognised. Apart, however, from very evident or pronounced general infection, bacteria may be excreted by the kidneys, having merely passed through the general circulation and having been derived in the first instance from the intestines, and from septic lesions of the skin and mucous membranes in general. When the kidneys are themselves the seat of septic disease, the occurrence of bladder infection is easily understood.

Direct infections through breaches of continuity in the wall of the bladder are relatively very rare. As belonging to this category we may mention—vesical fistulæ of all kinds, persistent urachus, extroversion, wounds, abscesses rupturing into the bladder, uterine, vaginal, and intestinal new growths, which ulcerate into the bladder, etc. In *indirect infections through the circulation* the organisms do not contaminate the vesical contents in the first instance, but exert their pathogenic influence on the tissues of the bladder wall. No one has so far succeeded in demonstrating bacillary embolism of the blood capillaries in general infections. Auto-infection from septic lesions of the organs and tissues in the vicinity of the bladder must be accepted, however, as proved, and in these cases the infective agent concerned is mainly transmitted by way of the lymphatics.

3. Regarding THE RESULTS OF THE ENTRANCE OF SEPTIC AGENTS INTO THE BLADDER, it is generally accepted that organisms may gain access to its

interior, and yet fail to lodge there. Unless certain additional favouring conditions are present, their effect may be nil. Provided these conditions are present, the organisms may contaminate the contents only and cause the condition known as *bacteriuria*. This may be regarded as the first stage or degree of vesical infection; it must be admitted, however, that the border line between bacteriuria and cystitis is very uncertain. Under different conditions the same organism may cause one or the other. Most authors describe the colon bacillus as the most constant agent in typical cases of bacteriuria. When the wall of the bladder suffers from bacterial infection, the condition is termed *cystitis*. While it does occur, very exceptionally, that the wall is initially infected by organisms carried to it through the lymphatics, and the urine is then infected secondarily, in the great majority of cases the order of events is reversed; the germs enter the bladder through one of its normal openings, they thrive in the contained urine in the first instance, and act later on its walls. The chemical changes in the urine, and the addition to it of bacterial products, play an important part in the lesions of the lining mucous membrane, and especially in lowering the vitality of the lining epithelium. The inflammation is at first superficial (or catarrhal); it may become suppurative, interstitial, phlegmonous, vegetative, ulcerative, pseudo-membranous, or gangrenous. It may spread to the surrounding cellular tissue or peritoneum. These widely varying results of bladder infection may be regarded as dependent, on the one hand, on the qualities of the infective agent as to genus, number, virulence, etc.; and, on the other, on the local conditions of the viscus infected, *e.g.* retention, congestion, injury from instruments, foreign bodies, stone, etc. These are generally recognised as of the greatest clinical importance. The report concludes with remarks on the diagnosis, prognosis, and treatment of vesical infections; for these we must refer our readers to the original article.

JOINT LESIONS IN HÆMOPHILIA.

MM. **Sabrazès** and **Cabanus** report a case of the above, in which skiagraphic examination of the joints proved of considerable value. The patient was a male, æt. 35, whose mother had bled freely after trivial injuries. The hæmophiliac tendencies of the patient were first shown at the age of 7, when an insignificant bite of his tongue bled for fifteen days, in spite of all sorts of applications, and only ceased on compression with forceps. He had a similar hæmorrhage from the tongue at the age of 10. There were similar outbreaks, following different superficial injuries, at intervals of two or three years. He also observed that extensive ecchymoses followed slight bruises. From the age of 4 or 5 he had been liable to joint troubles, recurring at intervals of about two months, appearing first in the ankle-joint, and characterised by the rapid appearance and equally rapid disappearance of fluid in the joint. After repeated attacks of this kind, the joint tends to become stiff, cracks and crepitates on movement. These conditions were specially marked in the knee and shoulder joints. The authors found that the dark shadow of the effused blood, obtained by the use of the X-rays, was likely to be useful, especially in the case of the knee-joint, in which the lesions of hæmophilia were liable to be mistaken for those of tubercle, and might be subjected to operation with disastrous consequences.—*Gaz. hebd. de méd.*, Paris, 16th November 1898.

POSTERIOR THORACOTOMY FOR FOREIGN BODY IN THE RIGHT BRONCHUS.

UNTIL recently it was supposed that the extraction of a foreign body from a bronchus was only possible by means of a low tracheotomy. In 1895, Joseph D. Bryant recommended the adoption of the operation which Nasilaff had planned for reaching the œsophagus, namely, resection of the ribs posteriorly, so as to obtain access to the posterior mediastinum. Dr. Farquhar Curtis appears to have been the first to put Bryant's suggestion into practice, for a foreign body in a bronchus. The patient was a boy, æt. 11, who, three days before admission into hospital, had drawn into his trachea a foreign body, consisting of a berry of the mountain ash transfixed by an ordinary pin. There being commencing consolidation at the base of the right lung, it was assumed that the right bronchus was occluded. On the fourth day from the accident a low tracheotomy was performed and curved forceps introduced, by which the body was grasped in the right bronchus, 2 in. beyond the bifurcation, but all attempts at extraction were in vain. The point of the pin was evidently directed upwards, and was caught in the wall of the bronchus.

The following day a quadrangular flap was raised from the posterior surface of the chest with its base outward at the scapula, and its free edge near the spines of the fourth, fifth, and sixth dorsal vertebræ. The ribs were then cleaned, and portions of the fourth, fifth, and sixth ribs, about 3 in. in length, were then resected subperiosteally, from the tuberosities outward. The periosteum and intercostal muscles were divided without injuring the pleura; the latter membrane was then detached from the posterior mediastinum and chest wall, so as to give access to the root of the lung. The bronchus was easily reached, but, owing to the embarassing movements of the lung and the failure of the pulse, the operation was suspended and the wound was packed with gauze.

The following day the packing was removed, and the bronchus was successfully opened on its posterior wall without hæmorrhage. It was found impossible to recognise the foreign body, which had become partly macerated, by means of forceps introduced through the opening. Finally, it was detected through the lung by pressing on the latter with the fingers, so it was cut down upon directly through the lung tissues with the thermo-cautery knife.

The foreign body could be felt in the wound, but it still eluded the grasp of the forceps, and the condition of the patient forbidding further delay, a drainage-tube was introduced to the bottom of the opening made in the lung, and the entire wound packed with gauze. Signs of consolidation, which had been present in the lower lobe of the right lung from the first, extended, and the patient died of pneumonia forty-eight hours after the last operation. A post mortem examination showed an absence of pneumothorax and of pleural effusion. The foreign body lay in one of the secondary bronchi, close to the end of the drainage tube, the pin having entirely penetrated the wall of the bronchus.

If confronted with a similar case, Dr. Curtis would only make half a dozen efforts to reach the foreign body through a tracheotomy wound, and then turn the patient over at once for the posterior operation, and would aim at reaching the bronchus at one sitting. Further, rather than again make prolonged efforts to reach the body through an opening in the bronchus, he would prefer to cut down directly on the body through the lung tissue, having previously stitched the parietal and visceral pleuræ together.—*Ann. Surg.*, Phila., November 1898.

THE ANTRUM OF HIGHMORE.

A FEW cases of suppuration in the maxillary antrum occurring shortly after birth have been recorded. The most recent case is reported by Douglas (*Brit. Med. Journ.*, London, 5th February 1898). The patient was a child 3 weeks old, in whom the following symptoms appeared a fortnight after birth:—The right cheek became swollen, the hard palate of the same side bulging into the mouth, the right eyeball protruded, and the conjunctiva was congested. Pressure upon the cheek caused a flow of pus from the right nostril. The antrum was opened above the alveolus, and pus escaped; the cavity was syringed through this opening, and a cure resulted. A somewhat similar case in a child 8 weeks old was reported by D'Arcy Power in the same journal on 25th September 1897. Here the abscess had burst externally, forming a sinus below the right lower eyelid, while a small quantity of pus exuded from the alveolar border of the right upper jaw. The sinus was enlarged, and a drainage tube inserted along it passed through the antrum into the mouth; death occurred in ten days. Injury to the face at birth by the application of forceps, or by pressure upon the pubic arch during parturition, would appear to account for the origin of abscesses in this region. But another case of a similar nature, occurring in an infant of 3 weeks, has been reported by Rudaux (*Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, September 1895. Apart from the age of the patient, additional interest lay in the fact that the cause of the abscess was assigned to the premature eruption of a tooth through the left canine fossa. An abscess burst below the lower eyelid. Some small sequestra and a loose tooth, which appeared to be a molar, were found in the antrum. After a radical operation had been performed, complete cure resulted.

A NEW SERIES OF SIX HUNDRED OPERATIONS FOR GOITRE.

IN the beginning of 1895, Kocher published his first thousand operations for goitre; since then he and his assistants have completed another series of 600.

It is pointed out that, in spite of the introduction of the thyroid treatment, there is no diminution in the number of goitres requiring operation. Kocher does not think the results of thyroid administration are any better than those obtained by iodine. The statistics of his outpatient department show that 90 per cent. of goitrous patients are so much improved by medical treatment, that no operation is called for; the latter is only indicated in some 10 per cent. of the total cases.

Interference with respiration is the chief indication for surgical interference. Kocher operates whenever there is evidence of stenosis of the trachea. It is his experience that patients deceive themselves on this point. They may deny it, or explain it away, when it is quite apparent to the observer. The most certain method of recognising it is to exert pressure on the trachea through the goitre. If such pressure is exercised, when there exists antero-posterior or lateral flattening of the trachea, the breathing becomes at once embarrassed, with more or less stridor. These patients suffer also from increased difficulty in breathing, on the occurrence of catarrh; they suffer from cough caused by the retention of mucous below the site of the stenosis, and from attacks like asthma; when left to themselves, they may suddenly suffocate during the night from some alteration in the attitude of the head or neck. In all such cases it is not justifiable to prolong the medical treatment for longer than three or at the most four weeks. When there is even the slightest element of Graves's disease, the internal treatment by iodine or thyroid is productive of positive harm.

Kocher's results conclusively show that the operation for uncomplicated goitre is one quite free from danger, if a general anæsthetic is dispensed with.

For more than two years the pain of the skin incision has been obviated by an injection of cocaine (1 per cent.); the further steps of the operation are borne well enough without any anæsthetic. The advantages of this method are well known—the breathing is better, there is less distension of the veins, the patient can be asked to phonate when the dissection approaches the recurrent laryngeal nerve. The risk of a general anæsthetic in tracheal stenosis and in exophthalmic goitre is very considerable, and has been the cause of death.

The actual steps of the operation are those described in the author's text-book. Kocher directs special attention to a condition which he calls thyreoptosis; this consists in the descent of one or other or both of the lateral lobes of the enlarged gland, so that they compress the trachea as it passes through the upper opening of the thorax. This may be associated with an abnormally low situation of the larynx, so that the inferior poles of the lateral lobes actually lie within the upper opening of the thorax (struma profunda, or intrathoracica), or with an abnormally movable goitre, which is sucked into the thorax in deep inspiration. Clinically the condition gives rise to dyspnœa, and attacks like asthma, which may terminate fatally. The diagnosis is often at fault in these cases. Kocher gives some useful guides, which, however, are beyond the scope of this abstract.

Total excision of the thyroid has been given up, except in malignant goitre, and in 1500 operations since 1883, symptoms of cachexia thyreopriva have only been observed in four cases; in two of these, tetany supervened, and proved fatal in spite of the administration of thyroid extract; the remaining two developed myxœdema. The 600 cases here reported include—

18 cases of malignant disease of thyroid, with six deaths (33·3 per cent.).
11 ,, strumitis, with two deaths, of which one was due to tetany.
15 ,, exophthalmic goitre, with two deaths (13·3 per cent.).
556 ,, colloid goitre, with one death (suddenly, from chloroform syncope), giving a mortality of 0·1 per cent.

The relatively high mortality in malignant goitre is due not so much to the operation on the goitre itself, but to the necessity of removing at the same time portions of the trachea, œsophagus, jugular vein, carotid artery, important nerves, and very frequently also glands. These complications are all to be avoided by early diagnosis and operation.—*Cor.-Bl. f. schweiz. Aerzte*, Basel, 15th September 1898.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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New Operation for Enlarging the Uterine Cervical Canal in Cases of Stenosis.

Dr. Guillaume Livet (*Polytech. Med.*, Feb. 1898) describes this operation. The os is dilated with laminaria tents, and subsequently with Hegar's dilators. The cervix is then drawn down to the vulva, and the cervical canal divided into four pyramidal sections, with their base at the external os. The mucous membrane and some surrounding tissue is then removed, leaving a conical cavity with its base at the external and its summit at the internal os. Into this cavity is placed a modified Championnière tube, with a wider flange at its outer end, and provided with two holes for ligatures to fix it *in situ*, the tube being surrounded with iodoform gauze. This tube is removed on the tenth day, when the wound is healed, the mucous membrane reproduced, and the cervical canal remains patent, without any tendency to contraction.

The Curette in Abortion.

Under this title Dührssen (*Berl. Med. Soc.*, May 1898) describes a case in which he eventually extirpated the uterus, per vaginam. The patient had been curetted on two occasions to remove the remains of an incomplete abortion. At the second operation, performed by Dührssen himself, on failing to remove all the placental tissue with the curette, he plugged the uterine cavity, and, after forty-eight hours, introduced the finger, and removed the remaining portions, but at the same time discovered a deficiency in the uterine wall. Severe hæmorrhage followed this operation, and led him to extirpate the uterus. On microscopical examination, the material removed by the curette proved the presence of muscular tissue. Dührssen concludes that the placenta in this case must have been abnormally adherent, and the uterine wall abnormally soft, and that the finger is a better instrument than the curette in imperfect abortion, as is proved by this case.

Castration for Rudimentary Uterus, with Absence of Vagina and Menstrual Molimina.

Dr. Hiram N. Vineberg (*Am. Journ. Obst.*, N. Y., Oct. 1898) describes the case of a domestic servant, æt. 20, who had never menstruated, but who, at the age of 15, was attacked with pain in the lower part of the abdomen, which lasted for three or four days, and recurred every four weeks. For some months previous to operation the attacks had been more severe, and returning every two or three weeks. The patient was well nourished, the breasts well developed, and the axillæ and pubes well covered with hair. The labia majora and minora were normal, as also the clitoris. On separating the labia, an irregular fringe was seen surrounding an oval opening, which was the urethral orifice. The distance between the meatus and the anus was 1 cm., and this space was covered with tender reddish skin, with a shallow depression, resembling the fourchette. There was no vagina. On rectal bimanual examination, an indistinct resistance was felt, running transversely in the normal position of the uterus, and on either side a small body was felt in the lateral pelvic wall, resembling the ovaries. No uterus was detected.

On July 21 laparotomy was performed, and the two bodies thought to be the ovaries were removed, but were found to be probably rudimentary cornua. A search was therefore made for the ovaries, when the left one was found hidden behind intestine, midway between the true and false brims, and had a small undeveloped tube attached to it, which had no connection with the left cornu. After prolonged search the right ovary was found near the vertebral column, about the level of the umbilicus, and showed a ruptured Graafian follicle on its surface, but had no tube attached. The patient made a good recovery, and one year after operation was free from symptoms. On microscopical examination, the bodies removed were proved respectively to be rudimentary solid uterine horns and ovaries. The author points out that the large calibre of the urethra, which admitted the index finger, does not result from its being used for sexual purposes, but is due to the imperfect constriction of that part of the allantois which forms the urethra. The author goes on to say that in some similar cases, where, on operation, no ovaries were found, this may have been due to the fact that, owing to their abnormal position, they had not been discovered.

DISADVANTAGES OF VAGINAL DRAINAGE FOR PELVIC ABSCESS.

DR. CHARLES P. NOBLE (*Am. Gynæc. and Obst. Journ.*, N. Y., March 1898) considers that the treatment of pelvic abscess by vaginal drainage is applicable in only a limited number of cases, and that perhaps 90 per cent. of such cases are better treated by abdominal section, with the removal of the abscess sac. He bases his statements on the result of six cases, in which he operated for supposed pelvic suppuration by the vaginal route. In one patient, who had previously been subjected to six abdominal sections, and who presented symptoms of pelvic suppuration, vaginal incision was practised, and no pus found; on subsequent abdominal section, a small inflammatory cyst was discovered in the broad ligament. In the second case, where the patient had previously been operated on by the abdominal route, and had septic symptoms, with a continuous temperature, vaginal section was performed, and a large inflammatory mass found, but no pus. This resulted in the absorption of the exudation. In two other cases, where septic symptoms presented themselves after childbirth, operation by the vaginal route ended in the discovery of masses of inflammatory exudation, but in neither was pus found. In all these cases the diagnosis of pelvic abscess was negatived by the vaginal operation. In the fifth case the patient suffered from a purulent discharge from the vagina of nine months' duration, and had an ischio-rectal abscess, which had been present for five months. The vagina was incised, and an abscess sac was found in the pouch of Douglas; this was washed out and packed, and the fistulous opening freely scraped. The purulent discharge soon, however, returned, and, on abdominal section, a small firmly adherent ovarian tumour was removed from Douglas' pouch.

In the last case the patient had been invalided for seven years by recurrent attacks of pelvic peritonitis. As pelvic suppuration was suspected, the patient, who was in a feeble condition, was operated on per vaginam, when a quantity of clear fluid, as from an ovarian cyst, was evacuated. On subsequent laparotomy, a multilocular ovarian tumour, partly purulent, and a double pyosalpinx were found and removed.

Dr. Noble concludes that the vaginal operation frequently fails from faulty diagnosis, and also that, should the diagnosis of pelvic abscess be correct, it may prove impossible to remove the abscess by that method.

CASE OF RETAINED OVUM.

H. C. NEER (*Am. Journ. Obst.*, N. Y., Sept. 1898) was called on 21st Dec. 1897, to see a woman, æt. 41, who was five months pregnant, and was suffering from hæmorrhage. This subsided under treatment, and five weeks later, on examination, it was found that the uterus had not increased in size. On 7th March 1898 she again was attacked with hæmorrhage, for which she was plugged. On the removal of the plug, a shrivelled, macerated fœtus, 5 in. in length, was found in the vagina; during the next three days, several masses, of a leathery consistence, were expelled. On the fifth day, as a mass was felt in the uterus, curettage was performed, and the placenta removed, which was converted into a dense white glistening mass, resembling adipocere. Recovery was complete, and menstruation established a month later. Dr. Neer points out the interesting points of the case—(1) The probable complete separation of the placenta at the first attack, without labour pains or serious hæmorrhage. (2) The cessation of the hæmorrhage without any discharge remaining, and the retention of the blood clots, with expulsion or absorption of the serum until they resembled leather. (3) The good health of the patient.

The author, in the literature to which he has had access, has not read of another case where blood clots as well as the ovum were retained.

THERAPEUTICS.

UNDER THE CHARGE OF

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ACTION OF ARSENIC ON THE BONE-MARROW AND BLOOD.

ARSENIC has a marked effect in increasing the number of red blood corpuscles in some cases of pernicious anæmia, of leukæmia, and of lymphadenoma, although not infrequently in these diseases the blood condition is not perceptibly improved by it. In malarial anæmia, also, its hæmatinic value is undoubted, although its mode of action is obscure, as it is still an open question whether it stimulates the formation of red corpuscles, or lessens their rate of disintegration by killing the specific organisms. In chlorotic anæmia, arsenic given alone does not improve the blood condition, but its combination with iron is supposed to greatly hasten the action of the latter. In health it is incapable of increasing the number of corpuscles or the amount of hæmoglobin. Stockman and Greig (*Journ. Physiol.*, Cambridge and London, vol. xxxiii. p. 376) have published the results of some experiments on dogs and rabbits which seem to partly at least explain the hæmatinic action of arsenious acid. Young and adult animals received small doses of arsenic daily for periods of 43 to 145 days. During this time the corpuscles and hæmoglobin were frequently estimated and compared with those of control animals. On killing the animals, the only changes observed were in the bone-marrow of those which had received arsenic. The corpuscles and hæmoglobin remained exactly as in the control animals; there was neither an increase nor a decrease observed. In the marrow the difference between the normal and arsenic rabbits was very striking. The red marrow in the latter was greatly increased in amount and of a deeper colour; there was a great increase in the number of capillary blood vessels in it; the fat cells had to a large extent disappeared or were atrophied, and there was a much greater number of marrow cells to take their place. In short, in the marrow there was greatly increased vascularity, great diminution of fat, and a great increase of marrow cells—all this indicating local stimulation and increased function. In spite of this stimulation of the bone-marrow, there was no increase in the number of red or white corpuscles or of hæmoglobin. This, however, is capable of being interpreted in two widely differing ways—(1) The arsenic may stimulate the formation of red corpuscles, but there may occur simultaneously a corresponding increase in their breaking down so as to keep the number present in the blood at a constant quantity. There was found no evidence of increased breaking down, so far as any increase of iron pigment in the spleen or liver went to show it. (2) In *health* there may be no increased formation of red corpuscles under the action of arsenic. As iron, good food, and tonics fail to raise the number beyond the physiological maximum in health, and yet act as hæmatinics in disease, it is not astonishing that arsenic should behave similarly. In pernicious anæmia, leukæmia, etc., the experiments seem to show that the hæmatinic effect is due to stimulation of blood formation in the bone-marrow. If this be so, although arsenic may increase the number of red corpuscles and apparently improve the patient's condition, it does not strike deeper and affect the real cause of the disease, and it must therefore be regarded as purely symptomatic treatment in these so-called blood diseases.

TREATMENT OF HEMICRANIA.

LAQUER, who himself suffers from hemicrania, treats his attacks successfully as follows:—There is usually gastric disturbance, which is combated by a draught of any alkaline mineral water, followed by a cup of tea, abstinence from food, and a powder containing lactophenin 6–10 grs. and caffeine citrate 3–4 grs. every two hours until three have been taken. He then walks in an open space or on a level road keeping his eyes fixed on the distance, and drawing deep breaths from time to time. Soon hunger and polyuria follow, the former being satisfied by simple and easily digested food. Alcohol is to be avoided. A strong cup of coffee usually banishes the last traces of the attack. Any mental or bodily work is forbidden.—*Centralbl. f. innere Med.*, Leipzig, No. 48, 1898.

THE ACTIVE CONSTITUENTS OF DIGITALIS LEAVES.

IT is well known that so long ago as 1874, Schmiedeberg described the active principles of digitalis purpurea as four in number, namely, digitonin, digitalein, digitalin, and digitoxin—the last three only having the characteristic action on the heart. It has been generally overlooked, however, that the distinguished Strassburg professor investigated the constituents of the digitalis seeds, and his results have been taken to hold good for the leaves also, without any controlling investigation having been made. It is the leaves which are used for therapeutical purposes in all countries, but the text-books on materia medica describe their chemical constituents in exact accordance with Schmiedeberg's results derived from an examination of the seeds, and make no reference to there being any uncertainty in the matter. The behaviour of the chemical constituents as regards solubility in water and alcohol differs greatly, and this has been made the foundation of theories regarding a difference in action between the tincture and infusion of the leaves. Digitalin, digitalein, and digitoxin differ also very greatly in activity, the last being much more powerful than the other two. Clinically, also, differences have been drawn between the action of digitoxin and digitalin as compared with the powdered leaves or preparations made from them. It is a very doubtful matter if such differences really exist, as the varying results may quite well be due to the cases on which the observations were made. Cloetta (*Arch. f. exper. Path. u. Pharmakol.*, Leipzig, 1898, Bd. xli. S. 421) has made an examination of the leaves with the object of checking Schmiedeberg's results derived from the seeds. The chemical methods available are very complex and tedious and not calculated to give exact quantitative results, but he finds that digitonin, digitalin, and digitoxin are all present in the leaves. The presence of digitalein is more doubtful, but there may quite well be a minute amount. It is also certain that these bodies constitute all the active principles. The chief point brought out is that the relative quantities in the seeds and leaves differ very considerably—in the former digitalin is the chief body, but in the latter it is digitoxin. As digitoxin is practically insoluble in water, it has been stated freely that the infusion does not contain it in appreciable quantity. This, however, is a mistake, as digitonin is very soluble in water, and its solution dissolves up the digitoxin freely; and Cloetta has used a solution of pure digitoxin in pure digitonin and water successfully for hypodermic injection. He is further of opinion that there is no difference of action between a tincture and infusion made from the leaves.

MEDICAL JURISPRUDENCE.

UNDER THE CHARGE OF

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ENLARGEMENT OF THE THYMUS GLAND AS A CAUSE OF SUDDEN DEATH.

IN the *Vrtljschr. f. gerichtl. Med.*, Berlin, October 1898, Professor Seydel, in referring to this subject, which has been so much discussed during recent years, and to its great importance to the medical jurist in cases of sudden death amongst children, relates a case which occurred in his own practice.

The circumstances of the case presented the usual features. A child, æt. 6 weeks, slept in an infant's crib beside its nurse's bed, and was in good health. At 2 A.M. it was given nourishment by the nurse and put back in its cot. At 6 A.M. she placed the infant upon its other side, when it appeared to be quite well. At a quarter past 8 o'clock the nurse and mother found the child dead; the face was quite free from the bed-clothes, and the child lay upon its side and had not vomited. From the post-mortem, Seydel came to the conclusion that death had resulted from asphyxia, though how this was produced the examination did not afford definite evidence. All the typical appearances associated with death from asphyxia were not present, *e.g.* the petechial ecchymoses on the surface of the lungs and the congestion of the vessels of the brain and its membranes. The thymus gland was much enlarged, being 6 cms. long by 5 cms. broad, and 2 cms. thick, and of a firm consistence. The lungs presented an appearance which is seldom absent in cases of sudden death where the thymus is enlarged, namely, the presence in the bronchioles of whitish-grey mucus. This appearance is so common in such cases, that it led Paltauf to suggest that death was not due to enlargement of the thymus, but to acute bronchial catarrh.

Seydel was able to exclude all ordinary causes of accidental suffocation, and from this case and others agrees with the conclusion of

Pott, that "a hyperplastic thymus gland influences the respiration and circulation gradually, and also, it may be, suddenly, and may thus be the cause, indirectly at least, of sudden death in children enjoying apparently good health." Probably the enlarged gland produces the fatal effect, by pressure on the nerves of the heart, either the vagus or other branches innervating the heart, so that death is due to sudden syncope, and not to asphyxia from compression of the air passages.

In the *Ann. d'hyg.*, Paris, December 1898, Dr. Perrin de la Touche has a paper upon hypertrophy of the thymus gland as an obstacle to the establishment of respiration in the newly born. In a case which he recently examined, where the mother was suspected of infanticide, the thymus was found considerably enlarged, while neither the lungs nor the stomach contained any evidence of respiration having occurred. The mother admitted that the child had moved after birth, but stated that it had not cried, and there was no evidence that she had suffocated it. De la Touche, while not holding that it could be positively asserted that the enlargement of the thymus had prevented the establishment of respiration, yet thinks the possibility must be admitted, and that the condition of hypertrophy of the thymus gland, in preventing the establishment of respiration, is a subject of considerable medico-legal importance, upon which further observations are desirable.

THE CORD AND THE PERIOD OF SURVIVAL AFTER BIRTH.

IN the *Beitr. z. path. Anat. u. z. allg. Path.*, Jena, Bd. xxiv. Heft 2, Professor Kockel discusses the microscopic appearances connected with the separation of the umbilical cord, and the value they possess in determining the period of survival after birth.

Many attempts have been made by investigators to discover a means of accurately determining the length of time a newly-born child has survived its birth, from the conditions presented by the body after death, but so far without much success.

The condition of the lungs, of the intestinal tract, the changes in the circulatory system, etc., may all afford evidence of a child having survived its birth, but fail to give a satisfactory answer to the question, whether a child died after a few inspirations, or whether it lived for a short time—one or two hours—after its birth.

The author, with a view to satisfactorily determining this question, has carried out a series of observations into the microscopic changes which take place in the cord after birth and during the process of separation. From a large number of cords of children who died at varying periods after birth, and from an examination of the cords of animals, Kockel states that—(1) In a child that has died within three-quarters of an hour after birth a slight circular leucocyte infiltration of the surface of the cord immediately adjacent to the margin of the skin, and about 1 mm. in breadth, is already present. (2) An inflammation occurs in the cord next to the permanent tissue of the umbilicus, which subsequently results in the separation of the remains of the cord. This inflammation, in the form of an infiltration of pus cells, can be observed in one, or within a few hours, after birth; at first it is only superficial, but afterwards deeper, and remains always confined to the basal portion of the cord. (3) In comparing the microscopic appearances of the cord in the case of children that have lived with those in dead-born children, the chief difference is found to lie in the presence of the leucocyte infiltration in the former, while it is absent in the latter. The presence of this infiltration is so constant in the case of children that have lived, even for an hour or two, that when it exists one may conclude that extra-uterine life has existed. (4) The microscopic appearances in connection with the separation of the cord entitle us to decide, even within the two first days after birth, with some degree of certainty how long a child has lived.

The author, while laying down these propositions as a result of his investigations, is of opinion that they should, as far as possible, be compared with the test of the presence of air in the stomach and intestines; and, under any circumstances, he holds that the microscopic appearances at the line of demarcation of the cord in the newly born should be carefully observed.

IODINE A NEW REAGENT FOR SPERMATIC FLUID.

DR. WYATT JOHNSTON of Montreal, in the *Journ. de méd. de Paris*, 25th September 1898, draws attention to the method of detecting spermatic fluid by means of a solution containing iodine.

He points out the difficulties often attending the confirmation of the presence of spermatic fluid in stains, especially when these are not fresh, and indicates the importance of possessing a simple and rapid preliminary test, applicable by everyone, by which, in the course of a few minutes, it may be seen whether a microscopic examination will hold out a prospect of success.

Florence of Lyons has proposed the following reagent, which appears to fulfil the above conditions :—

Iodide of potassium	1·65 grm.
Iodine	2·54 grm.
Distilled water	30 c.c.

A weaker solution may be used, but it should always contain an excess of iodine. A cold solution must be used, and it should be preserved in hermetically sealed vessels. On adding this solution to a drop of seminal fluid, small brownish red crystals are immediately formed, which are rhomboidal, and resemble in form, size, and colour, crystals of hæmatin. They are slightly soluble in cold, and very soluble in warm water. On cooling the latter, the crystals reappear. On exposure to the air the crystals slowly disappear, but are re-formed on adding a drop of the reagent. In order to procure typical crystals in considerable quantity, the stain under examination must be well diluted. Dried semen gives as good results as fresh. Dr. Johnston states that he has demonstrated the test upon a stain a year old, and that the smallest fibre from a piece of the stained linen suffices to show under the microscope an abundance of crystals. He believes that the reaction constitutes a distinct advance, and is an excellent preliminary test, while in certain cases it may even afford an absolute proof of the presence of seminal fluid.

RUPTURE OF THE INTESTINE IN THE NEWLY BORN.

DR. CIECHANOWSKI of Cracow (*Vrtljschr. f. gerichtl. Med.*, Berlin, October 1898) describes a case of rupture of the transverse colon in a newly-born child, and discusses the pathological and medico-legal questions which such cases give rise to.

The child in question was the third borne by a healthy mother. It was an ordinary head presentation, and, when the waters came away, the child was born after a single strong pain. The birth took place in hospital in the presence of midwives. The infant lived for four days, during which period it passed several motions, consisting of meconium. It died with symptoms of peritonitis. Post mortem there was found a rupture of the transverse colon on the left side. The colon above the rupture was distended with meconium, while beyond the rupture it was empty, and took a sharp bend upwards, forming a V with the descending colon. With the exception of the above-mentioned rupture, there was nowhere any sign, either macroscopic or microscopic, of any pathological changes in the intestines.

The author refers to similar cases described by Browicz, Paltauf, and Zillner, and points out that, while in adults traumatic ruptures most commonly occur in the lower part of the ileum, in all the published cases of rupture of the intestines in new-born children, or in fœtuses, the rupture has been in the large intestine. He concludes that probably in the latter cases the ruptures are not usually due to pathological changes in the intestine, as has been suggested by Paltauf, but to some sudden force or violence exerted upon the abdomen, the part of the intestine most prone to suffer rupture being the large intestine which is distended with meconium, and the walls of which are thus thinned, and less able to withstand any sudden pressure or force.

The author admits that rupture might result from violence on the part of the mother, either in attempted self-delivery, or in attempted child murder, but regards the possibility of a mother causing a rupture of the large intestine without producing signs of external violence, as highly improbable.

In the author's own case both possibilities were negatived by the evidence of the midwives present at the birth, and he is of opinion that the cause of the rupture was the force exerted by the strong contraction of the uterus acting upon the highly distended large intestine.

DISEASES OF THE EAR, NOSE, AND THROAT.

UNDER THE CHARGE OF
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THE TREATMENT OF HAY FEVER WITH THE EXTRACT OF SUPRARENAL GLAND.

IT cannot be said that up to the present time very much success has attended the employment of the many remedies which have been advocated for the treatment of this distressing and obstinate affection. Any new remedy which is suggested must always be gladly welcomed by those who yearly suffer from attacks of hay fever. With the introduction of the local anæsthetic, orthoform, a hope was raised that it might prove of considerable benefit in allaying the intense irritation of the nasal mucosa. Notwithstanding the success that has been claimed for it by Lichtwitz (*Arch. internat. de Laryngol.*, Paris, 1898, tome xi.) in the treatment of this affection, it has not met with the same favour in the hands of others. By the use of ischæmin, the extract of suprarenal capsule, S. Solis Cohen has in his own case successfully controlled the symptoms (*Phila. Med. Journ.*, 13th August 1898). While experimenting with it he gave up all other remedies, and he found that, while under its influence, relief from his symptoms was obtained. When it was omitted for a longer or shorter period, the symptoms returned. At first a glycerin extract of sheep's suprarenal capsule, freshly prepared, was taken, made more palatable in the form of an elixir, the dose employed being from fifteen drops to one teaspoonful, repeated thrice daily. Later, he found it necessary either to increase the dose or to repeat it more frequently. As a tendency to nausea was produced with this preparation, he substituted for it tabloids, prepared by Burroughs, Wellcome & Co. One tabloid, containing 5 grs. of suprarenal substance, was allowed to dissolve in the mouth every two, three, or four hours, according to the effect produced. On an average he took five tabloids daily, the last one being taken at bedtime, with the result that the night passed without his having any of the usual symptoms. In the light of the results thus obtained by S. Solis Cohen, it will be interesting to ascertain how much success follows its use in the hands of others. In connection with the employment of this drug in hay fever, one may here note its effects when used locally in the upper air passages. H. L. Swain of New Haven has tabulated the result of his experiments with it (*Med. Rec.*, N. Y., May 1898)—(1) Aqueous extract of suprarenal capsule is a powerful local vasomotor constrictor, and a contractor of erectile tissue; it is safe to use in a very considerable amount, without dangerous or deleterious effects locally or as regards the general constitution. (2) These effects can be produced any number of times without entailing any vicious habit, either in the tissue or in the individual. (3) It seems to heighten the effect of any drug used locally after it. (4) In acute congestions it has its widest application and greatest opportunity for good, but in certain chronic conditions of the hay fever type, when redundant tissue seems prone to develop, it can be relied upon.

THE ASCH OPERATION FOR DEVIATION OF THE CARTILAGINOUS SEPTUM OF THE NOSE.

WHILE admitting that the operation devised by Asch of New York for rectifying the condition of deviated septum is in no sense a recent one, we are led to refer to it at this time, and again bring it under notice on account of the success which has followed its employment. Mayer has published (*Med. Rec.*, N. Y., 5th February 1898) the result of two hundred operations for deviated septum, performed by Asch's method. At no previous time have deductions been drawn from so large a number of cases similarly treated, and as some of the operations date back five, six, seven, and eight years, ample opportunity is given us of estimating the permanency of the cure. The degree of deviation of the septum varies considerably, with consequent variation in the amount of nasal obstruction that is thereby produced. Different operations have been suggested and carried out for the correction of this deformity, and in many instances relapse has occurred, owing to the resiliency of the cartilaginous septum and its consequent return to the old deviated position. In the operation carried out by Asch, the septum is fractured in such a way that this tendency to spring back is reduced to a minimum. Of the two hundred cases which form the text of Mayer's paper, 85 per cent. resulted in the complete straightening of the septum, while in the remainder some degree of deflection was retained, but in no case was this sufficient to cause any stenosis or discomfort, so that in all a cure may be said to have followed, as far as symptoms are concerned. Perforations of the septum occurred in about 2 per cent. of the cases, but they were only of the size of pin-heads. It may not be out of place to describe the operation in the author's own words. Special instruments for the purpose have been devised, and these are figured in Mayer's paper:—

"The blunt separator is introduced sideways on the deviated side, in order that any adhesions existing between the septum and the adjacent mucous membrane may be broken up, enabling us at the same time to ascertain the presence of bony obstructions posteriorly. Should these latter exist, or the adhesions be non-yielding, the separator with sharp edges is required; this, however, is rarely necessary. There may be some hæmorrhage after the adhesions are broken up, but it is readily checked by an iced spray, or by pressure of cotton applicators. The open scissors are now introduced parallel to the floor of the nose, the sharp blade in the concavity, the blunt edge over the point of greatest convexity. They are then firmly closed, the blade cutting through the cartilage into the opposite side with a distinct snap. The scissors are then opened and completely withdrawn. They are immediately reintroduced, the direction of the instrument being upward this time, pointing to the frontal bone, the sharp blade again in the concavity and crossing the line of the first incision at as nearly a right angle as possible, and at its centre. In this way the incisions will intersect each other; the scissors are now firmly closed, and the second incision is made, after which the scissors are opened and withdrawn. We now have four segments as a result of this crucial incision. The finger is then introduced in the deviated side, and the segments are forcibly pushed into the concavity, effectually breaking them at their base. The powerful blunt forceps is now introduced, one blade in each nostril, and firmly closed, thus straightening the septum and forcing the broken segments to over-ride each other in the concavity. The compression aids also in controlling hæmorrhage, and the parts are firmly held in apposition. An iced antiseptic solution is sprayed into each nostril, the forceps being withdrawn. The sterilised tubes are then introduced into the nose, a snugly fitting one on the stenosed side, and a smaller one in the opposite nostril, thus causing equable pressure, their presence being usually all-sufficient to control hæmorrhage. The operation is now completed."

After twenty-four hours the tube is removed from what was the nostril with the concavity, and is not again replaced. Twenty-four hours later the tube is removed from the other side, cleaned, and reinserted, unless it prove too large for comfort, when a smaller one is introduced. For the first time the patient is now allowed to leave the recumbent posture. On the third and following days the tube is removed and cleaned, the nose being sprayed with some warm salt water at the same time. The tube is finally withdrawn five weeks after the operation. These tubes are of the greatest service, as they allow of free nasal respiration and act as splints to keep the septum straight. The overlapping fragments become firmly adherent, and this destroys the resiliency of the septal cartilage, so that the deformity is thus removed.

While referring to the subject of deviated septum, it will not be out of place to make a note of the somewhat ingenious method practised by Escat (*Arch. internat. de laryngol.*, Paris, July and August 1898). When the nasal obstruction is relieved by sawing or chiselling off the convex projecting portion, some risk is run of causing a perforation of the septum, from injury of the mucous membrane covering the septum on its concave aspect. In order to prevent this, Escat endeavours to protect the septal mucous membrane on the concave side by injecting sterilised water beneath it. This is done by means of a syringe, with the result that the mucosa is raised from the surface of the cartilage which it covers, and thus escapes injury.

BRONCHOSCOPY.

AT the present time there is a strong tendency to assist the clinician as far as possible in his diagnosis by bringing various organs and parts of the body under the direct observation of the eye. It is not necessary for us to specify what has been done during the last few years in the development of this particular branch. One of the most recent methods of examination suggested to the laryngologist is that of bronchoscopy. The examination of the trachea through a tracheotomy wound was first attempted by Schrötter and carried out by Pieniazek. Killian of Freiburg (*München. med. Wchnschr.*, 1898, No. 27) has gone a step further, and practised examination of the bronchi through a tracheal wound, and to this method he has applied the term "bronchoscopy inferior." For this purpose the bronchi were first anæsthetised with a 10 per cent. solution of cocaine; this was followed by the introduction of a tubular speculum through the tracheotomy wound, illumination being obtained by means of an electric forehead lamp. The size of the tube varied according to the height of the individual. In a patient who measured 168 cms., the tube had a diameter of 9 mm. When the distal end of the speculum had passed for a distance of 14 cms. from the tracheal wound, it reached the bifurcation and passed into the right bronchus. After further careful insertion, Killian found that he was able to explore the smaller divisions of the bronchi—first, the ramus leading into the upper lobe; then that passing to the middle; and, lastly, that entering the lower lobe. External measurement revealed the fact that the end of the speculum had reached the level of the fourth intercostal space, while, owing to the narrowing of the bronchial lumen, further insertion was impossible. He found the bronchi strong, elastic, and somewhat expansile, and, what was an important fact, they were to some extent displaceable, thus admitting of a more ready introduction of the metal tube. This was borne by the patient without pain; there was no hæmorrhage. After withdrawing the speculum for a certain distance, he was able to pass it in the next instance into the left bronchus, and in this way observe its branches passing into the upper and lower lobes of the lung. In a smaller patient, a boy, a tube with a narrower lumen was employed.

In addition to "bronchoscopy inferior," he also practised "direct bronchoscopy," or examination through a tubular speculum passed into the mouth. The larynx was first painted with cocaine, special attention being directed to the epiglottis and posterior laryngeal wall. The trachea and right bronchus were also anæsthetised, by the application of cocaine upon a long straight probe, the larynx being brought under observation by means of autoscopy. The distance from the mouth to the bifurcation of the trachea, in a patient 152 cms. in height, was found to be 27 cms. A tubular speculum 9 mm. in diameter was introduced, thus bringing into view the openings of the two bronchial tubes, while by further insertion for a distance of 5 cms. into the right bronchus its subdivision became apparent. The bifurcation of the left bronchus, on the other hand, was observed 4 cms. from its commencement. No difficulty in respiration was experienced during direct bronchoscopy, the patient being able to breathe easily through the tube. During the passage of the speculum, the head was thrown backwards as far as possible, while the tongue was held well forwards. How far this method of examination will prove to be of any practical value, remains to be seen. Further trial is necessary. While Killian has obtained a satisfactory result in a series of cases, it is questionable how far patients will be able to tolerate the insertion of such an instrument, so as to render its use of any clinical value.

PUBLIC HEALTH.

UNDER THE CHARGE OF

CHARLES HUNTER STEWART, M.B., D.Sc.,
PROFESSOR OF PUBLIC HEALTH, UNIVERSITY OF EDINBURGH.

Practical Disinfection by Formaldehyde.

Flügge (*Ztschr. f. Hyg.*, Leipzig, Bd. xxix. Heft 2) makes an important contribution on this subject from the practical and administrative point of view. He thinks two causes are operating to bring disinfection into disrepute. On the one side, the spoiling of furniture, pictures, bedding, clothing, etc., by the means that are usually employed, when disinfection is *really* aimed at; and, on the other, the conviction that, as a rule, the result attained is by no means commensurate with the inconvenience and loss caused. Take for example bedding or clothing sent to a public steam disinfector. Too often this returns home not only damaged in appearance, but with a most disagreeable smell, this latter being due to the disinfector having been used possibly immediately before for the disinfection of bedding, etc., from some filthy quarter of the city or district. There is no wonder that the proverb passes, "To have been twice disinfected is as good as to be once burnt out." During the last year he had tested the efficiency of the disinfection as carried out by the well-trained disinfecting staff of the city of Breslau. He infected portions of the walls, floor, and furniture of a room, which was then disinfected by the city staff. He performed this experiment twice, and on both occasions found that many of the organisms had been unaffected. He admits that we cannot hope that in practice we shall be able to kill every individual disease organism, but we ought to seek for some method which will approximate to this, with as little damage to property as possible.

The fact that gaseous disinfection, if thorough, does away with the necessity for separate treatment of the contents of the room, has always recommended it, but unfortunately, in the past, disinfection by chlorine, sulphur, etc., has either been inefficient, or, when efficient, has damaged the pictures, furniture, etc. He reviews most of the methods suggested for the use of formaldehyde, which, as is well known, has no deleterious action on such articles. As this substance has little or no penetrating power, it cannot be used in such cases as cholera, typhoid fever, and infective diarrhœas, where the infective substance often penetrates the bedding or body linen, or forms a somewhat thick layer upon them. For diphtheria, scarlet fever, measles, tubercle, and influenza it is to be recommended.

The conditions of efficient disinfection by this substance are—(1) Sufficient quantity, and this depends on the length of time the disinfectant is allowed to act. 250 grms. of formaldehyde (about 660 c.c. of commercial formalin) are required for 3500 cubic ft. of room capacity, if the disinfection lasts seven hours. During the disinfection the room must be as air-tight as possible. This is best secured by firmly packing all chinks in windows and doors with cheap cotton-wool, previously soaked in sublimate solution, and closing openings of chimney, etc. With these precautions a sufficient percentage of formalin in the room is assured, without the need of control test objects, as have been recommended. (2) The gas developed in such a way that no inactive paraform is produced either in the producing vessel or in the room. He finds that a large amount of water mixed with the formalin is as efficient in preventing polymerisation as chloride of calcium (Trillat) or glycerin (Schlossmann). Moreover, the steam which accompanies the formaldehyde as it escapes into the room prevents polymerisation there. He recommends that the 660 c.c. of formalin be made up to 3 litres with water and put into a form of kettle, heated by a large spirit lamp, into which the amount of spirit is put that is necessary to drive off all the formaldehyde and as much steam as is sufficient to saturate the air of the room at about 30° C. The spout of the kettle is connected by tubing with the key-hole through which the gas enters the room. (3) To ensure that clothing, beds, etc., are freely exposed to the action. The furniture should be removed from the walls, and all closed wardrobes, etc., opened. The disinfector should bring an iron screen, on which all bed and body clothing, carpets, etc., are to be hung, or clothes lines may be used for this purpose. All grossly soiled clothing is to be steeped in sublimate solution. (4) To remove the gas from the room when disinfection is over. This cannot be done by the opening of doors and windows, as the irritating vapour which would escape into the other rooms would make them uninhabitable. It is best done by heating a commercial 25 per cent. solution of ammonia in an ordinary kettle, having rubber tubing connecting its spout with the key-hole through which the formaldehyde entered. For the size of room under consideration, 800 c.c. of this ammonia solution is enough to entirely convert the formaldehyde in the room into the odourless hexamethylenetetramine. It takes twenty minutes to drive off the ammonia and thirty minutes should be allowed for its action. The room is then opened, furniture, clothing, and bedding replaced, and the clothing in sublimate solution washed in water. The time taken for such a disinfection by trained workmen is not long—twelve hours after they begin the room should be ready for occupation. For this size of room the cost of material is 4s. The apparatus for developing the formaldehyde costs 20s. Since in the cases in which this method is applicable there is no need of steam disinfection, the cost is not greater than by the present methods. In such diseases as puerperal fever, erysipelas, smallpox, and plague, where steam disinfection of clothing would also be required, the cost is proportionately greater. The method as detailed has been used with excellent results in Breslau, and has been approved by the citizens as not harming their property. The most important point is that here we have a method more worthy of confidence than those in use hitherto.

DISINFECTING POWER OF COMMON SOAP.

THE results obtained by different investigators have varied very much. Koch, *e.g.*, found that a strength of 1 per thousand killed *B. anthrax*, while Behring found that a strength of 14 per thousand was necessary. With *B. cholera* and *B. typhoid* the results also were conflicting. Di Mattei found that a 25 per thousand solution killed *B. cholera* in sixty minutes, while Nijland found that a 3 per thousand killed it in fifteen minutes. Professor Serafini (*Arch. f. Hyg.*, München u. Leipzig, Bd. xxxiii. Heft 4) has worked at this subject with the view of explaining the cause of the difference in results obtained, and of determining the value of soap as a disinfectant in practice. He used nine different kinds of soap met with in commerce. He restricted himself to the action of solutions of these on *B. cholera* for different times at different temperatures. In seven of these the bacillus was killed in ten minutes or less by a 10 per thousand solution, and by a 50 per thousand solution it was killed in all in from one to fifteen minutes. He does not agree with Behring that the disinfecting power is due to the free alkali which is always formed when even neutral soaps are dissolved in water. A 50 per thousand solution of soap gave as a maximum 1·9 per thousand of free alkali, while, according to Kitasato, it takes four to five hours for a 2·37 per thousand solution of caustic alkali to kill *B. cholera*. The amount of real soap, *i.e.* the soluble salt composed of fatty acid and base, in a given soap, measures its disinfecting power. Hard water of any kind, if used as a solvent, diminishes the disinfecting power by forming insoluble soaps. The raising of the temperature to 37°–40° C. increases the disinfecting power chiefly by keeping the soap in solution. The great drawback lies in the difficulty of getting such a strength of soap solution to permeate fabrics, especially when wet, and the insolubility of infected marks, *e.g.*, fæcal stains. Disinfection by soap should only be resorted to when other means are not available. Hard soap, *e.g.* Castile soap, which contains little water, should be used, and the temperature should be 30°–40° C. for several hours. Soft soap should never be used, as it contains too much water.

THE PRODUCTION OF PATHOGENIC PROPERTIES IN SAPROPHYTIC ORGANISMS.

THE seeming spontaneous origin of some epidemics, as well as their decline after a time, is one of the most puzzling phenomena met with in epidemiology. That many pathogenic organisms grown outside the body lose their virulence, and Pasteur's classical experiments showing that the *B. anthrax* can be entirely robbed of its pathogenic properties and then restored to its former virulence, are facts well known. M. Vincent (*Ann. de l'Inst. Pasteur*, Paris, 1898, No. 12) contributes the results of his experiments, which show that some of the commonest non-pathogenic saprophytic organisms, if grown as parasites, become endowed with marked pathogenic properties. *B. megaterium* and *B. mesentericus vulgatus* (potato bacillus), both very frequently found in the soil, strongly aërobic, and quite non-pathogenic, were studied. Broth cultures contained in collodion sacs were cultivated in the peritoneal cavity of guinea-pigs. After the sixth passage through guinea-pigs in this way, the *B. megaterium* attained great virulence, two drops of the culture killing mice in ten to twenty hours, and a ½ c.c. killing a guinea-pig in twenty-four hours. When cultivated *in vitro* aërobically, it slowly lost its virulence, which entirely disappeared in three months. Three successive cultures in guinea-pigs restored the virulence. The *B. mesentericus* gave results quite similar. A virulent culture containing spore-bearing organisms, if heated to 100° C. and then passed through a guinea-pig, was still virulent, showing that the virulence was imparted to the spores. The organs and the blood contained the bacillus showing no degeneration. Fatal effects were also produced by injecting a filtered virulent culture. The bacillus therefore kills both by toxins already existing in the culture and by a true infection. The serum of an animal which had got at intervals non-fatal injections of the virulent culture gave the phenomenon of clumping.

DIPHTHERIA BACILLI AND DIPHTHERIA IN SCARLET FEVER CASES.

DURING a two years' period extending between 1895 and 1897, Soerensen (*Ztschr. f. Hyg.*, Leipzig, Bd. xxix. Heft 2) made regular bacteriological examination for diphtheria bacilli in the throats of all patients in the scarlet fever pavilions of the Blegdom Hospital, Copenhagen. There were 1547 cases admitted, and these were examined on admission, from time to time during their stay and on discharge from hospital. In thirty-eight cases, or about 2·5 per cent., diphtheria bacilli were found on admission, and among the remainder there were 208 in which diphtheria bacilli were found without producing diphtheria, and thirty-two cases of diphtheria which were, however, mild. All these were therefore infected in the hospital. Cases were found in most of the pavilions. The wards themselves did not seem to be the source of infection, as frequently in those which had contained scarlet fever cases complicated with diphtheria, when cleansed and used for other cases, no diphtheria appeared, or only after months. Nor, as a rule, could the infection be due to patients having diphtheria bacilli on admission, as the bacillus often disappeared on the day after admission; and further, in wards to which none such had been admitted, the patients were found with the bacillus. Although he has no proof, he thinks it wise, provisionally, to refer the cases to infection by food and by the atmosphere. The time that the bacillus remained in the throat varied much, in some a very short time, and in others as long as five months. Very striking is the small number of cases of diphtheria in relation to the number of patients with the bacillus. In the city, at the same time, the number and severity of the cases of diphtheria was not great. The bacillus was therefore probably not very virulent, but in four cases in which it had been most persistent, experiment proved its virulence.

REPORTS OF THE MEDICAL SOCIETIES.

EDINBURGH MEDICO-CHIRURGICAL SOCIETY.—*21st December* 1898.—Sir JOHN BATTY TUKE in the chair.—PATIENTS EXHIBITED.—Professor CHIENE showed—(1) Coxa vara, probably traumatic in origin, in a lad æt. 16; (2) double partial dislocation of both hip-joints, with ununited fracture of both clavicles.—Dr. AFFLECK showed—(1) Two cases of Friedreich's ataxia, the patients being brother and sister; (2) Addison's disease, which had improved under the suprarenal treatment.—Mr. CAIRD showed—(1) A patient after removal of rectal carcinoma, with complete rectal control; (2) a patient after removal of appendix vermiformis and of renal calculus.—Dr. MELVILLE DUNLOP showed—(1) Double athetosis; (2) pseudo-hypertrophic paralysis, with some choreic-like movements.—Dr. JAMES showed—(1) A case of scleroderma in Raynaud's disease and thyroid atrophy; (2) a case of angio-neurotic œdema of the tongue.—Dr. GRAHAM BROWN (for Dr. G. A. GIBSON) showed—(1) A case of bulbar paralysis; (2) a case of pseudo-bulbar paralysis, in which the lesion was not in the bulb but in the cerebral cortex.—Dr. JOHN THOMPSON showed hemiatrophy of the tongue without apparent cause.—Dr. GEORGE ELDER showed—(1) Congenital hemiatrophy of the whole face, with malformation of the ear; (2) a case of senile chorea.—Dr. ALLAN JAMIESON showed—(1) A papulo-squamous eruption, apparently syphilitic, affecting the palms; (2) a case of psorospermosis follicularis vegetans (Darier's disease); (3) the results of treatment in a case of tylosis; (4) elephantiasis affecting both legs. —Mr. HODSDON showed pleniform neuroma of the upper arm.

SPECIMENS.—Mr. WALLACE showed—(1) A renal calculus; (2) kidneys from same case with calculi *in situ*; (3) fibro-adenoma removed from prostate; (4) portions of prostate removed from three cases by suprapubic cystotomy; (5) four kidneys (three tubercular, one carcinomatous) removed by operation; (6) dermoid cyst from floor of mouth; (7) biliary calculi removed by cholecystotomy; (8) microscopic specimens of tumours of prostate.—Dr. LOGAN TURNER showed a skull with unusual development of frontal sinuses.—Dr. DOWDEN showed—(1) A specimen of plexiform neuroma; (2) microscopic section of the same; (3) specimen of hydatids.—Mr. CATHCART showed—(1) Abdominal aneurysm arising from the posterior wall of the aorta; (2) multiple abscesses in the brain.—Mr. CAIRD showed preparations and drawings from an unsuccessful case of enterectomy and suture for carcinoma.—Dr. WM. ELDER showed—(1) The heart and intestine from a case of dysentery, with ulcerative endocarditis; (2) a brain showing hæmorrhage into the occipital lobe; (3) the brain from a case of word-blindness.—Dr. ALLAN JAMIESON showed a drawing of a case of urticaria pigmentosa.

EXHIBITS.—Mr. CATHCART. — Improvements on microtome. — Mr. CAIRD.—Needle case for sterilising needles and sutures.

18th January 1899.—Dr. P. A. YOUNG in the chair.—Mr. GREIG showed a man in whom there was symmetrical congenital perforation of both parietal bones, which had not before been described during life.—Mr. CAIRD and Mr. GUY showed a case of syphilitic destruction of the nose, palate, and upper lip.—Dr. HARVEY LITTLEJOHN showed—(1) Two specimens of hypertrophied heart from cases of sudden death, weighing respectively 28 oz. and 48 oz.; (2) a heart showing rupture of the internal surface of the left ventricle; (3) a specimen of effusion of blood on the surface of the brain in which the colour had been preserved.

COMMUNICATIONS.—Dr. **BRUCE** read a paper on "The Localisation and Symptoms of Disease of the Cerebellum, considered in relation to its Anatomical Connections." The symptoms of cerebellar disease can be grouped into two classes—(1) *General*—Headache, optic neuritis, and vomiting; (2) *Special*—Reeling gait, rolling of eyeballs, etc. The symptoms vary according to the seat of the lesion in the cerebellum, inco-ordination being only met with when the middle lobe is affected. A lesion absolutely limited to one hemisphere of the cerebellum produced, as a rule, no definite symptoms. The reason for this is seen when one traces up the direct cerebellar tract and the tract of Gowers. These tracts probably act as conductors of muscular sense impressions, and all end in the middle lobe. In this relation the connections of the nucleus of Deiters are important. This nucleus is filled with large cells, like those of the anterior horn, of grey matter. It is connected with the fibres of the auditory nerve supplying the semicircular canals, and a tract passes to the roof nucleus. A bundle of fibres passes from it to the nucleus of the sixth, and another to the nucleus of the third nerve. Another bundle passes downwards into the antero-lateral column of the cord. Thus it is connected with the centres of the ocular muscles, and with the anterior cornua of the cord. The cortex of the middle lobe is connected with the roof nuclei by means of fibres which are easily seen in the fœtus. The integrity of this chain seems to be essential to the maintenance of equilibrium. Fibres pass up through the superior cerebellar peduncle, and end in the motor cortex. If these are destroyed, loss of equilibrium results. If the inferior peduncle is destroyed there is falling to the side of section. A lesion exactly in the middle line gives rise to symmetrical loss of function, and so symptoms are latent. When the nucleus of Deiters is destroyed, we have nystagmus, inco-ordination of ocular muscles, and vertigo resulting.—Dr. **SHENNAN** read a paper on a case of aphasia which Mr. **MILES** himself had had under treatment. The case was one of temporo-sphenoidal abscess, resulting from middle ear disease, and was opened in the usual way. The aphasia did not exist before, but came on after the operation. There was paraphasia, but no word-deafness or visual aphasia. There was considerable amount of amnesia, especially for nouns. The symptoms pointed to a lesion between Broca's convolution and the motor fibres. The patient eventually got worse, and died with symptoms of compression. At the post mortem, secondary abcesses were found in various parts of the brain, but none in the situation indicated by the symptoms.

EDINBURGH OBSTETRICAL SOCIETY.—*Wednesday, 11th January* 1899.—Dr. **HALLIDAY CROOM**, President, in the chair. The **PRESIDENT**, Professor **SIMPSON**, Dr. **MILNE MURRAY**, Dr. J. W. **BALLANTYNE**, and Dr. **FORDYCE** showed specimens.

COMMUNICATION.—Dr. **PONDER** read a paper on "Instrumental Assistance in Parturition." He held that instruments should be used in practically every case, but held that the object of these should be to dilate the passages and not to pull on the child. He endeavoured to show that the forceps was an unscientific instrument, and that their chief action—that of traction—was harmful. The proximal part of the blades might be useful for dilatation, but the distal were harmful in compressing and pulling on the child's head. Dr. Ponder showed an instrument he had devised but had never used, whose action was to dilate the passages in front of the advancing head. The paper was adversely criticised by nearly all the fellows present.

ROYAL MEDICAL SOCIETY.—*6th January* 1899.—Dr. SIMPSON in the chair.—Mr. A. G. MILLER described a simple method of reducing dislocation of the shoulder. The surgeon abducts the upper arm to the level of the shoulder, the fore-arm being horizontal and at right angles to the humerus, and then performs internal rotation of the level of the humerus by dropping the hand. The action of the muscles and the locking of the head of the humerus with the lip of the glenoid are thus overcome practically without any force.—A dissertation on chloroform sickness by Dr. T. A. Ross was read by Dr. GOODALL, in which the causes, prevention, and treatment were fully discussed.

13th January 1899.—Dr. M'NAIR SCOTT in the chair.—Dr. MACLAGAN showed two children (brother and sister) suffering from Friedreich's ataxia. The absence of sensory phenomena, although the disease is due to a gliosis or sclerosis of the posterior and lateral columns, gave rise to some discussion.—Dr. STRUTHERS read a dissertation by Dr. COLMAN on Pott's disease, in which he fully discussed the pathology, symptoms, and treatment.—Mr. MACKIE described a case of missed abortion, and showed specimen.

20th January 1899.—Dr. COOPER in the chair.—Dr. FORDYCE showed typical cases of locomotor ataxia and progressive muscular atrophy, also a somewhat obscure case of facial paralysis.—Dr. WATERSTON brought forward for diagnosis a man with most of the left shoulder and arm or forearm muscles paralysed, the result of a fall from a dog-cart on the shoulder. Patient's neck was severely strained. The exact localisation of the lesion was discussed. Dr. Waterston regarded it as probably located in the fifth or sixth cervical nerves close to the column.—Mr. PAULIN gave a demonstration on Dr. Ford Robertson's platinum method of cortical staining. The method promises to be one of importance in neurology.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.—*9th January* 1899.—Dr. FLEMING in the chair.—Dr. FINLAYSON showed specimens from a case of primary sarcoma of the stomach in a young child. Though the number of white blood corpuscles was increased, and the spleen large enough to be felt, a leukæic cause was doubted.—Mr. MAYLARD showed—(*a*) Specimen of a cyst from the bowel wall, distended with a pale yellow, granular, viscid fluid, which, when examined under the microscope, was seen to be composed almost entirely of granular fatty particles with here and there crystals of cholesterin. The cyst completely blocked the canal. (*b*) Specimen of an adventitious diverticulum of the sigmoid flexure, from a case of intestinal obstruction, caused by a band which had passed from the apex of this diverticulum to the jejunum. The jejunum was free from any permanent pouching at the point of attachment to the band.—Dr NAPIER showed the parts from a case of disseminated sarcoma, appearing primarily in the ovaries.

OBITUARY.

A. A. Kanthack, M.D.

It was in November 1889 that the writer first met in Virchow's pathological laboratory in Berlin the tall athletic form of A. A. Kanthack, whose pale but powerful face even then bore traces of severe, almost too diligent toil. Then at work upon the histological and anatomical details of the larynx, on which he had already published original papers in *Virchow's Archiv*, he was preparing to refute the objections taken to his views by many of the German laryngologists, a purpose crowned later on with success. At that time, even as of late, Kanthack was a glutton for work; his whole mind was taken up with the subjects severally claiming his attention. In Krause's laryngological clinique he was as much at home and as highly esteemed as in Professor Virchow's department. Educated for the most part in Germany, Kanthack readily helped those of his compatriots whose knowledge of German extended only to sadly incorrect forms of speech learnt from conversation manuals. In 1891, after his return to England, he gained the John Lucas Walker Studentship in Pathology at Cambridge, and was appointed Lecturer on Pathology to, and Curator of the Museum at St. Bartholomew's Hospital. Later on he went out to India as one of the members of the Leprosy Commission, in which he did splendid work. When the late Professor Roy of Cambridge was laid aside by illness, Kanthack was installed as interim lecturer, and, if memory serves aright, combined his London duties with the necessary visits to Cambridge for teaching purposes. When Professor Roy died—it is not so long ago—Kanthack was appointed his successor. It is truly a misfortune for scientific medicine that two such able occupants of this chair should have been cut off within so short a space of time, in the ripeness of their trained natural abilities, the one by nervous disease, the other by the dreaded cancer.

Professor Kanthack held the degrees of M.D., M.A., B.S., B.Sc.—in all these he obtained honours or gold medals—F.R.C.S. Eng., F.R.C.P. Lond. His intellect was so highly endowed that not one of the many subjects in medicine, to which his mind was from time to time attracted, but was enriched and enlarged by his investigations. His only fault, if it be as much, was some degree of disregard for his environment. The last occasion on which the writer met him he was hard at work in a laboratory at St. Bartholomew's, where he had been all day in an atmosphere worse than stuffy, almost pathological. A well-known football player in his student days, his naturally strong physique was too frequently tried and strained, and although his death was due to cancer, we believe that the primary cause lay in his intense devotion to work, and to the extraordinary efforts made by him to overtake more than even his great power of endurance could sustain.

His chief contributions to medical literature comprise a "Handbook of Practical Bacteriology," a "Manual of Practical Morbid Anatomy," and many valuable papers on bacteriological and cognate subjects.

"Si monumentum quæris, circumspice."

SIR JAMES MOUAT, K.C.B., V.C.

FULL of years and honours, this distinguished veteran passed quietly away in London on 4th January. The son of an army medical officer, Sir James became an Assistant-Surgeon during the first year of the Queen's reign, and was steadily promoted to the higher ranks of the service. He received the Victoria Cross for conspicuous bravery at Balaclava, and served later with brilliant distinction during the Maori campaigns. He afterwards filled several important administrative posts, and finally left the service with the rank of Surgeon-General in 1876. He became Honorary Surgeon to the Queen in 1888, and Military Commander of the Bath in 1894. He will long be held in respect as a doughty champion of the army medical cause.

JAMES CAPPIE, M.D.

AFTER a brief illness, Dr. Cappie has passed from the scene of his long and unostentatious, yet valuable labours. After the usual course of study of his period, much of which was spent at the Minto House of that day, he graduated in 1850, and almost at once entered on the practice in which he spent the remainder of his life. Although a very retiring man, Dr. Cappie possessed strong opinions on many subjects, and, albeit holding no hospital appointment, he nevertheless made several valuable contributions to medical literature, especially on the physiology and pathology of the nervous system. In personal character Dr. Cappie was a man of the most benevolent type, and he endeared himself to very numerous friends by his kindly, sympathetic manners and unwearied patience.

JOSEPH TILLIE, M.D.

THE early death of Dr. Tillie has finally eclipsed the hopes centred in him by a large number of friends. The confident expectations they entertained for his future, which were amply justified by his brilliant promise, were overshadowed by the onset, four years ago, of pulmonary symptoms, for which it was found necessary to seek another climate. He accordingly proceeded to South Africa, and lived as far as possible an open-air life, while practising his profession. The end, however, came on 26th November 1898, at East London.

Dr. Tillie, who graduated with the highest honours as M.B. Edin., in 1886, very early made his mark as a pharmacologist, and has left work bearing the impress of distinctive individuality, and possessed of permanent value. But, brilliant though his work was in this direction, Dr. Tillie attained even greater success as a teacher, both systematically and clinically, while acting as Assistant to the Professor of Materia Medica, and Clinical Tutor in the Royal Infirmary.

Personally, Dr. Tillie was a man of sterling worth, at once loyal and generous, and his lovable character endeared him to a wide circle of friends who now mourn his loss.

ANALYTICAL REPORTS.

KLOSTERLE MINERAL WATER.

THIS water contains 2·15 per cent. solid constituents, reckoning the carbonic acid compounds with bases as bicarbonates free from water. It is particularly rich in carbonates, containing such salts of potassium, sodium, lithium, magnesium, strontium, iron, and calcium, in addition to carbonic acid, in the form of its incompletely saturated compounds, *e.g.* of bicarbonates, as well as a considerable proportion in a free state.

The official analysis supplied us refers also to traces of protoxide of manganese, and of oxides of barium, titanium, and copper. The constituents of the water suggest its use in cases of gouty and rheumatic habit; and its pleasant taste and general condition should assist in making it popular. It is palatable, slightly effervescent, and mixes well with stimulants, should their addition be permissible, and can be recommended as an excellent type of a carbonated water.

EUCRYL.

(Major & Co., Hull.)

THIS substance is a very pleasant antiseptic reagent, agreeably scented with one of the odours derived from pines. It has been sent us in two forms—the first, Bath-Eucryl, is a solution for addition to water in baths, basins, etc., or for application as a lotion to the skin after dilution, in cases of hyperidrosis, bromidrosis, dermatitis, and similar conditions; and the second, Eucryl powder, a rough powder, in a canister with a perforated cover, is for use as a disinfectant of drains, excreta, etc. We have not had a practical opportunity of recording its antiseptic power upon the micro-organisms of infection, but we can testify warmly to the refreshing and stimulating effect induced by its mixture with the water of the daily tub, and to the noticeable improvement in the condition of the skin which, *post hoc* or *propter hoc*, followed its use.

The addition of Bath-Eucryl to water, somehow or another, gives it an agreeably soft and velvety feel, as well as a pleasant odour. The admixture of the two presents a whitish opalescent appearance. We can recommend Bath-Eucryl highly for its invigorating and pleasant properties, when used for toilet purposes; for its healthy influence on the skin; and Eucryl fluid for its freedom from any penetrating carbolic-like odour, when employed for sanitary purposes.

HOMMEL'S HÆMATOGEN.

(Nicolay & Co., Zürich and London.)

WE have examined a sample of Hommel's hæmatogen, sent us by the makers. It is a dark, clear crimson-lake coloured thick fluid; a specimen, in fact, of "laked" blood, all the fibrin and corpuscular elements having been removed by one or other of the methods available, by which one is not noted. Whatever method is actually employed, the result is completely successful, the fluid being nearly free from matter in solid form. Its reaction appears to be neutral. No decomposition had occurred in the fluid. On analysis, we found its composition to be as follows :—

	Hæmatogen, per cent. of bulk.	Blood of Ox, per cent.
Water	71·53	79·959
Total solids and glycerin .	28·47	20·041
Ash	1·26 (4·42 p. ct. of solids)	0·821 (4·96 p. ct. of solids)
Organic solids . . .	27·21	19·220
Coagulable proteids . .	11·037	10·31 (fibrin and albumin)
Hæmatin (*circa*) . .	0·4	...
Other proteids . . .	1·010	...

Spectroscopic examination showed the pigment present to be pure oxyhæmoglobin. The filtrate, after coagulation of the proteids with heat and acid, was absolutely free from colour, but contained a small quantity of proteid bodies precipitated by ammonium sulphate, probably proteoses.

Our examination, therefore, confirms the composition of this preparation claimed for it by its maker, and shows it to be an absolutely pure solution of oxyhæmoglobin, suitable for administration to patients. It is especially recommended for children, in 1- to 2-teaspoonful doses; 1 to 2 tablespoonfuls for adults. Possibly the advantages to be gained from the use of hæmatogen might be extended were it found practicable to disguise the appearance it presents so like that of blood.

TANNALBIN.

(Knoll, Ludwigshafen: Agent, Kühn, London.)

TANNALBIN is formed by a combination of tannic acid and albumin, after they have been heated at 110°–120° C. for several hours. In fact, it is a tannate of albumin containing about 50 per cent. of tannin. As tannic acid is well known to coagulate albumins which are in solution, this compound is naturally insoluble in water; it is not affected by dilute acids, but alkalies in dilute form cause it to dissolve, and enable it to exert an astringent action. Judging from personal experience of the action of tannic acid upon proteids, we should suppose that the alkali unites partly with the albumin, setting free some tannic acid, and should say that it is improbable that no reaction occurs in the stomach. It appears likely that some at least of the tannic acid is replaced by other acids, the part set free combining with other proteids present. In the bowel the common predominance of organic acids over the alkaline substances might, in dealing with such an albuminous compound, have more to do with the after-reaction than is supposed, especially as much of the alkali and acid produced combines to form salts. That the tannin portion of such substances is partly broken up, and partly absorbed and excreted in the form of gallic acid, is shown by the presence of the latter acid in the urine along with various complex derivatives of tannin. Vierordt, whose paper was given in abstract on page 103 of our last January number for last year, regards tannalbin as the best of intestinal astringents.

A very convenient form by which it can be given pleasantly and efficiently is supplied by the makers in their tablets of tannalbin (Knoll). Each tablet contains 5 grs. of the active agent made up with cocoa and sugar, and can be crushed and taken as a powder, or more simply as a bon-bon, and broken up in the mouth. The ordinary dose for adults is from 15 to 30 grs. up to 150 grs. *per diem*, for children 5 to 10 grs. up to 60 grs. daily. Should the high value set upon this tannin compound, by the various authorities who have lately investigated its powers, be substantiated upon further trial, tannalbin must become a veritable boon to all those called on to treat inflammatory intestinal conditions, which apparently are so often easily allayed, but which more often are prone to reappear with increased intensity, from the irritation caused by the *force majeure* that tends temporarily to obscure the signs afforded by them.

TURTLE SOUP.

(T. K. Bellis, St. Mary Axe, London.)

TURTLE SOUP, so well known but so rarely tasted, that is to say in its true form, by the majority of persons, is a dainty perhaps more appreciated after repetition than properly valued on first acquaintance. It is an easy form of sarcasm to ascribe the merits of turtle soup to stimulation of the gustatory nerves by way of the higher centres, the perception of its price; but the soup is nutritious, its flavour rich and delicate, and the price as supplied in pints at 5s. each is not at all out of the way, when compared with other specially preserved foods and delicacies. The soup is put up in tins and glass jars, each holding a pint; two pints, with a quart of clear stock, sufficing to afford enough soup for twelve persons at dinner, and might serve an invalid for some time. There is no trace of any admixture with preservatives, nor the slightest trace of any flavour attributable to contact with the metal surrounding it or with anything during its preparation.

The sample of turtle soup received was clear, in good order, of perfect flavour, and made a perfect clear soup with some stock. The maker (T. K. Bellis) supplies various other preparations made from the tissues of *chelonia*,—calipee, calipash, turtle jelly, and so on. The turtle jelly is said to be an excellent restorative in convalescence, but we have not had an opportunity of trying it.

MONTHLY NOTES
ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,

MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

Meteorological Means for the East of Scotland District, for the Five Weeks ending December 31, and for the Year from January 2.

	Temperature.			Rainfall in Inches.		Sunshine. Hours.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees.						
		Above 42°.	Below 42°.	Actual.	Accumulated from Jan. 2.	Actual.	Accumulated from Jan. 2.	
Mean .	37°·0	*[1] 45° [2]2362°	163° 919°	3·4	32·0	37·0	1275	3°·3
Observed	41°·3	[1] 89° [2]2617°	120° 619°	3·60	30·4	29·0	1314	3°·8
Difference	+4°·3	[1] +44° [2] +255°	−43° −300°	+0·2	−1·6	−8·0	+39	+0°·5

Vital Statistics.

Mortality in Four Large Towns in the East of Scotland, during December, and since January 1. Actual Numbers and Rate per 10,000 per annum.

	Total.		Constitutional Diseases.		Circulatory System.		Nervous System.		Respiratory System.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1896 . .	[1] 1,321 [2]13,256	234·0 195·7	214 2535	38·0 37·4	115 1229	20·0 18·1	135 1494	23·7 22·0	334 2652	59·0 39·2
Actual Deaths and Rate per 10,000	[1] 1,176 [2]13,305	208·1 196·4	215 2766	38·0 40·8	118 1281	20·8 18·9	127 1513	22·6 22·3	278 2370	49·2 34·9
Difference from Mean	[1] −145 [2] +49	−25·9 +0·7	+1 +231	±0·0 ∓3·4	+3 +52	−0·8 +0·8	−8 +19	−1·1 +0·3	−56 −282	−9·8 −4·3

	Digestive System.		Urinary System.		Pneumonia.		Phthisis.		Cancer.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1896 . .	[1] 75 [2] 863	13·2 12·7	32 345	5·6 5·1	110 953	19·4 14·1	107 1262	18·9 18·7	50 574	8·8 8·4
Actual Deaths and Rate per 10,000	[1] 77 [2]1039	13·6 15·3	31 323	5·4 4·7	118 1077	20·8 15·9	96 1256	17·0 18·5	49 657	8·6 9·7
Difference from Mean	[1]+ 2 [2]+176	+0·4 +2·6	−1 −22	−0·2 −0·4	+8 +124	+1·4 +1·8	−11 −6	−1·9 −0·2	−1 +83	−0·2 +1·3

* Note.—The figure [1] denotes the data for December, [2] for 1898

The five weeks which ended on the last day of 1898 were marked with as unusual meteorological conditions as the previous months of 1898 almost invariably presented. The mean temperature was as much as 4°·3 F. over the normal; the day degrees above 42° F., 44 in excess; those below 42° F., 43 in defect. The rainfall was practically normal, the temperature variability high, the sunshine deficient. The three weeks, from the second to the fourth of the period examined, were most abnormally warm, with a mean of 43°·2 F., or 6°·3 over the normal.

The figures for the whole year demonstrate the large increase of warmth experienced and the still greater lack of cold weather. The amount of warmth, calculated as accumulated day degrees above 42° F., reaches a figure 255° over the average; the amount of cold shows a deficit of 300° from the usual value of day degrees below 42° F., or 10·79 and 32·6 per cent. respectively. The rainfall was 1·6 in. or 5 per cent. below the normal, the sunshine value rather over it.

The number of deaths in the four larger towns in the east of Scotland was 145 below the number expected, or 10·9 per cent. less than the average. The figures for all the systems included, except that of respiratory diseases, are practically normal. Diseases of the lungs, however, show a figure 16·7 per cent. below their average. Influenza caused four deaths.

For the whole year 1898, the total excess of deaths over that calculated for only reaches 49, or 0·5 per 10,000. The most striking facts presented by the values for the individual systems are shown by the decrease of deaths from total respiratory causes, equal to 10·6 per cent., notwithstanding 13 per cent. increase over the mean from pneumonia, and the large excess of deaths due to diseases of the alimentary organs, 20·39 per cent. over the normal. Deaths from constitutional maladies also were much above the usual, 9 per cent., and from cancer 14 per cent.

December, therefore, very mild, with an average rainfall, decreased sunshine, extraordinarily persistent west winds of greater strength than usual, and an almost entire absence of frost, was characterised by a greatly decreased mortality, and a marked diminution in deaths from respiratory diseases.

The whole year, warm, especially marked by the abnormal absence of cold weather (below 42° F.), dry, with rather more sunshine than usual, affords a practical normal total number of deaths, a large increase under constitutional and digestive diseases and pneumonia, a marked decrease in those from total respiratory affections. Deaths from circulatory and nervous diseases and from cancer were above the average, from urinary maladies and phthisis slightly below.

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ORIGINAL COMMUNICATIONS.

THE TREATMENT OF OZÆNA, WITH SPECIAL REFERENCE TO CUPRIC ELECTROLYSIS.[1]

By P. M'Bride, M.D., F.R.C.P.Ed., F.R.S.E., *Surgeon, Ear and Throat Department, Royal Infirmary; Lecturer on Diseases of the Ear and Throat, Edinburgh University.*

As the term ozæna has been used in a somewhat vague manner throughout medical literature, I may perhaps be permitted to give a very brief definition of the disease as it is now understood by specialists.

The affection is characterised by atrophy of the nasal mucosa, and sometimes even of the osseous structures of the turbinateds. This atrophy depends upon—(1) Fatty degeneration of the glands, followed by disappearance of these structures; (2) fibroid changes in the mucous membrane. At some period—some say from birth—the ciliated epithelium changes its character and becomes flat and horny, the ciliæ of course becoming lost. As a result of these alterations, there is a tendency for the nasal secretion to dry up rapidly when brought into contact with air, and to form greenish crusts, which emit the horrible stench so familiar to all who have met with cases. It is of importance to note that the fœtor is not fully developed until the secretion has dried. This can be easily demonstrated by cleansing the nares and then plugging with absorbent wool. The latter, if left in for some hours, is found to be soaked with fluid secretion, but often almost devoid of odour.

On examining the nares of a patient afflicted by ozæna, after removal of the crusts, no ulceration or lesion of any kind will usually be detected, excepting the marked atrophy which is commonly most noticeable in the inferior turbinateds.

Of late years numerous organisms have been detected in the secretion. Thus Löwenburg, Hajek, and Paulsen have each described bacilli; but in no case, so far as I am aware, has one been found capable of reproducing the disease. Belfanti and della Vedova have, as a result of their investigations, ascribed ozæna to an organism closely resembling if not identical with the Löffler bacillus of diphtheria, but have adduced no more definite proof in favour of their views than the other observers referred to.

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[1] Read before the Medico-Chirurgical Society, Edinburgh, 1st February 1899.

The prognosis of ozæna is, as every practitioner knows, not satisfactory. The affection usually begins about puberty, and sometimes towards middle life the worst symptom—viz. the fœtor —disappears, while the atrophy persists. Under continued local treatment the odour may often be masked, and until recently this was considered the best result capable of being achieved.

It would serve no useful purpose to discuss the various remedies which have been proposed for this all but incurable malady. Indeed, to do so would occupy much time without yielding any satisfactory results, for, as in all intractable affections, so in ozæna, innumerable drugs have been advocated. On the other hand, it will be well to glance briefly at the therapeutic principles which have been invoked, and to consider very shortly some of the methods which have been utilised in carrying them out.

1. *Destruction of the secreting area.*—It was many years ago proposed to destroy the lining membrane of the nose by means of scraping instruments, but this method has not to any appreciable extent survived the criticism of Voltolini, who, in his great work,[1] writes, " I never employ scraping with a sharp spoon; I leave that to others, and observe what effect they have produced. Thus I have observed that it does absolutely no good, and that the nose rather looks worse than before," etc.

2. *The application of stimulants and irritants.*—The employment of tampons of wool, as suggested by Gottstein, is an efficient palliative, as by their use fœtor is reduced. It is, however, difficult to get patients to tolerate plugging of the nostrils for a sufficient length of time. The application of irritating liquids, such as spirits of mustard, and caustics of various kinds, has been much recommended, and good results have been especially attributed to spraying with a concentrated solution of nitrate of silver (as suggested by Meyjes.). Mechanical stimulation has also been advocated. Thus Fränkel suggested light touching with the electric cautery; while Braun, Laker, and Demme have advised vibratory massage of the mucous membrane. This is carried out by introducing into the nostril a probe surrounded by cotton wool, which may be saturated with a suitable medicament. By bracing the arm, the instrument is made to vibrate rapidly. As the exertion required has been found somewhat fatiguing to operators, electric motors have been suggested as substitutes for the hand. This treatment seems really to have been attended by success, and Schech states that the mucosa may thus not only be brought back to the normal,

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[1] "Die Krankheiten der Nase," Breslau, 1888.

but even caused to become hypertrophied.[1] Chiari, on the other hand, ascribes the benefit derived to the irritation and to the solutions usually employed at the same time. He has obtained good results by painting the interior of the nose with Mandl's solution. The valuable effects of this last-named treatment I can fully confirm.

3. Since the widening of the nasal channel, which results from the atrophic condition, tends to make elimination of secretion difficult, it has been proposed to use apparatus for producing partial occlusion—so-called nasal obturators.

4. As remedies which do not come exactly under any of the above headings, I may mention a spray containing borax and 70 per cent. of glycerin, as suggested by Musebold; and ichthyol in solution, as advocated by Eitler, who uses the drug for syringing (2 to 5 per cent.) and painting (25 to 30 per cent.).

All these methods have been employed, some of them have yielded a degree of success, but still the fact remains that in many quarters careful cleansing of the nares, by means of alkaline, antiseptic, and other solutions, is looked upon as the most satisfactory treatment which can be instituted.

Of late, two novel and strangely divergent lines of treatment have been advocated, to wit—(1) The injection of diphtheria antitoxin, (2) cupric electrolysis.

As already stated, Belfanti and della Vedova[2] believed that they had found an organism in ozæna which was practically identical with the Löffler bacillus. They therefore employed antitoxin in thirty-two cases, and recorded sixteen cures and more or less improvement in all the others. Gradenigo[3] to a considerable extent confirmed the statements of these two authors, *i.e.*, he found that benefit and even cure resulted from the injections; he, however, soon after advised intramuscular injections of iodine as a substitute. Arslan and Catterina[4] also experimented with antitoxin, and admitted that it had a considerable effect on the mucous lining of the nose. They found the injections often followed by epistaxis, the mucosa became more moist and vascular, and even the odour was modified, but all the improvement rapidly disappeared. Another observer, Compaired,[5] found marked benefit, so far as the local affection was concerned, but the repeated injections produced fever and rashes.

I think that, from the above observations, we must deduce that a very definite, although probably temporary effect, is produced by antitoxin. I may, however, say at once that I have never employed it, because it seems hardly fair to subject patients to prolonged injections of a powerful remedy of this kind, which may be far from harmless, unless much more convincing proofs of its efficacy can be adduced.

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[1] Krankheiten der Mundhöhle des Rachens und der Nase," Leipzig u. Wien, 1896, S. 297.

[2] *Rev. hebd. de laryngol., etc.*, 2 Mai 1896.

[3] *Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, Aug. 1896.

[4] *Rev. hebd. de laryngol., etc.*, 17 October 1896.

[5] *Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, Mai 1897.

CUPRIC ELECTROLYSIS.—According to Moure,[1] Garrigou-Désarènes was the first author who proposed the employment of electrolysis. He, however, applied only one pole to the surface of the nasal mucosa, while the other rested upon the neck or some other indifferent part. His method was followed in a more or less modified manner by Delavan of New York. Then Jouslain suggested interstitial cupric electrolysis, in an article which unfortunately has not been accessible to me, but which Moure[2] states was communicated to a French scientific society in 1892. This method of treatment was not apparently referred to again until 1895. Cheval then described it, and at the same time recorded 90 per cent. of cures, and in seventy of his cases attained this result after a single séance. His paper was communicated to the Society of Belgian Laryngologists and Otologists,[3] and later this body appointed a committee of four to inquire into the effects of cupric electrolysis. Seven cases were treated by Cheval, and in none of them could the members find any improvement. In 1896, Bayer[4] gave a long account of the method and its results, which, according to him, were extremely good in a large proportion of cases. During the following year, Réthi[5] expressed the opinion that the benefits obtained from cupric electrolysis were greater than those derived from any other method of treatment.

Brindel, at the suggestion of Moure,[6] undertook the treatment of thirty cases in the clinique of the latter. He found that in every instance the nasal affection was modified for some days; the mucosa became swollen, red, and congested; the yellowish green thick crusts no longer formed on the side operated upon; in their place sticky mucus, sometimes tinged with blood, was found, while fœtor disappeared. Brindel, however, observed that relapses often occurred after from one to eight weeks. Nevertheless, in ten cases, permanent benefit followed the treatment. In none of these, at the time of writing, were there either crusts or odour, the patients having been under observation from three to eleven months.

The latest work upon cupric electrolysis with which I am acquainted emanates from Gouguenheim[7] and Lombard, who arrived at the conclusion that cupric electrolysis has a definite action on ozæna, causing disappearance of fœtor, but that it is not possible to say that the result will be permanent.

I do not think that cupric electrolysis has yet been touched upon in English literature, and with one exception its use seems to have been, so far, confined to Belgium and France. Struck by the preponderance of favourable evidence, I began to experiment with this treatment in 1897, and shall give my results as briefly as possible.

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[1] *Bull. Soc. franc. d'Otol., etc.*, tome xiii. p. 1.

[2] *Op. cit.*

[3] *Centralbl. f. laryngol.*, 1896, S. 406.

[4] *Rev. hebd. de laryngol.*, Mai 1896.

[5] *Wien. klin. Rundschau*, 1897, No. 10.

[6] *Op. cit.*

[7] *Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, Nov. 1898.

Case 1.—W. MacD., æt. 26, labourer; conscious of having had nasal trouble since 10 years of age, odour offensive to friends, no sense of smell, great atrophy of inferior turbinateds, with marked crusts. Electrolysis performed five times. The first séance was in October 1897, the second a month later, and the others at intervals of a fortnight. He at once began to improve, the smell disappeared, and the secretion became liquid.

When last seen (13th Dec. 1898) he had done no syringing, but had occasionally to clear the nose; there was, however, no bad smell. On examination, a somewhat moist detached crust was found in the right nostril, while the mucosa on the left side was clear and moist. The parts were still atrophied. A good deal of fluid secretion was present in the naso-pharynx.

Case 2.—K. M., a girl, æt. 17, seen first in 1897, suffering from crusting, fœtor, and atrophy. During eighteen months has had four sittings; has never syringed during this time.

13th December 1898.—Keeping well; no fœtor; occasionally something on handkerchief which comes away easily.

Objective.—Right nostril.—Small non-smelling crust on floor; some moisture; middle turbinated slightly glazed. *Left nostril.*—General moisture.

Atrophy remains in both; naso-pharyngeal secretion causes inconvenience.

Case 3.—R. J., female, æt. 20.—Seven years of crusting and fœtor. No subjective sensation of smell; crusts very adherent. In November 1897 electrolysis was employed, and repeated twice afterwards. Smell diminished, but did not cease; crusts came away more easily, and there was fluid discharge from the nose. About six months afterwards she noticed that the condition was again worse, but it has never been so bad as before the first séance. In November of last year (1898), electrolysis was again employed. When last seen, on 15th December 1898, it could hardly be said that there was appreciable improvement, as the patient had to syringe twice daily to prevent crusting. This patient was again seen on the 24th January 1899. She was told not to syringe, and on the 26th there was no fœtor, but some moist crusts were seen, both in the anterior and posterior nares. On the 31st there was no fœtor, but crusts had accumulated in the naso-pharynx and larynx, although hardly at all anteriorly.[1]

Case 4.—C. C., female, æt. 24.—When first seen had suffered from bad smell and crusts for ten years. The inferior turbinateds here were not so much atrophied as in the other instances, but the case was still a pronounced one. Cupric electrolysis was used in the right nostril on

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[1] This patient was shown at the meeting, having done no syringing since the 29th January, and no odour could be detected.

25th July 1897. On the 27th the condition was much improved. No more crusts; odour hardly perceptible; general moisture, excepting on middle turbinated, while a good deal of fluid muco-pus was seen lying about.

1st August 1897.—Electrolysis again employed; this time in left nostril.

7th August 1897.—More crusting; very slight odour; electrolysis again.

Examined for the last time, 12th December 1898.—She then stated that she relapsed two or three weeks after last sitting, and she thought she was very much as before treatment, requiring to use the syringe daily.

CASE 5.—A. D., female, æt. 30.—For more than two years suffered from considerable crusting and offensive odour. Marked atrophy of inferior turbinateds.

25th July 1897.—Cupric electrolysis applied to right nostril.

27th July.—Marked improvement in both nostrils; no odour; no crusts; general moisture and some fluid muco-purulent secretion.

27th August.—Electrolysis applied to left nostril; no syringing for a fortnight; no odour or crusts.

Examined 10th December 1898.—Keeps very well. During the preceding four months had only required to syringe the nose once. There was a little discharge from the nose.

CASE 6.—M. M., female, æt. 22.—History of crusting and fœtor for six years; atrophy of inferior turbinateds; a bad case. Cupric electrolysis was first employed in June 1897, and was repeated every three weeks, until six sittings had been accomplished. Patient, writing on 14th December 1898, stated that she had never syringed her nose since the treatment; that the odour had never returned, but that a few crusts still continued to come away.

CASE 7.—J. B., male, æt. 19.—Had crusts and fœtor for five or six years; had syringed for some years. In May 1898 electrolysis was used for left nostril, and a fortnight later the right was also treated. A third and fourth sitting followed. The trouble disappeared until the middle of September, when fœtor and crusts recurred, and the patient resumed syringing, which had been given up. On the 27th December 1898 he was very much as before treatment was begun.

CASE 8.—J. B., female, æt. 20.—Crusts, fœtor, and atrophy for one year (?). She also complained of feeling bad smell and taste. No previous treatment; electrolysis, October 1898. When examined in January 1899, felt much better. No perceptible odour, but slight crusting and persisting atrophy.

Although a much larger number of cases were treated, I have been unable to keep records of them, owing to the sufferers having been seen as out-patients, and also because I have not a sufficient staff of clerks. Indeed, I am very much indebted to my friend Dr. Logan Turner, who kindly kept the records I have just given. It may, however, be permissible to state that the cases given were in no sense selected. The strength of current used varied from 3 to 10 milliampères, rarely exceeding the latter. Cocaine was used in most, if not in all, and, after cleansing the nostrils, the copper needle attached to the positive pole was inserted into the inferior or middle turbinated—sometimes into the tissues lining the middle meatus, while the platinum (or steel) needle was passed into the septum. Usually the patients complained of little pain, nor did they experience disagreeable after-effects. A private patient, on whom I recently performed electrolysis, however, suffered a good deal from neuralgia, swelling of the eye, and general disturbance for a day or two. As a rule, each sitting lasted about ten minutes. As I have said before, the cases quoted were not selected by us, but it is just possible, nevertheless, that a process of natural selection may have been brought about. They all occurred in out-patients, and it is quite conceivable that the majority of those who were benefited repeatedly presented themselves, while there may have been an opposite tendency on the part of those who received little relief. It will be noted that out of the eight patients whose histories have been briefly given, four (Cases 1, 2, 5, 6) were practically cured for long periods, extending to eighteen months. In one (Case 8) there was marked improvement; in one (Case 7) apparent cure for some months, but then syringing had to be resumed; while in two (Cases 3 and 4) there was only improvement for a few weeks. The former is here to-night, and it appears to me that the fœtor has been practically cured, but that what she complains of is the discomfort caused by crusts accumulating in the naso-pharynx and larynx.

In the last sentence, when I use the term "cure," I refer to the fœtor, for in most of the cases the atrophy remained as before. I am quite prepared to admit, as above stated, that the results achieved in these seven cases may have been beyond what we can reasonably expect to average. Still they prove that in cupric electrolysis we have a valuable therapeutic resource, probably the most valuable that has yet been suggested, for ozæna. The rationale of the treatment is still uncertain, but probably the formation of copper salts at the positive pole has at least as much effect as the electric current.[1]

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[1] It will be noted that the application of electrolysis to one nostril in our patients often produced benefit, temporary or otherwise, on both sides. This is in accordance with the experience of most other observers.

ON THE RELATION OF THE NERVOUS SYSTEM TO DISEASE AND DISORDER OF THE VISCERA: THE DISORDERS OF VISCERAL MOTION.[1]

By ALEXANDER MORISON, M.D., F.R.C.P.Ed., *Physician Out-Patients, Great Northern Central Hospital, and Children's Hospital, Paddington Green, London.*

(*Concluded from page* 168.)

Body and mind.—The brain, regarded as one of the viscera, both from its own interest and importance in the organism, and from the nature of this lectureship, is worthy of separate consideration, although I have endeavoured, in what has already been said, to show its intimate connection with the rest of the body, and thus to divest it of that glamour with which the older physicians and metaphysicians invested it, and which blinded them to the essentially visceral nature of the organ of mind. One can respect both their veneration and their prejudices. These were, however, partly founded on fear—fear lest a rude prying into the secrets of consciousness might have robbed them of some of that sense of the sanctity and mystery of life, the capacity to entertain which is part of the nature of man. Their fears were groundless. The crude splutter of an occasionally recrudescent materialism dies out at each ineffectual attempt to illuminate. Man will have none of it. The creature that can entertain the idea of the Infinite will be satisfied with nothing less. Did we, moreover, bear in mind more constantly the material substratum of mind, and the possibility of injuring through the mind, not some diaphanous and impalpable condition which would close over the wound inflicted by an inconsiderate word or deed, as the shadowy form of a ghost might over the rapier thrust of a human antagonist; but that discourtesy and cruelty may, as they do, work havoc upon texture, without which mind, as we now understand it, could not exist, it is, I think, probable that, in the absence of an irredeemable brutality of disposition, such a materialistic conception would promote courtesy, gentleness, and the other amiable virtues of altruistic civilisation. The physiology and pathology of the emotions, however, considered in the spirit and by the methods of modern research, is still, I think, an unwritten volume. The cell, considered from the point of view of the metaphysical psychologist, will, I believe, one day yield quite as important results as psychology viewed from the standpoint of the physicist and histologist. In other words, such a volume must be the work of both schools, and, like many human guesses at truth, be a compromise between contending errors.

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[1] The Morison Lectures, delivered before the Royal College of Physicians, Edinburgh.

We have seen that the condition of one viscus may influence the state of another, either through the agency of the nervous system directly, or through the medium of the blood. The nervous system being segmented and with localising reflexes, the influence of a stimulating organ upon an organ stimulated by it may be partial and of a particular kind. Influence exerted through the blood, an omniambient fluid, is general, and may therefore affect many organs. Thus the dislocated kidney may cause tachycardia, and the atrophied thyroid gland myxœdema, with its general depression of vitality and general blunting of motion throughout the body. In order to impress upon ourselves the influence of the body upon the mind, and conversely, the influence of the mind upon the body, it will be convenient to emphasise the visceral nature of the brain, and to examine briefly the anatomical differences between its constituent organules, by which diminutive I mean the different portions of the brain with distinct though associable functions. In its organular constitution the brain is not singular. The liver exercises more functions than one.

The functions of the brain are receptive, retentive, or connective and executive. Just as the functions of the stomach are the reception, digestion, and transmission of food, so the higher brain receives and inwardly digests impressions and transmutes them into action, voluntary, emotional, regulative, and trophic.

If we regard the brain, as physiology teaches us to do, as the seat of sensation, and local sensations in other viscera as those referred thither by the dominating centre, we must also acknowledge the partially independent activities of peripheral organs, and see in these the elements of a reversed process, namely, the stimulation of the centre by the periphery, or, more correctly, the stimulation of the viscus, which is the seat of predominant sensation and motion, by the molecular activities of the other viscera. The empty stomach normally constitutes the hungry man. Fill his stomach and his hunger disappears. The brain appreciates the existence or non-existence of a condition which is in a measure local, namely, appetite. What is true, moreover, of one appetite is true of all. What is true of the semi-independent activity of one organ is true of all organs. The receptive organules of the brain perceive the other-visceral stimulation, the retentive organules prolong the reception, that is, retain impressions, and the executive organules carry out the behests of those functional and persistent products of such impressions, the will and the emotions.

A good deal of not very entertaining merriment has been the result of Descartes' suggestion, that the pineal gland might be the seat of the soul, a suggestion for which he gives very philosophical but not quite satisfactory reasons. More serious writers have attacked him with an earnestness which shows that his views, although indefensible in some particulars, contained a germ of permanent force—of truth. As that Scots worthy, Duncan Forbes of Culloden, remarked, "*Quidquid clare et distincte percipio est verum,*" was the foundation upon which Descartes built, and Forbes acknowledged it was a good one.[1] Like many positive assertions, however, this also requires important qualification. Descartes distinctly perceived the truth that impressions received from various sources by the receptive organules are unifiable into simple conceptions by some portion of the brain. The mistake he made was in being too positive about the seat of such simplification, in his well-known proposition concerning the pineal gland.[2] Taking warning, therefore, by Descartes' example, but pursuing our theme in the spirit of his researches, it will be interesting to inquire whether there is any recognisable anatomical distinction between the systems of organules to which I have referred, and to endeavour to ascertain how that multiunity mind results from their operation, how it is influenced by other organs, and how it influences them. If we but partially succeed, the effort is worth making. The anatomical question, no more than the physiological, is not by any means novel, being as old as the comparative histology of the different portions of the brain, but the collective examination of such histology with a view to associating it with cerebral function is of comparatively recent date. When details of the anatomy and physiology of the brain are better known, it may be confidently anticipated that what is true of the large will also be found to be true of the minute, and that the histological character of a part having a special function will be recognised as being as distinctive as those functions themselves. According to Kölliker,[3] Meynert was the first, in 1867, to note a histological difference in the occipital lobe from other portions of the brain. In this country, Bevan Lewis's work in this direction is well known, his results having first been published in papers,[4] and more recently in his Treatise.[5] Lewis recognised sensory and motor types in the histological character of the cortex cerebri, as well as some subsidiary types which he named from the localities in which they occur. The point, however, which his researches appear to have settled most satisfactorily is the occurrence of the giant pyramids, or, as he prefers to call them, "ganglion cells," most characteristically in small groups in the motor areas of Fritsch and Hitzig and Ferrier. Golgi and others have also investigated this point, and to Hammerberg, a young Swedish physician too soon deceased, Kölliker allots a distinguished place in this sphere of work.[6] To his conclusions I shall refer again.

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[1] "Reflexions on the Sources of Incredulity with regard to Religion," Edinburgh, 1752, p. 43.

[2] "Œuvres," Paris, 1844, p. 224.

[3] *Op. cit.*, p. 672.

[4] *Proc. Roy. Soc. London*, 1878, 1880.

[5] "Mental Diseases" 1889.

[6] *Op. cit.*, p. 674.

The circumstance which first awakened my own interest in the matter was the histological examination of a portion of the temporo-sphenoidal cortex, in which the central portion of the section differed very strikingly in histological character from either extremity, as these also did from one another. It will be observed, by reference to the figures, that in the central portion the cellular elements are more loosely arranged than in the posterior portion, while there also occur what Kölliker calls double pyramids, that is, cells which at either pole have substantial prolongations, which, at about the same distance from the cells, each develop a brush of dendrites. Kölliker states[1] that it is only in the pyriform lobule that he has met with these peculiar cells, and it is only in this region that I also have observed them.

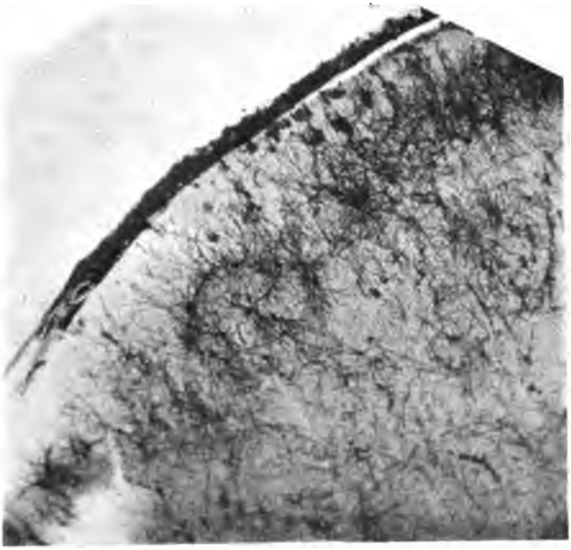

FIG. 1.—Central (receptive) portion of pyriform lobule in a cat's fœtus at term, under low power. Golgi preparation.

In the posterior portion the cells are more closely approximated, and present the characteristic appearance of serried ranks, in which pyramidal cells of the usual type are generally arranged; in this region, however, those of small size preponderate.

The anterior portion, finally, is characterised by polymorphous cells sending their neuraxons in all directions, and being themselves as various in position. These resemble in a measure the cells of the subcortical centres, but may be distinguished by a

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[1] *Op. cit.*, p. 274.

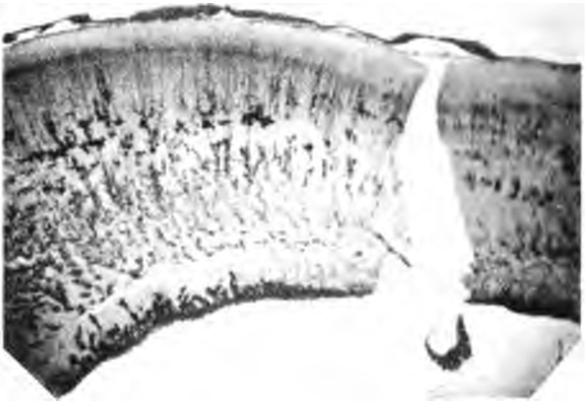

FIG. 2.—Posterior (projective) portion of pyriform lobule from a cat's fœtus at term, seen under low power. Golgi preparation.

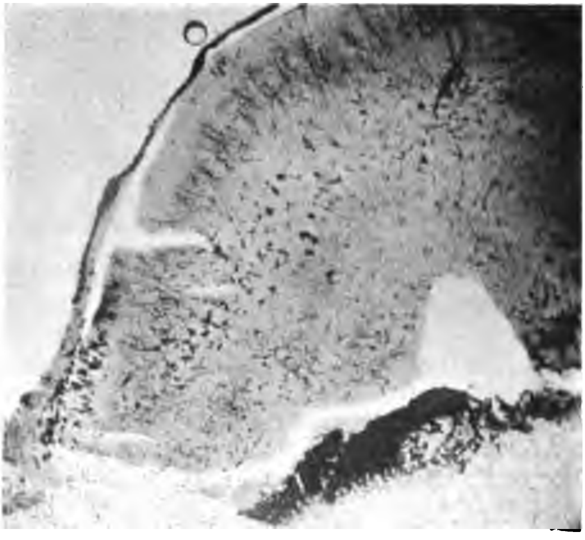

FIG. 3.—Anterior (retentive) portion of pyriform lobule from a cat's fœtus at term, seen under low power. Golgi preparation.

nearer approach to the pyramidal type. Well-stained specimens from the corpus striatum are shown for purposes of comparison.

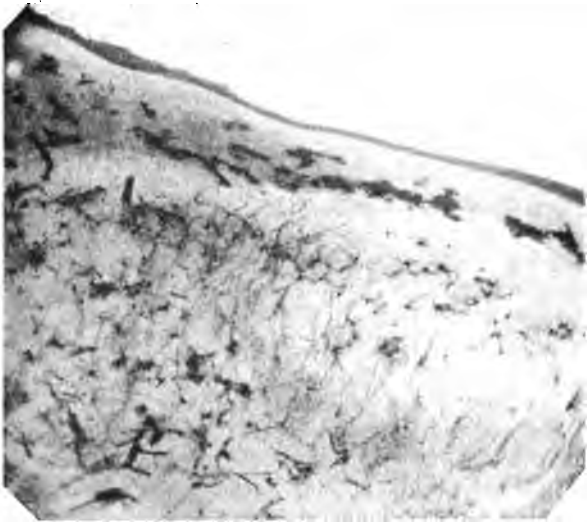

Fig. 4.—Cells and fibres of the corpus striatum of a cat's fœtus at term, seen under low power. Golgi preparation.

The section of the temporo-sphenoidal lobe of which I have spoken was taken from the brain of a fœtal cat at term, and stained by the quick Golgi method. In a section which I made of the same part from the brain of an adult cat, which I received from my colleague, Mr. Peyton-Beale, and which was stained by him by a modification of the Cox-Golgi method of his own devising, I found the same essential features present. Preparations from different portions of the brain also show roughly a specific character in regional histology, but I shall not now examine the matter in greater detail, than by stating Hammerberg's conclusions on this point, which are given by Kölliker[1] as follows:—"(1) It appears that in all convolutions of the usual kind, a layer of small pyramids with some irregular cells, the fourth layer of Hammerberg, is invariably pushed between the medium sized and larger pyramidal cells. (2) The layer of polymorphous cells (Hammerberg's spindle-cell layer) is larger than we have hitherto known. (3) These cells, in so far as they are spindle shaped, lie horizontally in the convolutions. (4) The large pyramidal cells are most developed in the frontal convolutions.

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[1] *Op. cit.*, p. 676.

(5) The giant pyramids only occur in small areas around the central convolutions. (6) Occipital convolutions are distinguished by a large number of small cells. (7) The frontal lobe and the island are distinguished by an unusual number of medium-sized cells, corresponding in all respects with the larger pyramidal cells, while these are themselves altogether absent."

Kölliker does not altogether endorse all these conclusions, but they are sufficiently correct to demonstrate the point under discussion, namely, the histological peculiarities of different portions of the cortex.

It struck me that we might find in the differences shown by the temporo-sphenoidal lobe to which I have referred, a clue to the characters of the receptive, retentive, or connective and projective organules of the brain respectively, and to gain more light upon the subject examined, the cerebral structures concerned in the sense of smell, taken as a type of special sensation.

Kölliker, with the caution and sagacity born of an unrivalled experience, deems it prudent in the meantime to assume that all brain cells act alike, notwithstanding differences in configuration; but, as Bevan Lewis very justly remarks, the association of function with organisation is an accepted doctrine in natural science, and a tentative use of hypothesis is permissible even if the future should cast into the limbo of oblivion as much ingenious speculation as time has already done.

Physiological experiment and the clinical observations of disease in the neighbourhood of the uncinate and hippocampal gyri point to these being intimately associated with the sense of smell. In selecting this sense rather than that of sight for the purposes of argument bearing upon the cortical structures concerned in the evolution of thought from impressions received, I have been guided by the circumstance that these impressions converge upon the chief site, so far as physiologists are at present agreed, of general tactile sensibility. The few remarks I shall make as regards the deeper anatomy of smell must be regarded as suggestive rather than demonstrational; more would not be germane to our present task. Foster and Sherrington suggest[1] that the uncinate gyrus may not be the site of "full olfactory sensations," and that these may find completion in the cornu ammonis. It is not necessary to follow in detail all the different tracts, even so far as they are known, which lead to the supposed centres of smell. It is enough to know, as the authors just quoted state,[2] that a portion of these reaches the front end of the hippocampal gyrus, often called the "uncinate gyrus." There are, however, certain features in the histology of the cornu ammonis which suggest that it may bear somewhat the same relation to the uncinate gyrus as a portion of the temporo-sphenoidal lobe, that the cerebellum does to the cerebrum.

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[1] *Op. cit.*, p. 1181.

[2] *Op. cit.*, p. 1180.

Sir William Turner[1] mentions the fact that Mr. A. B. Stirling recognised a granular layer in the hippocampus which so resembled the similar structure in the cerebellum, that it might appropriately be called the "rust-coloured layer of the hippocampus." In some sections of the cornu I have also met with cells which bear a considerable likeness to the Purkinjean bodies of the cerebellum, though they are smaller than these. If a longitudinal section be made of the cornu ammonis, so as to get the full twist of the spiral to the end of the grey matter, cells may be seen, especially at the very extremity, which exhibit the resemblance I speak of. I show specimens which contain these. The sections also demonstrate very well the fact of the invariably vertical position, relatively to the surface from which they spring, of the pyramidal cells of the cornu, and of the smaller and deeper pyramidoid cells of this structure. The true significance of this comparative histology of the cornu is as little known as that of the cerebellum, but if we are justified in regarding the cerebellum as subordinate to the cerebrum, it appears feasible to suggest that the cornu may also be subordinate to the more superficial areas in the tempero-sphenoidal lobe, some of the characters of which have been briefly sketched. What, then, may be assumed to be the histological character of the sensory area on which the afferent tracts of the sense of smell converge? As we have seen, there are two portions of the lower temporo-sphenoidal section referred to which differ considerably in histological features from a third part. This third part has all the characters of marshalled pyramids of the ordinary type, which are found in portions of the cortex which we know have distinctly motor properties. It would seem, therefore, rational to assume that the sensory function of the temporo-sphenoidal lobe is exercised by those parts which have not the features of the motor cortex; that, in short, the essentially receptive cortex, the cortex for special sensibility, is not that distinguished by pyramids of the ordinary type.

It comes, therefore, to be a choice between two portions. Of these, one, as we saw, has a structure which is apparently peculiar to this portion of the temporo-sphenoidal lobe; the other part has the polymorphous character of those irregular cells which lie chiefly below, but also to some extent among, the pyramids. It would therefore seem rational to assume, and I am aware that I am assuming much, that the part peculiar to this portion of the temporo-sphenoidal lobe is more likely to be associated with its special sensory function, than that portion which has more general characteristics.

I have selected this lobule, and one of the senses running up to it, rather than the centre of vision, because the structure of it and the mechanism of smell seemed so peculiar, and because it is situated in what is avowedly considered the most sensory region of the brain. But peculiarities are also to be found, as has been stated, in the occipital lobe, and we must remember that mind is not the product of one, but of all the senses—of a correlation of the senses by means of which unity, coherence, is evolved from many sources of impression, although not, as Descartes supposed, in the pineal gland. I feel, therefore, disposed to regard the characteristics I have referred to as a chief type of the receptive organule, but not the only type, and desire tentatively to express this opinion with all the diffidence which an acknowledged uncertainty should and, I trust, does inspire.

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[1] "Introduction to Human Anatomy," 1882, part i. p. 291.

The retentive or connective organule.—It may be remembered that I ventured to define mind last year as "a mode of vital motion manifested by some cells of the cerebral cortex, and as the more or less permanent product or effect of such motion which we call memory." Memory or retentiveness, we know, is not an exclusively cerebral property. The behaviour of the decapitated frog is sufficient evidence to the contrary; but physiologists have also shown that the tendency of progress in development is to increase cerebral at the expense of spinal retentiveness. In man, spinal memory is probably reduced to simple reflex action, which is a receptive-motor rather than a retentive-motor phenomenon. For our present purpose, therefore, it is not necessary to widen the subject by inquiring whether there be or not a histological basis to spinal memory. As a study in comparative mnemonic histology, however, the inquiry might not be without interest.

Cerebral memory may be impressional and conclusional; that is, the automatic record of impressions received through the senses, or the record of the results of that inward digestion of such impressions by the recepto-retento-motor process we call thought or reasoning, in the exercise of which more cerebral organules than one take part. The histological characters of the storehouse of memory or the retentive organule need not, however, vary with the method of filling it. If, therefore, there be reason in regarding the histological type of the receptive organule to be such as has been described, and the histological type of the motor organule, on which I shall say a few words presently, to be of the pyramidal type, it seems feasible to argue, or assume or imagine, as you will, that the polymorphous cell, with its universality of axonic direction and equal universality of dendritic receptivity, is the type of that retentive and connective organule, the repletion and utilisation of which is the mainstay of mental life, as the greater or less storage of supplies—potential energy—is the necessary condition for the exercise of any power whatever, or kinetic energy.

The retentive organule in its histological features has some likeness to the cells of the subcortical centres. These may be regarded as an extension of the spinal cord into the brain, and the lower nuclei, we have reason to believe, may be the seat of simple reflex actions like the cord. These are, however, no more independent of the cortex than the spinal cord is, and perhaps no less. The vegetative centres, although beyond the power of will, are not, as we have seen, beyond the power of emotion, and emotion is a cortical, not subcortical, phenomenon, although the motion inseparable from it is, so to speak, involuntary, that is, the executive energy of emotion is projected peripherally before it reaches the seat of will. Between this, however, and simple reflex action there is the important distinction, that the consciousness is fully aware of the efferent event in the case of emotion, which it need not be in the case of simple reflex action.

The projective organule.—I have used the term projective here instead of motor, because I have employed a term intermediate between sensory and motor to distinguish the mnemonic sensory source whence mental motion probably derives a considerable portion of its impulse. I have already mentioned Bevan Lewis's belief that the largest cells of the projective or motor type—the giant pyramids—are to be met with in the parieto-frontal region of greatest provocable movement, as also the practically unvarying direction of the primary axons of pyramidal cells generally. A nerve cell, be it receptive, retentive, or connective, or projective, is of necessity a mixed cell. It receives, transmutes, and transmits impressions. It has an individual resistance which has to be overcome, and in the overcoming of which fresh energy is evolved. There are, moreover, preponderant centres for the evolution of energy, otherwise direction could not be given to motion, and the universe itself would have been a chaotic pyrotechnic display of short duration, if it had existed at all, which, having flared imposingly into being like the rocket, would, like the residual stick, have fallen into the abyss of oblivion in a similarly ridiculous manner.

The preponderance of energy in nerve cells is in the sphere of sensibility or receptivity, because this is the aspect of animal life which is turned, as it were, towards the sources of general energy in the universe which surrounds it, and the impressions received from whence through the senses are converted into various forms of energy, from the simplest reflex act to the highest effort of conscious thought. Kinesis, movement, projection is, however, the preponderant function in common which the executive section of the brain, the projective organule, possesses, and in this specialisation of function lies the argument for specific organisation. The special organ of cerebral kinesis appears to be the pyramidal cell, differing in size and in distribution, as it probably does, in the range of its projective power and in the character of the movement evoked, be it a gross muscular contraction, an imperceptible secretion, or a subtle movement of thought. A thought is a product or outcome of executive energy—*an act*—which has its roots in the imperishable around us, and in impressions within us, and is rendered intelligible to others by symbols which are in a measure a reflex of our feelings, and represent more or less distinctly such impressions. Thought is not consciousness, although to think we must be conscious. "Consciousness" is life manifest in brain, as "motion" in its conventional sense is life manifest in muscle. Both are based upon "irritability," which is the primary expression of life in all living matter. Thought is regulated consciousness, as co-ordinated movement is regulated motion in muscle. Movement in the somatic system is regulated by consciousness, and in the splanchnic by emotion in its various phases of calm and excitement, and in a subordinate degree by a so-called automatic power. Life, therefore, is expressed in irritability and its various manifestations in different organs, but is itself an unexplained fact. Beyond and before "irritability" were the constituents of life. Why not then life itself? And if life, why not consciousness among other manifestations of life? Such primal consciousness is, according to Herbert Spencer and his followers, necessarily existent but unknowable; according to others not less worthy of respect than they, made known. At this point the process men call Science comes face to face with the process men call Revelation, and it is not our duty to pursue this line of thought further. The subject has been ably but too iconoclastically treated by Maudsley.[1]

I offer no apology, however, for this digression, if digression it be, for that physician must have a very limited experience who has not observed both the baneful and beneficial consequences of the influence of these questions on the life and health of his patients. Who has not felt the hopelessness of dealing with the profound melancholy of the religious maniac, or observed the invigorating influence upon the body of a happier conception of the relation of the individual to the forces in and around him of which he is one expression. I had at one time as a patient an elderly lady, who, many years before my acquaintance with her, had for a time been insane. For some years I knew her as a kindly and shrewd woman of sound common sense. One morning I was hurriedly summoned to visit her, and found her recovering from an attack apparently of a syncopal nature. When she had revived, she told me she had, while in the condition stated, had a terrible experience. She had seen hell opened, and filled with all the accompaniments of torture with which students of fantastic eschatology are familiar, and had been apprised of the fact that her irrevocable destiny was to be in flames. The delusion became fixed, and though she lived for several years, the black cloud of her melancholy never lifted for a moment. An interesting visceral concomitant of this state was that a broncho-pneumonic condition, from which she regularly suffered every winter previously while I had known her, and which was accompanied by unmistakable local signs, never recurred. External influences in the presence of her profound sorrow and approaching doom had no influence upon her. As Thekla, about to seek the grave of Max Piccolomini, remarked, "Frei geht das Unglück durch die ganze Erde"—Misfortune travels safe throughout the world.

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[1] "Natural Causes and Supernatural Seemings," 3rd ed., London, 1897.

To convey to others the operations of our consciousness—our thoughts—we have to refer them more or less symbolically to things which their senses may perceive outside themselves, and which we can perceive in common with them. We have each our own Ego, but we may have non-Egos in common with others. In these non-Egos lie the sources of our language, and to their unchangeability and perpetual perceptibility we refer as a standard to determine whether thoughts conveyed to us by another are in agreement with the truth of things or rational, or in disagreement with such or irrational. In the essentially threefold sources of mind one or more factors may be disordered. Language, for example, may be affected in its sensory, its executive, or its retento-motor or connective phase; and in the latter, the disorder may be impressional or conclusional, to use the terms already employed. There may be defect in memory without confusion of ideas, and confusion of ideas with a considerable retention of impressional memory. In all these spheres disorder may be based upon organic or upon so-called functional disease. That functional change may become organic or permanent is a fact which is becoming more appreciated in the present day, from the investigations into the life history of the nerve cell to which I referred in the first lecture. These likewise make it clear to us how peripheral impressions from all parts and organs of the body may act upon the centres, and how also such central effects may react upon the splanchnic and somatic periphery. It is not difficult, moreover, to conceive how the short-circuit motions in the mental organules may lead to excessive action and exhaustion having general consequences, and how a compound of peripheral and central excitations may wear out both body and mind by the unregulated play of the emotions, due to a want of control, which may have its roots in inherited or acquired disease, and of which even moral turpitude may be but a symptom.

I may take the opportunity of stating now that this year, as last year, I am much indebted to my friend Mr. Frank Crosbie of London, for his invaluable aid in producing the photographic illustrations for these lectures. I cannot sufficiently acknowledge his ungrudging expenditure of time and skill to this end.

In bringing to a close this course of lectures, of the many defects in which no one can be more conscious than I am, I have again to express my thanks to the Fellows of the College for the indulgent courtesy which induced them to appoint me to the office, and to pray that the leniency they have already shown may suggest excuses for the inadequate manner in which I have executed my task. I am aware that I have touched but the fringe of a great subject of entrancing interest and supreme moment, the better knowledge of which is calculated to have an important influence upon much that is of interest to us as men and as physicians. If, however, in their opinion I have added anything in the course of these lectures to such knowledge—if, buried in the worthless quartz of much nebulous hypothesis, they should deem it possible to detect some grains of golden truth—I shall feel happy in believing that my labour has not been altogether in vain, and that I have shown myself sensible of the honour they conferred upon me in appointing me to the Lectureship.

THE CAUSATION AND RHYTHM OF THE CRESCENDO MURMUR OF MITRAL STENOSIS.

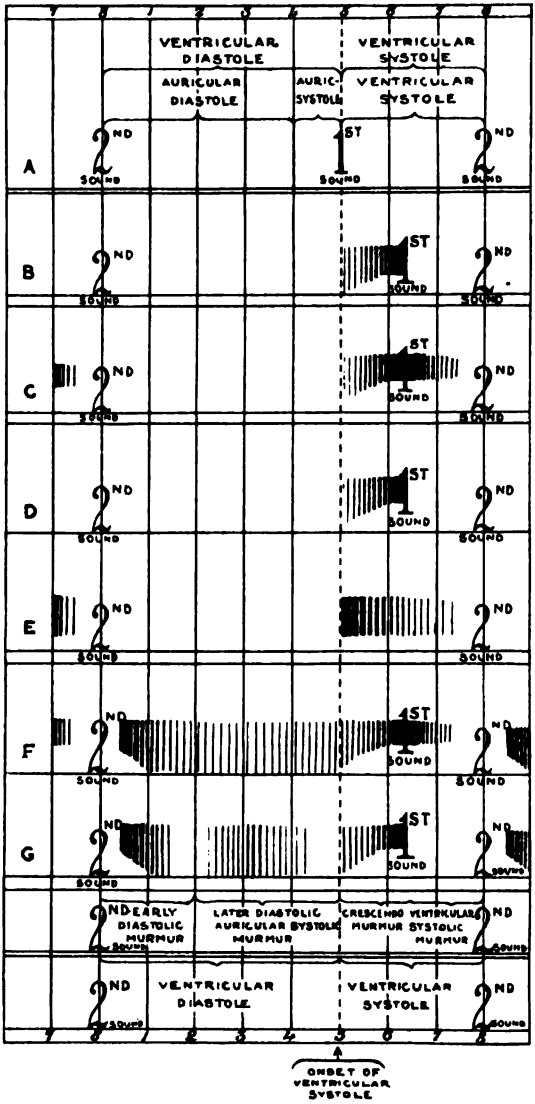
By E. M. Brockbank, M.D., M.R.C.P., *late Resident Medical Officer, Manchester Royal Infirmary.*

Definition.—By the crescendo murmur of mitral stenosis, I mean that abnormal heart sound generally called "presystolic," and less often "auricular systolic," which is practically diagnostic of obstructive narrowing of the auriculo-ventricular valve orifices, especially on the left side of the heart. Though the true crescendo murmur is diagnostic of stenosis of the mitral and tricuspid valves, several observers have described a murmur which they call presystolic, as occurring in other cardiac lesions, and even in dilatation of these same valves. I shall deal with this latter murmur in a future paper, and in the meantime will confine my remarks to the true crescendo murmur of mitral or tricuspid stenosis, and especially of the former affection.

Nature of the Murmur.—The characteristics of the crescendo murmur are known to everyone, but there are one or two points to which I wish to direct special attention. The murmur is short and vigorous, and evidently produced by some strong force. It immediately precedes, and terminates abruptly with, the closure of the mitral valve, an event which is generally marked by an accentuated first sound of the heart, and invariably by the true cardiac impulse. The essential and most peculiar characteristic of the murmur is that it rises progressively from low to high pitch, like an ascending scale of notes, as it rushes rapidly to its abrupt termination. It is also generally of a true crescendo character—that is, it gathers in power of tone coincidently with its progressive rise in pitch. By the abrupt termination of the murmur at its stage of most complete development, the two important features of rising pitch and increasing force are emphasised.

There are, broadly speaking, two types of crescendo murmur, both of which terminate abruptly with the first sound of the heart, one rough and bur-r-ing or spluttering, and the other smoother and more whiffy in character, the latter type being often overlooked by observers. Some murmurs are accompanied by a tactile thrill, which can be palpated at the apex beat of the heart. The rhythm of this thrill is identical with that of the murmur which it accompanies.

Though of variable duration, the true crescendo murmur of mitral stenosis occupies at most not more than half the interval between the second and first heart sounds (Fig. 1, B, C, D, F, G). But it may appear to be much longer than this, for not infrequently it is apparently continuous with an ordinary diastolic mitral bruit of quite different type, namely uniform or decrescendo. In such cases murmurs



IG. 1.—Diagram showing the rhythm and the relative duration of the events of a normal cardiac cycle, and of the murmurs of mitral stenosis in a heart beating 75 times per minute. Each numbered space represents ·1 second; each cardiac cycle occupies ·8 second.

may be heard to occupy the whole of the interval between the second and first heart sounds, either as one long continuous sound or with a slight break between the component bruits (Fig. 1, F, G). Some eminent authorities call this whole composite murmur crescendo, presystolic, or auricular systolic, but such a nomenclature is confusing, as they also apply the same terms to the true crescendo murmur of ascending pitch when it occurs, as it does in the majority of cases, quite separately in the latter portion of the interval between the second and first heart sounds, and immediately before the latter sound. But, be this as it may, I am only concerned with the undoubted crescendo murmur of ascending pitch in this paper, whether it occurs alone or as part of a composite murmur, and I shall confine my remarks almost entirely to this peculiar murmur.

In the accompanying diagram of the rhythm of the murmurs of mitral stenosis, I have departed slightly from the arrangement of lines of various length, thickness, and closeness together, by which Gairdner[1] first represented schematically the varying intensity, rhythm, and duration of these murmurs.

The varying length of the lines in my diagrams represents the comparative *areas* of the stenosed orifice during the production at it of the different types of murmur; the thickness of the lines and their closeness together indicate the *force* of the murmur; and, finally, the elevation of the lines above the base line represents the rise or fall in *pitch* which the crescendo and mitral diastolic murmurs undergo. Thus the diagram of a crescendo murmur (Fig. 1, B, D), interpreted according to the above explanation, signifies that this bruit begins at the very onset of ventricular systole (the line 5), becomes rapidly more and more vigorous, and of higher and higher pitch, as it rushes to its abrupt termination with the accentuated first sound of the heart, and that the orifice at which it is created is open to its widest extent at the beginning of the murmur, and closed, or almost completely closed, at the appearance of the accentuated terminal sound.

Exactly the opposite sequence of events is met with in the development of the early diastolic decrescendo murmur, as indicated by the diagram (Fig. 1, F, G), whilst the later diastolic murmur, part of which is produced (Fig. 1, F, G) in the period of auricular systole, is created at an orifice which remains as widely open as possible during the course of the murmur, and is the result of the action of forces which vary in intensity, and which generally die away more or less before the onset of ventricular systole.

The ordinary systolic murmur of regurgitation in mitral stenosis (Fig. 1, E) is of a lower pitch, and produced at an orifice of greater area of leakage than is the case when the systolic murmur continues the abruptly-terminating crescendo murmur (Fig. 1, C, F).

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[1] "Clinical Medicine," 1862, p. 574.

CAUSATION AND RHYTHM.—Two theories are advanced in explanation of the rhythm, and, to a less extent, of the causation of the crescendo murmur.

1. The *presystolic* or original and orthodox view is that "the murmur is due to the extrusion of blood through the stenosed aperture, under pressure by the auricle, aided by the aspirating force of the ventricle, during the period of diastole of the heart; in most cases it suddenly ceases at the moment of the production of the first sound of the heart; it manifests a heightened intensity at its close, due to the contributory impulsive force of the auricular systole; the first sound which terminates it is usually very short and abrupt, and is due chiefly to the sudden tension of the tricuspid valve and of the walls of the right ventricle."[1]

2. The *early systolic* or other theory is, that the murmur arises from blood escaping through the mitral valve, before it is closed by ventricular systole, and that it is early ventricular systolic in rhythm.

This latter theory, though looked upon as in a way heretical, is gaining more and more supporters; and, though brought up in orthodoxy myself, I have been obliged to join the smaller band of unbelievers. Some of my reasons for doing so were given in a previous paper,[2] and the object of the present communication is to mention further arguments which appear to me to bring more support to this early systolic rhythm theory of the crescendo murmur.

It will be advisable for me to sketch briefly the main points of my former paper. In it I started with an entirely new explanation of the causation of the ascending pitch characteristic, which is so typical of the crescendo murmur of mitral stenosis. The theory I then stated is, that *the crescendo murmur of mitral stenosis is produced by blood regurgitating through the stiff, rigid orifice of the narrowed valve, whilst this orifice, which resists closure, is being rapidly diminished in area and finally obliterated by the action of a strong force, which on its part increases progressively in strength with the duration of the bruit.*

The main point in this theory is, that the ascending pitch of the murmur is imparted to it by the blood rushing through an orifice which is being gradually diminished in area of lumen, and to illustrate this contention I described a simple experiment with a piece of drainage tubing about 1½ in. long and ⅓ in. to ¼ in. in diameter of lumen. If the observer will place such a piece of tubing between his teeth, and, with his lips closely applied to it, blow through it, he will produce a sound or bruit;

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[1] Sansom, "Diseases of the Heart and Thoracic Aorta," 1892, p. 378.

[2] *Med. Chron.*, Manchester, June 1897, p. 161.

and if, whilst producing this bruit, he brings his teeth together and obliterates the lumen of the tube (Fig. 2), the character of the bruit will change from low to higher pitch, until it finally is cut off when at its stage of highest pitch, by the obliteration of the lumen of the tube. The movement of the teeth must not be too rapid, but occupy, to begin with, about 1 to 1½ second, in which case the changes in pitch are very noticeable. At the same time, by varying the rapidity by which the tube is obliterated, the character of the resulting bruit varies within limits similar to those of a natural crescendo murmur.

If the above series of changes be carried out rapidly, the murmur so produced terminates with a thud, caused by the collision of the walls of the tube; and this point I used as experimental evidence in favour of the theory, that the accentuated first sound of the heart, which terminates a crescendo murmur, is due to the impact of the walls of the thickened rigid orifice of the valve.

The intensity of the artificial sounds varies with the force used to produce them, and with the abruptness with which the teeth obliterate the lumen of the tube; and the greater the diameter of the tube, the lower will be the pitch of the note produced by blowing through it, and *vice versâ*. If in manufacturing the crescendo murmur, in the way described above, the teeth be moved together more carefully, so that, whilst the lumen of the tube is practically obliterated, its walls are not brought into violent collision, the crescendo murmur will still be heard, but without a terminal thud.

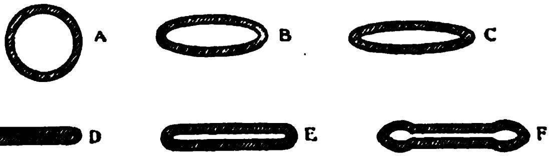

FIG. 2.—A, B, C, D, Progressive stages in the obliteration of the lumen of a tube in which is being produced an imitation crescendo murmur; E, F, condition of the lumen of the tube when a uniform murmur continues the crescendo murmur.

Finally, if the teeth be brought together so as to cut off the greater portion of the lumen of the tube, but to leave a slight area unobliterated (Fig. 2, E, F), the crescendo murmur and a modified terminal thud will still be produced, but running on from the thud will be heard a high-pitched, uniform murmur, which is caused by the escape of air through the "leak" in the tube. This last bruit is an experimental imitation of the systolic murmur which not uncommonly runs on continuously from a crescendo murmur in mitral stenosis. This simple experiment, to my mind, explains away very clearly what is to many observers a stumbling-block to the acceptance of the systolic rhythm theory of the crescendo murmur, namely, that it is impossible for even an abnormal first sound of the heart to appear in what is apparently the middle of ventricular systole, and to separate two murmurs, each of which is also produced during ventricular systole.

With the above simple experiment in mind, such an event may easily be explained, and I have shown diagrammatically in Fig. 1, C, F, the rhythm of such a compound bruit in mitral stenosis. The increased vigour which also characterises a crescendo murmur is imitated by increasing the force with which the air is blown through the tube in the production of the imitation bruit. All the above experiments with air bruits can be carried out with the lips, if they be pursed up as if for producing a whistle of low pitch, a method which is frequently adopted by teachers for giving students a good idea of what cardiac murmurs are like.

Having in this way explained the causation of the most important characteristics of the murmur, I went on to consider what must be the part of the heart which can act with sufficient force to develop the murmur, and came to the conclusion, from physiological, pathological, and clinical evidence, that it must be the left ventricle.

I will not go into any further details of the original paper, but beg to refer any one interested in the subject to it.

I have paid a good deal of attention to the same subject since June of 1897, and will now give an outline of the arguments which seem to me to throw fresh light on the subject, and to confirm my adhesion to the "heterodox" theory, that the true crescendo murmur of mitral stenosis is of early systolic rhythm, and is formed *by blood regurgitating through the stiff jaws of the stenosed valve, whilst they are abnormally slowly closing under the rising force of intraventricular pressure, generated by the earliest phase of the ventricle's contraction.*

NEW ARGUMENTS IN SUPPORT OF THE EARLY SYSTOLIC RHYTHM THEORY—EXPERIMENTAL.—1. Although there can be no doubt in my mind that the *method of production* of an imitation crescendo murmur by air is analogous to that which produces a crescendo murmur in the heart, I carried out similar experiments with water, using an old Higginson syringe with the outlet valve cut off, but with 3 or 4 in. of free outlet tube left. I found that by filling the syringe under water in a basin, and by holding the chest piece of a binaural stethoscope with the left hand to the end of the outlet tube, whilst compressing the ball of the syringe with the other hand, a bruit could be heard which was caused by the flow of water out of the orifice of the outlet tube, and that this bruit rose in pitch as the orifice of the tube was obliterated gradually by pressing it against the rim of the stethoscope (Fig. 2). Here then is an identical experiment to the drainage tube, but carried out with water as the bruit-producing medium; and, moreover, I found that all the variations of an imitation crescendo murmur which can be produced by air can equally easily be developed by water. So my theory, that the crescendo murmur of mitral stenosis is developed by blood rushing through the stenosed valve whilst it is closing, which was formed as the result of experiments with air, is confirmed by similar experiments with water.

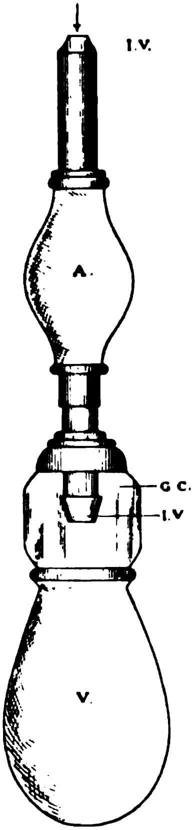

Fig. 3.—Diagram of apparatus for investigating the rhythm of crescendo murmurs.

2. *Rhythm of the murmur.* — Having found that I could imitate a crescendo murmur with water, I carried my experiment a stage further to investigate the rhythm of such a murmur.

My apparatus consisted of two syringes, coupled together by a tube, and represented the left side of the heart (Fig. 3). The auricle syringe (A) is simply a ball syringe with inlet tube and valve, and a short valveless outflow tube, which is connected with the tube running into the other syringe. The tube represents the auriculo-ventricular ring, and the second syringe (V) the ventricle. This latter syringe consists of an old type of Glover's lithotripsy evacuator, part of which is formed by a glass chamber. Into the free end or head of the glass chamber the auriculo-ventricular tube screws, so that about 1 in. projects into the chamber and an equal amount remains outside. To the outer end of the tube the auricle syringe is fixed, and on to the inner end imitation stenosed valves are placed, where their behaviour, under different conditions of water pressure, can be clearly watched.

The imitation stenosed valves are best made by shaping out of a piece of grey rubber sheet, $\frac{1}{24}$ in. thick, a slightly conical tube about $\frac{3}{4}$ in. long, and with a diameter at the apex equal to about that of a black-lead pencil. The rubber is cemented into the proper shape by means of the solution of rubber in puncture-mending outfits for bicycle tyres. Rather stiffer, and not quite so satisfactory, but still very fair imitation valves, may be made with $\frac{1}{2}$ to $\frac{3}{4}$ in. of rubber drainage tubing, about $\frac{1}{4}$ in. in diameter of lumen. Either of these valves is simply slipped on to the inner end of the metal tube, so that about $\frac{1}{4}$ in. of it projects beyond the end of the tube. The metal tube is then screwed home, and the valve is seen in position through the glass chamber, and it will be noticed at once that it tends to prevent the flow of water out of the ventricle syringe, but allows a free flow in the opposite direction. In this apparatus, then, the syringe (A) corresponds with the auricle, (V) with the ventricle, and the imitation valve with the stenosed mitral valve. There is no outlet from the ventricle syringe to compare with the aorta, but I have purposely avoided such, as without it a high pressure can be more readily generated, and the results obtained are the same as if an aorta were present.

I will not go into any more detail concerning this apparatus, but will mention the results obtained with it. As before, the experiments were carried out under water, to keep all air out of the apparatus. The object of my inquiry was, to see whether I could produce a crescendo murmur by forcing blood from the auricle syringe into the ventricle syringe, or in the opposite direction. To hear the bruits the chest-piece of the binaural stethoscope must be applied to the glass chamber under water, but I may say that the murmurs are not nearly as distinct as in the former experiment, owing to glass being a bad conductor of sound. To cut a long story short, I found that it was quite impossible to produce any bruit of varying pitch, and nothing at all resembling a crescendo murmur, whilst the water was being forced from the auricle into the ventricle syringe through the stenosed valve; but the moment the current of water through the valve was reversed, by compressing the ventricle syringe, very fair imitations of "blubbering" ascending-pitch murmurs were obtained. If the ventricle syringe be compressed gently—these ventricle experiments are best done after disconnecting the syringes—to imitate a feeble contraction of the ventricle of the heart, water will flow out through the valve, which is stiff enough to resist closure by a slight force of water pressure, and if the syringe be compressed more strongly a bruit of regurgitation ("systolic") will be heard. Further, if more force be used, the escaping water under high pressure throws the valve into "blubbering" sonorous vibrations, which can be felt by the compressing hand as well as heard with the stethoscope. The vibrations of the valve producing this sound can also be *seen* through the glass of the apparatus. They vary in character with the thickness and stiffness of the imitation valve, for, the harder and stiffer the latter, the louder and rougher is the murmur. Finally, if still more force is used, the murmur assumes a crescendo character, and is heard to rise in pitch and to terminate abruptly with a thud comparable to that at the completion of a crescendo murmur in mitral stenosis.

With the less perfect valves a smooth uniform "systolic" bruit may run on from the thud which terminates the crescendo murmur, in the same way as described with my air experiments, and is caused by the presence of a small leak in the otherwise closed valve, probably at the edge (Fig. 2, E, F). This experimental method of producing an imitation crescendo murmur also shows the probability of the abrupt first sound of mitral stenosis being caused by the impact of the walls of the closing rigid valve, for a clear note of impact can be heard when the sides of the rubber valve are seen to be forced into collision by compressing the ventricle syringe. There is no difficulty in understanding how a note of collision should be heard when the sides of the rigid, often cartilaginous, jaws of a stenosed valve meet abruptly under forcible pressure, even if the colliding surfaces should only travel ⅛ in. in approaching each other.

ORIGIN OF THE FORCE WHICH PRODUCES THE CRESCENDO MURMUR. —In my previous paper I argued that the auricle is not always, if it is ever, strong enough to contract with that vigour which is necessary for the development of the murmur. My own experience is, that the auricle is as often as not abnormally weak in mitral stenosis, and quite incapable of producing a vigorous murmur. As further support of this belief, I may quote the following figures, obtained from the post-mortem room, which show the condition of the wall of the left auricle in cases of mitral stenosis.

Sansom[1] found the auricular wall hypertrophied in thirteen cases and dilated in eighteen cases of mitral stenosis, whilst Samways[2] found hypertrophy recorded, in the *Guy's Hosp. Rep.*, London, in sixty-five, and dilatation in sixty-eight, of a total of 173 similar cases. Sansom[3] says that "the muscular fibres of the wall of the auricle may be attenuated to almost extinction"; and Balfour[4] writes: "It does seem remarkable and even mysterious that the comparatively feeble and brief contraction of the auricle should be associated with a murmur so loud and rough as the presystolic murmur," nevertheless the facts in favour of such being the case seem "incontrovertible." Any theory which explains the production of a crescendo murmur must hold good in whatever condition the left auricle may be, and it must be remembered that this murmur not uncommonly occurs in cases in which the auricle is dilated and abnormally weak. On the other hand, there is no question as to the ability of the left ventricle to produce a vigorous murmur like the crescendo murmur of mitral stenosis.

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[1] "Diseases of the Heart and Thoracic Aorta," 1892, p. 353.
[2] *Brit. Med. Journ.*, London, 1898, vol. i. p. 365.
[3] *Ibid.*, 1898, vol. i. p. 1644.
[4] "Clinical Lectures on Diseases of the Heart," 1898, p. 118.

Duration of a Crescendo Murmur.—The time occupied in the production of these murmurs, as explained by my theory, is only a small fraction of a second. Thus, in a heart beating at the rate of seventy-five per minute, each beat occupies ·8 of a second of time.[1] Of this time ventricular systole takes up ·3 and ventricular diastole ·5. Auricular systole occupies ·1 of the time taken up by ventricular diastole. Less than half the duration of ventricular systole fully covers the period which I believe is occupied by the crescendo murmur, *i.e.* ⅕ or less than ⅛ of a second (Fig. 1, A, B, C, D). But though of very short duration, crescendo murmurs may vary in length, some being more prolonged, whilst others are very short and abrupt.

In its more marked appearances a crescendo murmur is of a drawn-out emphatic character, and may be imitated by producing the murmur in a leisurely way with the lips, as described in a former paragraph. On the other hand, it is often so short and abrupt, that careful auscultation is necessary before a trained ear is certain of its presence. One feels that there is "something" which gives an impression of impurity to and terminates with the abrupt first sound. This type of the murmur can be imitated by making a crescendo sound as rapidly as possible with the lips. Between these two extreme types are crescendo murmurs of different lengths, which vary with certain conditions under which the murmur is produced. These conditions may be arranged somewhat roughly in the following groups:—

1. *Rigidity of the valve.*—The more rigid and stiff the valve, the longer will closure be delayed, and therefore the longer will a crescendo bruit produced at the valve be. The extreme of this condition is a rigidity which cannot be overcome, with the consequent development of an ordinary systolic murmur of regurgitation.

2. *Rapidity of development of intraventricular blood pressure.*—The earlier the stage of ventricular systole at which an intraventricular blood pressure sufficiently high to close the rigid valve is developed, the shorter will be the crescendo murmur, and *vice versâ.* The development of high intraventricular blood pressure depends on the degree of the pressure within the aorta, and on the force of the heart's contraction. With a high intra-aortic pressure, or with a strong heart beat, the crescendo murmur runs a short, rapid, and vigorous course, but with the opposite to these conditions prevailing the crescendo murmur will be prolonged and less forcible. If with a very low intra-aortic pressure and feeble heart-beat the intracardiac pressure remains too low to close the valve, then the crescendo murmur will be replaced by a uniform true systolic bruit. The latter bruit may often be heard whilst the patient is lying on his back in bed, and yet be immediately replaced by a crescendo bruit when the intra-aortic

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[1] Allbutt's "System of Medicine," vol. v. p. 470.

pressure has been raised, and the force of the heart-beat strengthened by the patient sitting up.

3. *The rapidity or slowness of the heart's action—i.e.* the length of the ventricular systole—also has some effect in determining the length of the crescendo murmur. The quicker the contraction of the ventricle, the shorter will be the crescendo murmur under otherwise similar conditions, and *vice versâ.* The transition from the more prolonged to the rapid type of crescendo murmur is at times observed in the same patient. The more prolonged bruit may be heard whilst the patient is lying at rest and the heart beating quietly, but when any increased exertion—as for instance the change to the erect posture—excites more forcible contraction of the heart, the crescendo murmur is heard to quicken in a very marked manner, and it may even vanish completely, the terminal accentuated first sound alone remaining.

In some most favourable cases the change from a systolic murmur and no first sound to a deliberate, and then rapid crescendo murmur, with terminal accentuated sound, and finally to simply an accentuated first heart sound and no murmur, may be observed in the same patient during rest in bed and subsequent increased action of the heart induced by the erect posture or by walking about. Such a series of events is to my mind a further confirmation of the theory, that an ordinary systolic murmur of mitral regurgitation is but an imperfectly developed crescendo murmur, and points most forcibly to the conclusion that the latter bruit is caused by early vigorous ventricular systole and not by auricular contraction.

NATURE OF VENTRICULAR CONTRACTION.—The nature of ventricular systole favours the theory that the stenosed valve is closed in this phase of the cardiac cycle. Contraction of the whole ventricle wall is not simultaneous, but is in all probability of a peristaltic nature. Teaching, to my mind, is not definite in saying whether the wave begins at the base and extends to the apex, or *vice versâ.* For my own part I think that the contraction ought to begin at the apex, for this is the thinnest and weakest part of the ventricle, being often not more than ¼ in. thick (Quain); whereas the rest of the ventricle wall is two or three times as thick. Thus the contraction beginning at the apex will protect the weakest part of the ventricle and prepare it for the strain which it will be subjected to by the completion of systole. It is by this apical contraction, or at any rate by the earliest portion of the peristaltic wave, in health, I believe, that the already apposed cusps of the mitral valve are competently locked, and the regurgitation of blood into the auricle during the rest of ventricular systole prevented. Foster[1] recognises a phase of ventricular systole, in which pressure is being got up before the semilunar valves are opened. The time so occupied is ·03 of a second in health. In mitral stenosis, however, the peristaltic contraction is, I believe, too feeble at its onset to *close* the stiff immobile jaws of the valve, but sufficiently strong to force blood to regurgitate through them into the auricle, and to give rise to the commencement of a crescendo murmur. The more forcible contraction of the rest of the ventricular wall, following rapidly and with increasing vigour, develops sufficient intraventricular blood pressure to close the valve, thus finishing off the crescendo murmur, which is terminated abruptly by the snap of the closing valve.

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[1] "Text-Book of Physiology," 6th edition, vol. i. p. 264.

By this explanation we account for the development of both the ascending pitch and crescendo characteristics of the murmur which we are considering.

DURATION OF VENTRICULAR SYSTOLE IN MITRAL STENOSIS.—In health, as stated before, ventricular systole occupies three-eighths of the whole time of the cardiac cycle, the rest being attributed to ventricular diastole; and in a heart working at the rate of 75 beats per minute, each beat occupies ·8 of a second of time. If the pulse rate increase in health, the additional beats are obtained without appreciable shortening of the cardiac systole, and almost entirely by reduction of the diastole of the heart.[1]

The advocates of the presystolic theory state that the phase of ventricular diastole is abnormally prolonged in mitral stenosis, to the extent, I take it, of the duration of the crescendo murmur, for the early and late diastolic murmurs when present occupy fully the time attributed to the normal phase of ventricular diastole. Now the crescendo murmur lasts for at least ·1 of a second (see Fig. 1); and if this period of time be added to that already occupied by ventricular diastole (·5), we have this latter phase of the cardiac cycle taking up ·6 and ventricular systole ·2 of the total duration (·8) of the whole cycle. This means that the phase of the ventricle's diastole is three times as long as that of its systole. As ventricular systole is the most stable part of the cardiac revolution (Gibson[2]), such a division of the cardiac cycle is very improbable. It is much more likely that the time occupied by the different phases of the heart-beat in mitral stenosis is as indicated in Fig. 1, B, etc., *i.e.*, practically as in the normal state of affairs. Gibson,[3] in his criticism of my view of the origin and rhythm of the crescendo murmur, groups me with other "early systolic" theorists, as believing that the ventricular systole, when it produces a crescendo murmur, begins earlier than is normally the case. But he has misunderstood my view of the case, whatever other writers may think, for I do not consider that there is any alteration in the time of onset, or even in the duration of the ventricular systole, in these cases. There is plenty of time in the three-eighths portion of the whole cardiac cycle occupied by systole of the ventricle, for the development of a crescendo murmur of ascending pitch, whether it occurs alone or is followed by an ordinary systolic murmur of mitral regurgitation.

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[1] Sherrington, *loc. cit.*

[2] "Diseases of the Heart and Aorta," 1898, p. 55.

[3] *Ibid.*, p. 156.

The possibility of the existence of a transient systolic murmur, terminating with the first sound and due to regurgitation through an incompetent mitral valve during ventricular systole, is acknowledged by Balfour.[1] In the chapter on curable mitral regurgitation the following passages occur:—"The result of even a slight dilatation (from muscle failure) is the establishment of so-called relative insufficiency, not because the auriculo-ventricular opening itself is dilated—that does happen, but at a later period of the affection—nor because the segments of the valve are unable to close the opening, as one alone of these segments is almost sufficient for this purpose; but because, owing to the separation of the cardiac walls by dilatation of the (ventricular) cavity, the insertions of the *chordæ tendineæ* into the papillary muscles are set so wide apart, and so far from the centre of the ventricle, that the trifling pressure of the auricular blood is unable to bring the valve segments into apposition just before the commencement of the ventricular systole. But any interference with the instantaneous closure of the valve, at the moment of ventricular systole, favours regurgitation, and in these cases this is a constant phenomenon, varying in degree at different times. In all these cases there is a more or less impure first sound audible in the mitral area, or a transient systolic whiff may terminate in an apparently normal first sound, and in still other cases a murmur is heard throughout the whole of the systole from its commencement to its end. This is quite in accordance with the account just given. In slight dilatation, at the commencement of the ventricular systole, the valve segments are not in apposition, as they ought to be, and there is some regurgitation, but this ceases as the systole progresses and the valve segments are perfectly closed. When the dilatation is greater, the valve segments never come together, and the regurgitation persists throughout the whole of the systole."

Such opinions seem to me to be strong evidence in favour of the early systolic rhythm of the crescendo murmur. For if in such cases of mitral insufficiency, with mobile valve curtains, as Balfour describes, the development of a transient systolic whiff, terminating in a first sound, is possible, why should it not be equally easy for a similar transient systolic whiff to be developed at a rigid, stiff, narrowed mitral orifice, which most certainly requires an abnormally great force of intraventricular pressure to close it? The transient murmur described by Balfour must in my opinion be of rising pitch, from the fact that it is formed at a gradually diminishing area of leakage in a valve. It is thus an imperfectly developed crescendo murmur.

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[1] *Op. cit.*, p. 168.

(*To be continued.*)

VAGINAL HYSTERECTOMY FOR CANCER.[1]

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The question of vaginal hysterectomy for various conditions has been already so fully and exhaustively dealt with in the Society, in papers by Professor Simpson and Dr. Barbour, that I do not think I need enter into any discussion as to the history of the operation, or how its technique has been evolved.

In any account of vaginal hysterectomy, with our improved methods of operation, recovery is, as a rule, to be expected. The immediate risks of the operation, when undertaken in circumstances at all favourable, are small. Those cases which succumb shortly after the operation must and can only be badly selected ones, where the fornices are small, where the disease has involved the posterior wall of the bladder (a condition always difficult of diagnosis beforehand), or where hæmorrhage or sepsis has resulted. But, in any well-selected group of cases, such an untoward event is never to be looked for.

In the list of cases which I now publish, all the patients immediately recovered except one, who died from sepsis some days after the operation, due to my having failed to realise that the uterus was closely attached to the bladder, the whole tissue being soft and friable. The operation was thus, to a certain extent, incomplete. So, therefore, I may dismiss now any question about vaginal hysterectomy, so far as my present paper is concerned, except the question of the remote results.

Neither Professor Simpson's nor Dr. Barbour's paper touches upon this. They deal only with the history and technique of the operation, and the records of immediate results, which are entirely satisfactory; and to Professor Simpson we are especially indebted for bringing the subject directly and definitely before us.

I am at present concerned with the question, as to how far this operation prolongs the life of the patient; and, before touching upon that matter, another question readily suggests itself. I suppose no one will deny that a case, to be not only immediately but also to be remotely successful, must necessarily be attacked in its most early stage. Now, a very early carcinoma of the cervix, still more an early tubular carcinoma of the body of the uterus, will, under ordinary circumstances, give an expectation of life for the patient, of eighteen months to three years. With sarcoma the expectation is distinctly longer. Therefore, in ascertaining the value of any operation whatever, the first question to decide is, Whether or not that operation can prolong the patient's expectation of life? and, obviously, the second question is, Whether it prolongs her expectation of life or not, does it relieve her symptoms, and give her an easier death than she would have had if no operation had been undertaken? These are the two questions which, in this paper, I propose to discuss.

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[1] A paper read before the Edinburgh Obstetrical Society, 8th February 1899.

During the past ten years, 300 cases of cancer have presented themselves to me, either in the wards of the hospital or in my consulting-room, and with regard to these I have the following remarks to make:—

1. They include not all the cases of cancer which I have seen, but only those which have been admitted to my ward, or which I have seen in my own consulting-room. I have not included those cases seen in the out-patient department, and of those which I have seen in consultation in private I have kept no record. Hence, therefore, the experience which I am putting before you may seem rather less than might have been expected.

2. The accompanying table includes all the cases of cancer of the uterus for which I have performed vaginal hysterectomy, except two, and those two were done in my earlier years, and were, in some respects, incomplete. On that account they must be omitted in estimating the value of the operation. The 14 now recorded were performed with the Greig Smith and Doyen improvements of the operation; and, save one, they were complete in every detail. Of the 14, only 3 were done in the hospital, the others in private, which is itself a suggestive fact, because those seen in private practice are always seen at an earlier stage than those which apply for advice to a hospital. Each one of those 14 cases was most carefully selected; all the others, if interfered with at all, were only dealt with by the ordinary palliative measures.

3. These cases include all the recognised forms of cancer affecting the cervix and the body of the uterus.

It would be a work of supererogation to describe each case of those operated upon in detail; nor have I any intention of referring to those cases in which merely palliative operative interferences were employed, such as amputation of the cervix, and the employment of the cautery, etc.; but I refer only to those in which I performed vaginal hysterectomy.

First let me take a hospital case:—

Case 1.—A widow, mother of four children, a worker in a Dundee mill, presented herself at the hospital, complaining of slight pain in the back, and menorrhagia. Now there was no leucorrhœa, no fœtid discharge, no bleeding, and no emaciation, and a certain slight degree of erosion and eversion of the cervix was all the sign of disease that could be found. At first it was supposed to be a case of cervical catarrh, and was put under treatment appropriate to such a condition. As there was no improvement after a fortnight, and as the cervix seemed to become suspiciously hard, a piece of tissue was examined by competent authorities at the laboratory of the Royal College of Physicians, and the condition pronounced to be malignant. As the case fulfilled every one of the con-

No.	Name.	Age.	Married or Single.	Complaint.	Duration.	Family.	Physical Examination.	Operation.	Remarks.
1	H. S. .	47	...	Pain in abdomen and back; profuse hæmorrhage; weakness.	10 months.	...	Primary carcinoma uteri.	Hysterectomy.	Immediate recovery; died 9 months later from recurrence.
2	K. O. .	36	S.	Pain in stomach and bearing down.	2½ years.	...	Early epithelioma of cervix.	,,	Immediate recovery; died 18 months later from recurrence.
3	H. S. .	48	S.	Bearing-down pain; constant desire to micturate; brownish, foul, watery discharge.	6 months.	One, 22 years ago.	Carcinoma cervicis.	,,	Death.
4	M. B. .	49	M.	Profuse hæmorrhagic discharge with severe pain.	21 months.	Eight, last 4 years ago.	Early carcinoma cervicis.	,,	Recovery, but died 14 months later.
5	I. M.. .	51	M.	Flooding.	2 months.	...	Early carcinoma of the cervix.	,,	Recovery, but died 16 months later.
6	Mrs. L. .	32	M.	Foul discharge.	2 years.	Five, last 4 years ago.	Early epithelioma cervicis.	,,	Recovery, but died 1 year later.
7	Mrs. S. .	44	M.	Hæmorrhagic discharge, constant.	4 months.	Thirteen, last 1 year ago.	Early epithelioma of cervix.	,,	Recovery, but died 8 months later.
8	M. F. .	38	M.	Pain; foul discharge.	12 months.	Ten, last 2 years ago.	Early cervical cancer.	,,	Recovery, but died 17 months later.
9	M. A. H.	43	M.	Constant offensive discharge, with pain in left iliac region.	6 months.	Eight, last 5 years ago.	Cervix thickened and indurated. No deposit in fornices; carcinoma cervicis.	,,	Recovery, but died 1 year later.
10	I. S. . .	58	S.	Periodic pain in lower part of abdomen; hæmorrhagic discharge.	4 months.	None, unmarried.	Cervix smooth; uterus enlarged, but movable.	,,	Recovery, but died 11 months later.
11	J. S. . .	38	M.	Menorrhagia and metrorrhagia.	5 months.	Three, last 13 years ago.	Thickening and erosion of cervix; early epithelioma cervicis.	,,	Recovery, but died in one year.
12	J. H. .	29	M.	Hæmorrhagic discharge.	6 weeks.	Five, last 16 years ago; 1 abortion.	Cervix hard and indurated; uterus movable; carcinoma cervicis.	,,	Recovery, but died 10 months later.
13	J. W. .	49	M.	Constant hæmorrhagic discharge; pain.	11 weeks.	Nine, last 12 years ago; 3 miscarriages.	Carcinoma cervicis (early).	,,	Recovery, but died 16 months later.
14	E. W. .	33	M.	Pain in side; frequent flooding.	8 months.	Three, youngest 8 years.	Carcinoma cervicis (early).	,,	Recovery, but died 8 months later.

ditions for hysterectomy—mobility, non-involvement of glands, and freedom of the ligaments, with apparently no infection of the body of the uterus or the bladder—the operation was performed, lasting eighteen minutes, and the patient made a perfect recovery. The further history of that case is as follows:—

The operation was done on 16th July. The following Christmas the patient was seen by Dr. Lackie, who said that there were several bleeding nodules at the site of the cicatrix, and that the patient was suffering pain. During my absence in April, Dr. Lackie again saw her, and pronounced the whole pelvis to be occupied by a cancerous mass, with hæmorrhage, emaciation, and great pain. She died during that summer.

Now, my comment upon this case is—(1) That I could not wish for a more suitable case, both as to the general and local conditions, to operate upon, and that the operation could not be easier or shorter; (2) that for a short period of a month or two the patient was comparatively well; and (3) that in six months the disease had recurred with intense pain, in nine months there was a fungating cancerous mass in the pelvis, in a year the patient was dead.

Now, I am bound to say that, so far as my experience of such a case of cancer is concerned, had the patient been left alone her expectancy of life would have been from eighteen months to two years, and her sufferings, which were very great, would have been less.

With regard to Case 2, it is that of a lady from the west country, æt. 47 years, and in evidently robust health. Her symptoms were chiefly severe pain in the back and loins, with some hæmorrhagic discharge. Her doctor, Dr. Muir, who sent her to me, believed she was suffering from an early cancer of the uterus. When I saw her the uterus was perfectly mobile, not enlarged, the fornices were free and no glands were involved, and, save a distinct hard nodular cervix, there was nothing abnormal to be found. I was disposed to agree with Dr. Muir as to the malignancy of the case, but had a specimen examined, and the report confirmed this opinion. The operation was performed quickly and easily, after Doyen's method, and the patient made an absolutely uninterrupted recovery, leaving the Nursing Home a month from the day of operation. Three months afterwards the pain recurred, and soft, granular, bleeding masses appeared at the site of the wound. From that time onwards the pain was so severe, that the patient practically lived on opium, and died seven months after operation.

My comment on this case is, that this woman had only a very short immunity from pain. In three months it recurred, and except for morphia her life would have been intolerable. The disease had markedly affected the peritoneum, although the mass in the pelvis never became very large. This woman's expectation of life when I first saw her was unquestionably from a year to eighteen months, and as a matter of fact, with all the so-called advantages of the operation, she only lived seven months.

The third type of cases is illustrated by the following:—

Case 3.—When I first saw her she was supposed to be suffering from an incomplete abortion. She was a lady in the enjoyment of excellent health, except for persistent hæmorrhage, which had lasted for two months before I saw her. I was quite contented to believe that the diagnosis given me by her medical attendant was correct, and I proceeded to curette the uterus, which was enlarged, with the cervix somewhat gaping, and well-marked ectropion. I submitted the débris removed by curettage to Mr. Stiles for microscopic examination, and to my surprise he told me the case was one of tubular carcinoma. Even from such an authority as Mr. Stiles I was unwilling to accept this, and it was only after an interval of a fortnight that I removed another scraping for examination by Mr. Stiles, but his report was just the same as before. I therefore, without further delay, performed vaginal hysterectomy. If ever there was a case in which the operation was easy, this was the one; and where hopes of remote recovery were warranted, there never was a better, because the broad ligaments were absolutely free, and the disease was entirely localised to the fundus and body. Now, within six weeks she had slight recurrence of the hæmorrhage from the cicatrix, and an enlarged gland in the left groin. She went to her home in the north, and although I never personally saw her again, she died of malignant peritonitis within the year.

My comment on this case is obvious. A woman's expectation of life with intra-uterine cancer is from two to five years. Howard Kelly, himself a keen operator, admits five years as a possible duration for an intra-uterine cancer, when not interfered with. This patient was in excellent health, she suffered absolutely no pain, and had no symptom except the hæmorrhage; and had it not been for the practice that I invariably make of having the scrapings after a curettage examined by the microscope, this patient would have gone home without any reason to suppose she was suffering from cancer of any sort or kind. It will be further observed that she had no pain before the operation, and it was only afterwards that, when the disease spread to the peritoneum, her pain was intense, and she died in terrible suffering from malignant peritonitis.

I need not multiply examples, because in all the other cases the history before and after the operation is somewhat similar. But perhaps I may be allowed to show the converse. A lady came under my observation with what I supposed to be senile uterine catarrh. I do not usually dilate and scrape in this condition, but as the os uteri was somewhat gaping, and the hæmorrhage rather profuse, I further dilated the cervix, and curetted and packed the cavity. The report on the scraping was "well-marked carcinoma." I announced this to the patient's friends; and for various reasons, personal, domestic, and otherwise, any further operative interference was declined. Personally I was not sorry for this, because the uterus was somewhat fixed and the fornices small, and the operation would not have been a very easy one. That was in 1894; to-day, though seriously ill, she is still alive, and only recently has she suffered much at all. During these years she has at intervals enjoyed many consecutive weeks of excellent health. Her position in life allowed her to have every luxury, which may possibly have had something to do with the prolongation of her life; but nevertheless the fact remains, that four years ago she had fundal cancer, verified by the microscope, and she is alive still.

My further comment on this case is, that that duration of four and a half years is probably an under-estimate of the time, because it is a certainty that the cancer had originated a considerable time before I first saw her. I must admit that this is a longer duration than is commonly met with, but a three years' duration is by no means uncommon in my experience.

I have already intruded myself on the Society too long, having gone into the question some months ago, but I should like to say that in the present paper I am not dealing with anybody's results but my own, nor am I venturing to criticise any other operator's work. I am only stating my own experience of vaginal hysterectomy for cancer. This has been an unfortunate one; but it seems to me the duty of every operator, whatever his ability or experience may be, to record his ultimate results, even though the truth so told may tell against himself. Let me add that all these cases operated on were exceptionally favourable ones, and selected with special care. Bad though my results have been, I tremble to think of what they might have been had I operated on cases indiscriminately. I am hopeful, however, that by some improvement in the management of the wound at the time of operation, so that the diseased surface may not come in contact with the peritoneum, infection may become less likely, and better remote results be secured.

I shall continue to operate upon early selected cases until I succeed, or until some method of dealing with cancer, such as by the serums or the antitoxins, will render operative treatment unnecessary.

THE INEBRIATES ACT OF 1898.

By G. R. Wilson, M.D., *Medical Superintendent, Mavisbank Asylum.*

There is a sense in which nearly every Act of Parliament is of the first importance. Nothing can become law without contributing something material to the spirit of government, and each Act determines to some extent how the idea of the rights and liberties of the subject must henceforth be construed. But there are some Acts whose effect is both more obvious and more immediate; and the Inebriates Act, 1898, is one of them. It is an Act which materially impairs the fulness and freedom of the Englishman's right to be drunken.

For many years the legal and the political professions have combated the demand of the medical faculty that, in the interest both of the drunken individual and of the State, some interference with habitual drunkards was called for. The objection generally opposed to our petition has been, that to take a drunkard in charge, merely because he was a drunkard, would be a serious encroachment upon the liberty of the subject. Each man, we were told, had a right to be drunken if he chose, and the law would as soon think of dictating to the individual what and how much he should eat as try to compel him to be sober. No amount of drunkenness has hitherto entitled the State to interfere with a subject, unless he has in addition broken some law. Even then—supposing a man, for example, to have committed a criminal assault when drunk—the attitude of the Court to the drunkenness was traditional and arbitrary. The law might or might not regard drunkenness as mitigating the crime. Until the 1st of January 1899, mere drunkenness may be said to have been without the jurisdiction of the Courts.

By the new Act a new era is begun. For, although it refers only to drunkards who have come into Court because of crime, or who have, four times within a twelvemonth, been convicted of the minor drunken offences, the principle for which our profession has so long contended has at last been conceded. We do not propose to consider the Act in detail. It is an Act which every medical man ought to read for himself, because it is an Act which refers to the drunkards as "patients," and practitioners may at any time be called upon to appear in the Courts in connection with it. But there is a concession in the first section of the Act, which may be cited as bearing out the fact that the law now takes cognisance of drunkenness as such, apart from crime, and we do well to consider what this implies.

The first section of the Act refers to persons who are convicted of "an offence punishable with imprisonment or penal servitude, if the Court is satisfied from the evidence that the offence was committed under the influence of drink, or that drunkenness was a contributing cause of the offence." Having found the prisoner guilty of the crime, the Court may then decide whether or not he is a habitual drunkard. If he is found to be so, the Court may then, in substitution for, or *in addition to*, any other sentence, order that he be detained for a term not exceeding three years in an inebriate reformatory. That is, the drunkenness of a prisoner may be dealt with in addition to his crime. The law says that it will take cognisance of drunkenness only in those drunkards who are found guilty of crime—a somewhat fortuitous selection. But, once the crime is proved, the habit may be dealt with under compulsory powers, in addition to whatever punishment may be inflicted for the crime.

This concession to an old demand is very gratifying, and we have no complaint to make because the application of the law is restricted to the unfortunate few who, being drunken, are not shrewd enough to refrain from a transgression of the law. But the Act brings responsibilities with it which especially fall upon our faculty. We have for years asked for this reform, and, now that we have obtained a beginning of it, it is upon us that the duty falls of doing what we can to make the law of good effect. Every practitioner knows how greatly better it would be for every one concerned, if habitual and intractable drunkards could be compelled to remain under treatment. That power will surely come, if the success of the method can be proved in the case of those few who come within the meaning of this new Act. How that success is to be achieved we cannot say. Medical advisers have been appointed to assist in drawing up regulations for the conduct of the inebriate reformatories in England, and doubtless the Secretary for Scotland will see to it that similar help will be given in Scotland. But when the regulations are approved—having lain for four weeks on the table of the Houses of Parliament—the duty of the profession will only have begun. An important question, which we shall partly help to settle, will be as to what method of treatment the "patients" should have. We must see to it at least that they are not neglected; for it is quite certain that mere detention for a period, however prolonged, is as likely to weaken the nature of the drunkard as to strengthen it. But a more obvious good may be done if we create a right opinion as to what kind of drunkards are properly to be detained under the Act. That is the question which is at present vexing all who are interested in the subject—lawyers, physicians, and those of the public who are earnest in attempts to reform. As the Act is worded, the selection of prisoners to be dealt with under the Act is left entirely to the discretion of the Court.

In the second section of the Act, which refers to those who are four times within the year convicted of the ordinary drunken offences, we are told that the person convicted "shall be liable . . . to be detained for a period not exceeding three years." But the history of Acts relating to the liquor trade prepares us to expect that this detention to which the drunkard has made himself liable may not be enforced. Similarly, criminals who are referred to in the first section "may" or may not be indicted as drunkards. So far as we can judge, the selection of cases is left to the prosecutor and to the bench. Who will advise them no one seems to know. In all probability these cases will be first chosen which have for long been a burden upon the State, because of vexatious repetition of offences. That class of case is not one which physicians would naturally select as most likely to benefit by the Act. Society may greatly benefit by their segregation; but that is not our first concern. Every practitioner knows the cases in his practice which he has tried in vain to cure by ordinary means, and in which the habit has become inveterate.

There are others more hopeful, younger in the vice, who are only becoming grave, and who may drift into the Court so as to come under the Act. It will be a loss if such cases as these do not have the benefit of the new law. It is not our function to play the part of a police, but it is part of our public duty to influence society in the direction of stimulating the authorities to enforce the law. This new Act is one which would go a long way towards making the country sober, at least in public, if the law which relates to public drunkenness were strictly enforced, and if it were the habit of the Court, in every case of crime, to inquire whether drunkenness contributed to it and whether the prisoner is a habitual drunkard; and, if it finds him so, to "detain" him for special treatment.

CLINICAL CONSIDERATIONS IN CHOREA.

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Chorea is a common-place disease. So many excellent descriptions of it are to be found in medical text-books, that it may seem well nigh impossible to invest with interest so threadbare a subject. The following paper, therefore, is merely an embodiment of one's personal experience in a recent series of cases of that disease. I have looked through my notes of the cases of chorea which came under my immediate observation during my term of office as house physician to the National Hospital for the Paralysed and Epileptic, and I am indebted to the members of the Queen Square staff for their courteous permission to make use of the facts recorded in my clinical note-book. My cases, forty-one in number, are not a selected series, but were recorded indiscriminately as they came under observation. Yet their number is sufficient, I think, to justify one in emphasising certain points in the symptomatology of the disease which have hitherto, perhaps, been inadequately recognised.

Chorea has a clinical rather than a pathological existence. Very little is known with regard to its essential pathology, although various appearances have been described in autopsies on fatal cases. Amongst other conditions may be mentioned hyperæmia of the brain, occlusion of arterioles by thrombosis or by embolism, and minute perivascular hæmorrhages in the brain and spinal cord. But an insurmountable objection to the acceptance of any of the above as the essential cause of the disease, lies in the fact that any or all of these changes may be absent, and the brain cord and nervous system may appear absolutely normal — so far, at least, as the present methods of neuropathology enable one to judge. The nature of the choreic movements points strongly to their origin from some irritable condition of the motor cells of the cortex, and, as we shall see, the frequency with which the symptoms are confined to one side of the body still further corroborates such a hypothesis. We seem driven to regard the disease as a functional rather than a structural disorder of the nerve-cells, although there can be little doubt that, underlying all so-called "functional" disorders, there must be some molecular and chemical changes in the nerve elements. But the precise nature of such changes is a problem as yet unsolved, and when it is solved it may be by the physiological chemist rather than by the histologist. That the disease may be due to a toxin appears not at all improbable.

Clinically, a history of rheumatism or of scarlet fever, or of a family diathesis towards rheumatism, is very common, and should be inquired for in every case of chorea. Out of our forty-one choreic patients, in twelve there was a previous history of rheumatic fever in the patient; one patient developed pain and swelling of both wrist-joints whilst under observation, within a week after the outset of the chorea; and another case, a primigravida, developed pain and swelling in one knee-joint a fortnight after the commencement of the attack of chorea. Of the remaining twenty-seven cases, fifteen had a strong family history of rheumatic fever, whilst in the remaining twelve there was no history of rheumatic fever, either in the patient or in the patient's near relations. Yet out of these twelve no fewer than five had mitral regurgitation, one had mitral stenosis, which ultimately proved fatal, and only six had no valvular affection of the heart.

Another fairly common antecedent is fright, or shock to the nervous system. Out of forty-one cases, fifteen gave a history of some fright or mental shock. But of these fifteen, three of the patients had previously suffered from rheumatic fever, and six cases had signs of mitral disease. One little patient was said to have acquired the complaint by imitating another child afflicted with chorea, but in this case the patient developed joint-swellings during the attack. Two other cases had a very strong family history of rheumatic fever. Only three of the cases attributed to some nervous shock were free from evidences of rheumatism, either in themselves or their near relatives, but one of these was an instance of "hereditary" chorea, and another was a case which had presented all the features of choreic movements without intermission for eight years. The single remaining case followed a mental shock in a patient who had no previous rheumatism nor cardiac disease, nor had any near relative suffered from rheumatism.

The presence of worms in the intestinal canal has been believed by some to cause the disease in certain cases by reflex irritation, but in our series of cases the only case of chorea in a child with tapeworm occurred in a patient who had previously had an attack of rheumatic fever. Two patients were pregnant—primigravidæ; one of them, however, had had repeated attacks of chorea before, attended by joint-swelling during her third attack, whilst the other had previously suffered twice from rheumatic fever. The probability, therefore, is that, in most cases, shocks and irritations to the nervous system only precipitate an attack of chorea in a constitution which is already rheumatic.

Chorea is essentially a disease of childhood, being most common between the ages of 5 and 15 years. In our forty-one cases, no fewer than thirty-two came on between the fourth and fifteenth year (this, of course, refers only to first attacks of chorea). After 15 years of age chorea is much less common as a primary attack—six of our cases occurring between the ages of 15 and 24, but second and third attacks are by no means uncommon after the age of 15. In our series of cases the earliest one occurred in a child æt. 4 years, whilst the latest age of a first attack of ordinary chorea was 24 years. Only two of our cases came on for the first time at a later period—at the ages of 38 and 47 years respectively, both of these being instances of hereditary, or "Huntington's" chorea. One old lady, it is true, had a second attack at the age of 60, but she had suffered from chorea fifty-five years previously, at the age of 5 years.

The female sex is much more liable to this disease than the male. Out of our forty-one cases, thirty-four occurred in females and only seven in males, the proportion being thus almost five females to one male. This is a little more than the average proportion, which is commonly stated at about three females to one male.

The onset of the disease is more or less gradual, usually taking a day or two to develop, before anything abnormal is noticed about the child. The patient is often fretful and more irritable in temper than usual. Then restless wriggling movements are observed in the face or limbs. These symptoms gradually increase in severity, and in fully developed cases they may be classified into various groups, namely—(*a*) Irregular involuntary movements; (*b*) inco-ordination on attempted voluntary movement; (*c*) weakness of the affected limbs.

The irregular involuntary movements are quite *sui generis.* They are of a twisting, wriggling, tossing type. The face, trunk, and limbs—indeed, all the voluntary muscles of the body—may be affected, but the choreic movements in the majority of cases affect one side of the body earlier and more severely than the other, and may often remain entirely confined to one side. In a few cases they migrate completely from one side to the other. In our experience the left side is far more commonly affected than the right. Out of the forty-one cases referred to, no fewer than twenty were either wholly left-sided, or affected the left side distinctly more than the right. In ten the movements were right-sided, either wholly or preponderatingly; but, of these ten, two cases were in left-handed patients (which still further strengthens one's opinion as to the greater frequency of left-sided chorea in ordinary, or right-handed, patients), and a third case occurred in a patient who had received a severe electric shock in his right hand from a dynamo, five days before the choreic movements began in his right hand. Eleven cases were bilateral, affecting both sides apparently with equal severity.

It is interesting to observe the fact that in ordinary right-handed patients the choreic movements more commonly begin on the left side. A similar preponderant left-sided distribution of "functional" paralyses, motor or sensory, in right-handed people (and, conversely, of right-sided "functional" paralyses in left-handed people) has also been observed in other "functional" diseases and in paralysis agitans.

The commonest starting-point for the choreic movements is in the hand. Once begun, the movements are quite characteristic. They may vary in degree, from the slightest restlessness of the fingers to the wildest and most violent irregular movements of the whole limb. The affected joints are impetuously flexed, extended, rotated, or circumducted. The eyes roll about restlessly, and the patient makes peculiar foolish grimaces, sometimes shutting his eyes or showing his upper teeth. The jaws may open and shut irregularly, the tongue may be rolled in various directions, and not infrequently it is bitten by a sudden involuntary snap of the teeth. In several cases I have observed the soft palate moving irregularly and involuntarily up and down. Respiration is often jerky and irregular; the action of the diaphragm is frequently overshadowed by that of the intercostals and extraordinary muscles of respiration. Sometimes the intercostals and diaphragm contract alternately instead of synchronously.

In severe cases the limbs and trunk may be thrown about so violently that the patient falls out of bed or may bruise or cut the bony prominences of his arms, legs, or head, by knocking them against surrounding objects.

Voluntary movements are always more or less impaired when involuntary choreic movements are going on. The irregular spontaneous movements of chorea are very little under control of the will. But sometimes, in mild cases, no spontaneous movements can be observed, the chorea only becoming evident on voluntary exertion. A good test for slight cases of chorea is to make the patient hold up both hands for a few seconds above his head, with the fingers outstretched. This usually succeeds in bringing out a few irregular wriggling movements of the fingers on the affected side. Or make the patient squeeze one's hand, and one perceives the choreic grasp to be sudden, spasmodic, jerky, and ill-sustained. If the grasps be compared on the two sides in a case of hemichorea, it will be found that the grasp on the choreic side, although more sudden, is yet actually weaker than that on the unaffected side. Also, when picking up objects, the patient makes a sudden dash for the object, and often drops it after securing it. Sometimes, again, the patient has a difficulty in letting go an object when he wishes to do so. Thus, in feeding himself, he often drops his cup, and scatters his food about the table with his knife and fork.

In the lower limbs, besides involuntary wriggling movements, similar to those observed in the arms, the gait may be peculiarly altered. The knees may suddenly give way, during walking, causing the patient to fall.

Articulation is often jerky and indistinct. In bad cases the speech may be quite unintelligible, or the patient may speak only in whispers, or he may absolutely refuse to speak at all. In the latter instance a mental element is superadded, which in severe cases of chorea is by no means infrequent. Deglutition is often inco-ordinated and imperfectly performed.

A considerable degree of muscular weakness is common in chorea, and in some cases it may be so severe that the patient is unable to raise a limb from the bed, although slight restless movements are seen going on irregularly in the weakened limbs. Such cases, where the weakness is out of all proportion to the choreic movements, have been termed "paralytic chorea," and in many of them the choreic movements are only brought out on attempted voluntary movement.

The sensory phenomena in chorea are not referred to in the text-books with the emphasis which their frequency merits. True, in cases of chorea, the patient does not usually complain of numbness or pain, or any other abnormal feeling. But if the cutaneous sensibility be carefully tested in every case of chorea, it will be found that a certain proportion of patients exhibit a degree of blunting of sensation on one side of the body. This was so in ten cases out of forty-one in our series. Such hemianæsthesia, where present, is always found on that side of the body on which the choreic movements are best marked. One of our cases exhibited marked diminution of acuity of vision, smell, taste, and hearing, on the preponderatingly choreic side, without hemianæsthesia to touch or pain. The hemianæsthesia of chorea is very slight in degree, and can only be detected by careful comparison of corresponding points on opposite sides of the body. When this is tried, a considerable proportion of patients will be found to perceive light touches and pricks more acutely on one side than on the other, and the boundary of this area of blunted sensibility will always be found to lie in the middle line of the body. Sometimes the trunk and limbs are alone involved in the hemianæsthesia, the face and scalp escaping.

As to the reflexes, the superficial reflexes are occasionally diminished on the hemianæsthetic side. The only other point worthy of note in this connection is the "choreic knee-jerk," which is demonstrable in a certain proportion of cases. When present it differs from the healthy knee-jerk, in that when the patellar tendon is tapped, instead of producing a brisk contraction of the quadriceps extensor, followed at once by a sudden relaxation, the knee in such choreic cases is suddenly extended to its full extent, and remains so for a second or two, the foot and toes meanwhile exhibiting irregular wriggling movements. In very severe cases of chorea, with much mental dulness, the sphincters may become uncontrolled, the patient passing urine and fæces in bed.

The duration of the disease is very variable. An ordinary case of moderate severity usually lasts six or eight weeks, but some cases may last six months or more. Moreover, the disease tends to recur again and again, so that it is not uncommon for a patient to have four, five, or six attacks.

Here it may be well to refer to certain cases, which present all the motor symptoms of choreic movements continuously for a number of years without intermission. One of our cases was of this variety, and I have Dr. Hughlings Jackson's kind permission to publish a reference to this case. He was a lad 17 years of age, who had no history of previous rheumatism, nor of any nervous disease either in himself or in any of his relations. His heart, moreover, was normal. Eight years previously, when he was 9 years of age, he is said to have been frightened by a thunderstorm, and ever since then he had exhibited typical choreic movements of face, trunk, and limbs, equally on both sides, the movements gradually increasing in severity, affecting the tongue and soft palate, and with very irregular gasping respiration. There was no hemianæsthesia, nor any affection of the special senses. His left testicle was undescended. He remained under observation for four months, and was discharged only slightly improved, in spite of treatment. Here, then, is a case which differs markedly from the ordinary type of chorea, both in respect of its abnormal duration and in its resistance to treatment. And yet, although it presents all the motor phenomena of chorea, it is undoubtedly not the same disease. We have, however, no better category at present in which to classify it. It is probably, I would suggest, a "developmental chorea." Since this case I have seen a second similar instance in a female patient. Sir W. Gowers[1] refers to another case in a lad, lasting three and a half years, which I would class in the same category.

Again, two cases of our series were examples of Huntington's chorea—a hereditary disease which is probably entirely distinct in its etiology from the ordinary juvenile type of chorea. They both occurred in females, æt. 38 and 47 years respectively, at the time of onset of the disease. In the first of these, the patient's father, her paternal uncle, and paternal grandfather, had all suffered from the same disease, as well as the patient's own younger sister. In the second, the patient's mother had suffered from the disease for a number of years before death, also her maternal grandmother. The first case had had rheumatic fever five years before the onset of the disease; the second was not rheumatic herself, although one of her children died of rheumatic fever. The first case began spontaneously, without apparent cause; the second patient's symptoms came on immediately after the mental and physical shock of being discharged forcibly and thrown down stairs by her employer. In each case, both sides of the face, trunk, and limbs, were affected, but the first patient showed more movements on the left side than on the right. The heart was normal in both cases. Both of them exhibited the characteristic slight mental enfeeblement of Huntington's chorea. Such cases of hereditary adult chorea are obviously not the same disease as the ordinary acute chorea of youth. They are probably examples of a degenerative disease.

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[1] "Diseases of the Nervous System," 1893, vol. ii. p. 609.

As to the complications of chorea, by far the most important is valvular heart disease, generally of the mitral orifice. We have already seen that many cases of chorea occur in patients who are already rheumatic. And yet, even where there is no previous history of rheumatism, a large proportion of choreic patients exhibit well-marked valvular disease. As we have seen in our series of cases, out of forty-one patients there were twelve in whom no history of rheumatism was obtainable, yet six of these had mitral disease, and two of the others were cases of Huntington's chorea, and a third was the patient with "developmental" chorea. Five other cases out of twelve, in which there was an actual previous history of acute rheumatism, showed some cardiac valvular disease. Thus a large proportion of cases of chorea (nineteen out of forty-one) show some evidence of cardiac affection. It is extremely rare to find a patient with a second or third attack of chorea, in whom the heart has not become affected. Moreover, valvular disease may be quite distinct at the second attack of chorea, in a patient in whom it was quite absent during the first attack, and this without any rheumatic fever in the interval between the two attacks of chorea. It is certain, therefore, that chorea may produce valvular disease, in the absence of an attack of rheumatic fever, and this fact may be held to support the theory of the essentially rheumatic nature of chorea.

In some choreic children we find subcutaneous "rheumatic nodules," varying in size from a pea downwards, and most commonly distributed along the subcutaneous borders of the ulna and tibia, and around the elbow- and knee-joints. One child in our series had a painful contracture of the palmar fascia, clearing up under anti-rheumatic treatment.

Certain cases of chorea are accompanied by mental dulness, amounting in some cases to acute dementia. In one of our cases, the patient was silent and apathetic for four weeks, passing urine and fæces in bed, and requiring to be fed by the nasal tube, as she would not swallow food put into her mouth. Yet this patient, after passing through a stage of mild delusions, ultimately recovered completely, as most cases of post-choreic insanity do.

In the treatment of chorea, the first and all-important thing to secure for every patient is absolute rest, physical and mental. Rest in bed should be insisted on, even in mild cases, for several weeks at least, and when the patient is allowed up, it should only be for a short time each day, gradually lengthening the time permitted to be spent out of bed. In severe cases, to prevent the patient from falling out of bed or hurting himself, it may be necessary to lay him on a mattress on the floor in the corner of the room, well padded with cushions or pillows, to prevent him from knocking himself against the walls. It will be found advantageous to keep a choreic patient in bed until his involuntary movements, on holding the hands horizontally in front of him, are very faint.

The diet should be nutritious and easily absorbed. The more food the patient can take, the better. In severe cases, where there is difficulty in deglutition, or where, from mental dulness, the patient does not take enough nourishment, nasal feeding must be resorted to.

Dr. Hughlings Jackson treats his cases of chorea by the administration of alcohol—a teaspoonful of brandy every four hours in severe cases—and with excellent results. As to drugs, arsenic is the classical remedy. It is most safely given in small doses. Large doses of arsenic, 15 to 20 minims of liquor arsenicalis, are recommended by Dr. Murray of Newcastle; but, as Dr. Murray admits, this method of administration, if it be persisted in too long, is not devoid of risk. Arsenic in large doses, administered for a lengthened period, not only induces a brownish pigmentation of the skin, but has sometimes produced very intractable peripheral neuritis.

Antipyrin is useful in some cases, but care must be taken not to give doses large enough to produce cardiac failure. The patient must, of course, be kept absolutely at rest during the treatment by antipyrin.

In cases of very violent chorea, chloral may be exhibited until the patient is rendered drowsy, but it must be administered with caution, bearing in mind that in such cases the chloral sleep may sometimes be followed by noisy maniacal excitement.

Finally, we must remember that chorea is a disease which is prone to recur. Each successive attack must be treated on the same broad principles—rest and feeding being vastly more important than medicinal therapeutics.

THE FUNCTION OF THE DECIDUAL CELL.

By W. E. FOTHERGILL, M.A., B.Sc., M.D., *Manchester*.

THE stroma of the uterine mucous membrane, when healthy and unaltered by pregnancy, presents little to the eye of the microscopist beyond a multitude of small cells, round or oval in shape, with conspicuous nuclei and but little cell protoplasm. As modified by pregnancy, the stroma of the uterine mucosa has a very different appearance, for many of the cells are greatly enlarged and they vary infinitely in shape. Their nuclei are large and vesicular, with clear outline and nuclear network, while their cell protoplasm is abundant and granular in appearance. These changes are well seen in sections of casts shed from the uterus in cases of ectopic gestation, for in these the formation of the uterine decidua goes on uncomplicated by contact with fœtal structures.

It has been maintained that this enlargement of the connective tissue cells of the uterine mucosa occurs only during gestation, and that the so-called "decidual cell" is accordingly pathognomonic of pregnancy. This view, however, has been strongly opposed; for cells indistinguishable from decidual cells have been found in the uterine mucosa in various morbid conditions. Further, the formation of a decidual membrane is now thought by some to be possible, apart from pregnancy either uterine or ectopic. Be this as it may, the fact remains, that the enlargement of the cells in the stroma of the uterine mucosa is a constant feature of the pregnant state.

The purpose of this paper is to suggest a hypothesis as to the action of the decidual cells in normal gestation; my attention, however, was called to the point while engaged in pathological work. For whilst investigating fleshy moles and placental polypi, I noticed that, under certain circumstances, the decidual cells appear to play an active part in the changes that occur in placental tissue retained *in utero* after the death of the fœtus. Thus, if retained placenta remains free from septic infection, and if its decidual portion is not separated by effusion of blood from the uterine wall, the decidual cells appear to attack and absorb the dead fœtal portion of the placenta. They first destroy the chorionic epithelium covering the villi and the clotted maternal blood in the intervillous space; later and more slowly they appear to attack the connective tissue cores of the villi.

Most of the material upon which these conclusions are based was obtained from cases of missed abortion. This condition has long been well known clinically in this country; and, of late, the term which we use in naming it has come into use abroad. In fact, "missed abortion" is now good French and good German But though reports of cases from the clinical standpoint are very numerous indeed, records of cases which have been submitted to complete examination are very rare. To make clear the action of the decidual cells in certain cases of missed abortion, I mention the following example:—

Mrs. P., æt. 35, had an ordinary menstrual and obstetric history, and always enjoyed good health, until, in November, menstruation suddenly ceased. She did not suffer from morning sickness, nor did she notice enlargement of the breasts or any other sign of pregnancy. She was slightly ailing in January, due to the fatigue of nursing two of her children through scarlet fever. Amenorrhœa remained complete for seven months. In June she complained of some pelvic pain, and a brownish discharge appeared. Upon examination, there was no suprapubic dulness; the uterus was longer and narrower than is usual in pregnancy, but in size approached that of one three months pregnant; its position was normal; its consistency was firm, like that of a fibroid. The cervix was not soft, and the external os admitted only the tip of a finger. The examination appeared to set up uterine contraction; for a "fleshy mole" was expelled a few hours later without much bleeding. Recovery was rapid and complete.

The mass expelled from the uterus consisted of the products of conception compacted into an ovoid body 2¾ in. long, 1¾ in. thick, and firm and tough to the touch. Its outer surface consisted almost entirely of the maternal aspect of the placenta, which was wrapped round so as to enfold the membranes. The cavity of the

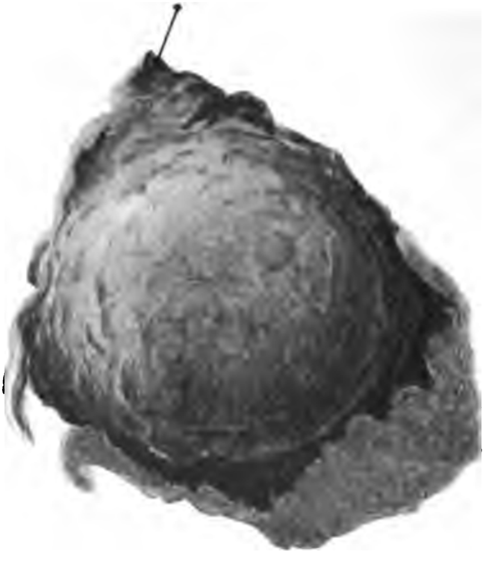

FIG. 1.—Fleshy mole. Uterine surface.

ovum was intact, and was reached by cutting through a thin layer of shreddy decidual tissue and the united chorion and amnion. The amniotic cavity contained nothing whatever, liquor amnii, fœtus, and cord having been absorbed. The membranous portion of the ovum having been incised and gently turned back, the specimen now presents the appearances figured. The uterine surface of the placenta (see Fig. 1) is smooth, and is formed of decidual tissue and blood clot. The amniotic surface of the placenta is thrown into bosses or elevations separated by deep furrows. The amnion is stretched smoothly over these bosses, and dips down between them into the furrows (see Fig. 2). Transverse section shows that the placenta varies in thickness from ¼ in. to ½ in., the membranes and decidua being together less than ⅛ in. thick. Micro-sections show villi, fibrin, decidual cells and a little fresh blood compacted into a firm structure.

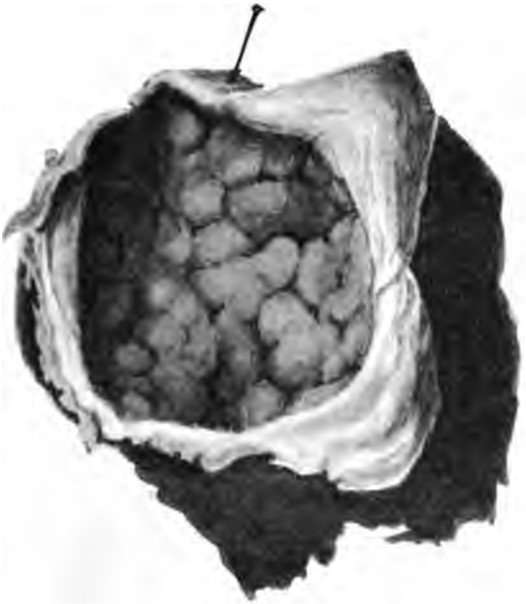

FIG. 2.—Fleshy mole, amniotic surface.

Fig. 3 is from a section cut at right angles to the uterine surface of the placenta. The lower part of the figure shows a portion of the decidual tissue which forms the maternal surface of the placenta, above are parts of three chorionic villi. The dark areas are fibrin, derived from the maternal blood which fills the intervillous spaces. It is seen that the villi are partly surrounded by decidual tissue and that traces of the chorionic epithelium are visible only where the villi are in contact with fibrin, and not where the decidual cells are touching the villi.

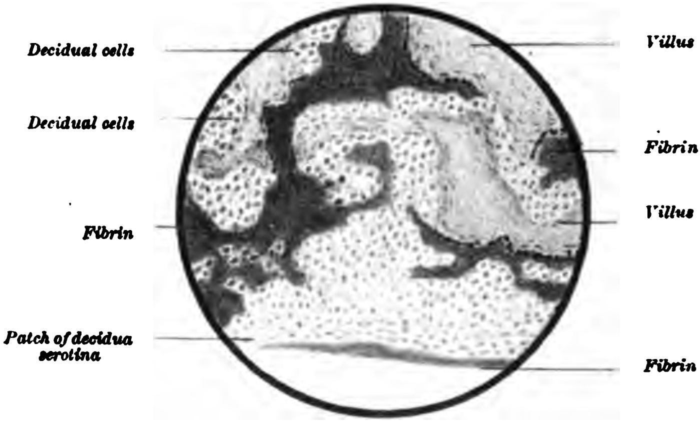

FIG. 3.—Section showing the uterine surface of the placenta. Decidual cells are seen spreading from the patch of original decidua, removing fœtal epithelium from the villi and organising fibrin.

Fig. 4 is from a section cut at right angles to the amniotic surface of the placenta. The upper portion of the figure shows the

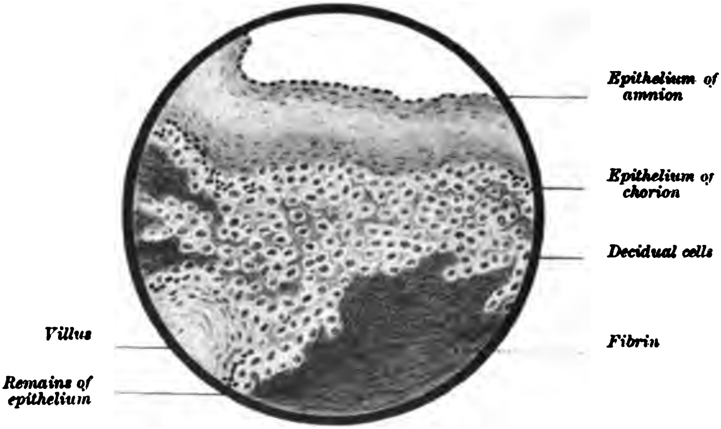

FIG. 4.—Section showing fœtal surface of the placenta. Decidual cells are seen removing the degenerated fœtal epithelium from chorion and villus, also organising the fibrin.

amnion and the chorion united into one membrane by the union of their connective tissue layers. The epithelium of the amnion remains almost unchanged, and appears as a single row of clearly stained cells. The epithelium of the chorion, however, is represented only by a few darkly stained and somewhat scattered nuclei. A villus is shown below, its epithelium reduced to a few nuclei like that of the chorionic membrane itself. Some fibrin is seen, and the remainder of the field is filled with decidual cells.

Judging by the size of the specimen and the stage which placental development had reached, it is clear that the ovum died when the patient was "ailing" in January, and that it was retained *in utero* dead for about five months. During this time the decidual cells appear to have invaded the fœtal portion of the placenta, and to have destroyed all the chorionic epithelium with which they came in contact, besides a certain amount of the connective tissue cores of the villi. The maternal cells have exercised what may be called a phagocytic action upon the dead fœtal structures.

Now if, in certain morbid conditions, the decidual cells have a phagocytic action upon fœtal epithelium, is it not possible that their normal function also is to destroy fœtal structures with which they come into contact? With this question in view I have examined all the sections of decidual tissue in my possession, and have made a number of new ones, using for this purpose the freshest and most healthy specimens obtainable. To describe all material examined would be impossible in one short paper. I therefore give a brief general account and three figures illustrating the appearances observed.

In sections of the healthy placenta or of the membranes of the ovum, the decidual or maternal portion is seen as a layer of tissue varying in thickness, separated from the chorion by a space containing chorionic villi, most of them cut transversely. If the sections have been prepared with sufficient care, maternal blood is seen in the spaces between the villi. Such preparations show that comparatively few villi penetrate into the decidua, though many touch it superficially, and a considerable number have more or less of their circumference embedded in it. Where villi are in contact with the decidua, their epithelium is either absent or is represented only by a few darkly stained nuclei. As a rule only the connective tissue core of the villus remains, and in this no fœtal blood vessels can be seen. In early specimens the surface of the decidua, where in contact with villi, is always degenerated. It appears as an almost structureless layer of material, staining more deeply than the cell protoplasm in healthy parts of the decidua. This material contains numerous clouded and darkly stained nuclei. It can be traced downward into the underlying portion of the decidua, where it is seen to be continuous and identical with the intercellular material, which forms thin lines and defines the outlines of the decidual cells in some places.

Fig. 5 shows the surface of the decidua in an ovum at the end of the third month of development. This ovum showed no trace of disease, and the condition of the maternal blood between the villi indicates that little change, if any, occurred in the placental circulation before the specimen was expelled from the uterus. The villi in the maternal blood space are complete, though too small to contain fœtal blood vessels. The villi in contact with the decidua are losing their epithelium. One embedded villus contains a trace of a fœtal blood vessel. The surface of the decidua is degenerated; the cell outlines are lost, the cell protoplasm being fused into a continuous mass, somewhat deeply stained, in which some dark clouded nuclei remain visible. Below the surface large healthy decidual cells are seen.

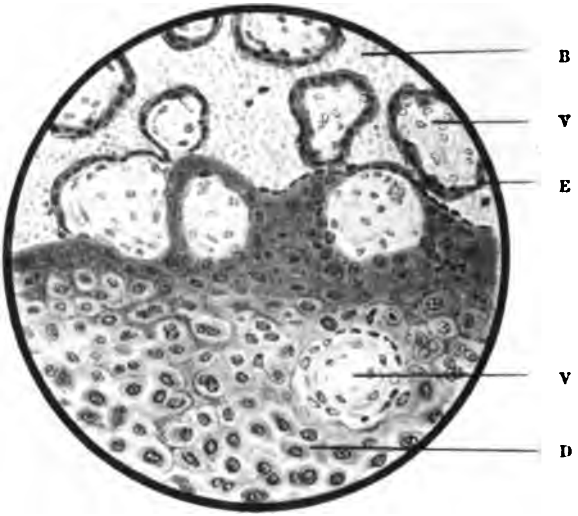

FIG. 5.—Decidua and villi, third month. B, maternal blood; V, villus; E, fœtal epithelium; D, decidual cell.

Figs. 6 and 7 show the surface of the decidua in an ovum over 4 months old, which was fresh and free from post-mortem change. In Fig. 6 two villi are seen cut longitudinally. Where their tips penetrate the decidua their epithelium is absent, though it remains complete on those parts of the villi which are in contact with the maternal blood. The core of a completely embedded villus is seen cut transversely, its epithelium being entirely absent. Fig. 7 shows a large villus cut longitudinally; one side is touching the decidua and has no epithelium, the other side is free and complete. Two fœtal blood vessels are seen in section within the villus. In Figs. 6 and 7 the degeneration of the decidua is more superficial than in Fig. 5, healthy decidual cells having approached the surface more closely than in the younger specimen.

It has for some time been allowed that in the early stages of development of the ovum, the chorionic epithelium has a destructive or phagocytic action upon the surface of the maternal mucosa. Indeed, erosion of the surface of the decidua appears to

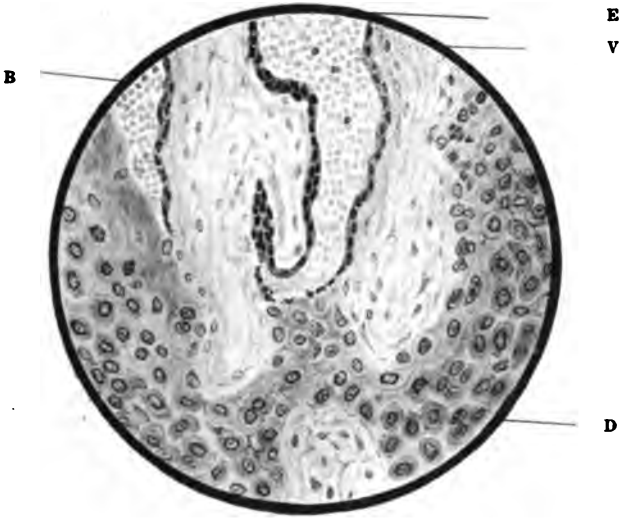

Fig. 6.—Decidua and villi, about 4½ months. Letters as in Fig. 5.

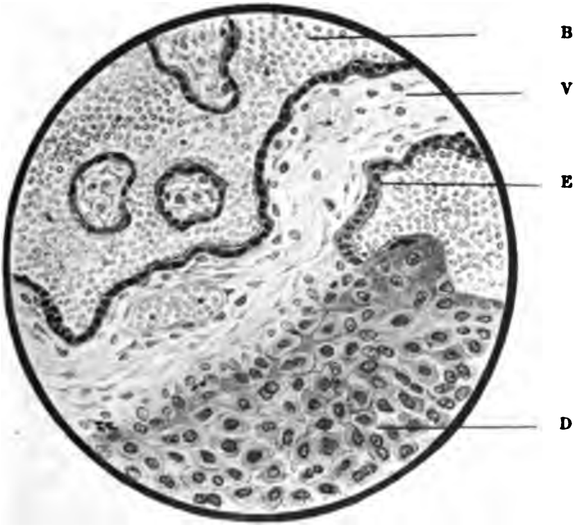

Fig. 7.—Decidua and villi, about 4½ months. Letters as in Fig. 5.

be the means by which the decidual blood vessels are opened up, so that the maternal blood can flow into the intervillous space and come into direct contact with the chorionic villi.

But if fœtal epithelium can destroy the maternal tissues, how is it that the tips of the villi do not penetrate farther into the uterine wall? The view I would suggest is that the decidual cells have, in their turn, a destructive action upon fœtal epithelium. In the formation of the placenta and membranes there seems to be a struggle between two tissues, one fœtal—the epithelium of the chorionic villi—and one maternal—the connective tissue of the uterine mucosa. Both tissues suffer in the struggle, and, during the early development of the ovum, the line of contact between the fœtal and maternal portions of the placenta and membranes is marked by a layer of degenerated and practically dead material—the dead superficial layer of the decidua, and, embedded in it, the dead tips of chorionic villi, which have penetrated it and lost their epithelium and their blood vessels in the process. To begin with, the fœtal epithelium is the more active, the decidua the more passive tissue, and the former invades the latter. Then the decidua reacts against and checks the invasion, its superficial layer perishing in the struggle, along with the tips of the invading villi. Later, the fœtal epithelium becomes less active. This change can be seen throughout the whole placenta, for after the fourth month the coverings of all villi show degenerative changes. The underlying layer of the decidua now becomes more active, its cells multiply and enlarge rapidly and advance into the superficial degenerated layer, eating it up as they go, until, soon after the middle of pregnancy, large healthy decidual cells occur right up to the surface of the decidua which is in contact with maternal blood. Amongst these cells hardly a trace of fœtal structures can be found.

Struggle between the elements of a couple of tissues is a well-known factor in various developmental processes, and there appears to be no reason, *a priori*, why it should not play a part in the formation of the placenta and membranes. The fertilised ovum, in arranging for its own nutrition, is known to make an attack on the maternal structures. The decidual cell, it is suggested, has been evolved as a protection, its function being to prevent injurious invasion of the uterine wall by the fœtal elements of the placenta. The decidual cell might be called a phagocyte, whose duty is to balance the well-known phagocytic action of the chorionic epithelium. In somewhat inaccurate but intelligible language, the ovum is a parasite, and the decidua prevents it from being an injurious parasite.

It is possible to imagine circumstances in which the decidua might fail in the performance of this duty, and permit of injury to the mother by the products of conception. For instance, perforation of the uterine wall by villi, in cases of vesicular degeneration of the chorion, might possibly be explained in this way. Again, those who believe in the existence of malignant neoplasms composed of fœtal epithelium might find an explanation in terms of this hypothesis. Failure of the decidual cells in the performance of their phagocytic function might be supposed to permit the entrance of fœtal cells into parts of the maternal organism where they could multiply into a new growth of foreign origin.

In studying the physiology of the placenta, the great difficulty is that of obtaining healthy specimens. It may be said that my own material, being from abortions, is all pathological, and that my conclusions are therefore untrustworthy. Perfect material is obtained only when the pregnant uterus is removed by operation for disease which does not affect the uterine cavity. Next best are pregnant uteri removed post-mortem. Though I have not yet been able to obtain such specimens for my own investigation, I have had opportunities of examining preparations made by others who have been more fortunate. Their appearances are identical with those seen in my own preparations from complete ova expelled from the uterus as the result of accident, or of maternal disease not directly affecting the ovum. One of the main objects of this paper is to urge those who may be fortunate enough to obtain pregnant uteri in good preservation, to have them examined microscopically. I may be allowed to express regret that some pregnant uteri recently removed have been put into bottles and placed in museums where they can never add anything to our already sufficient knowledge of the naked-eye appearances of the pregnant uterus. If hardened and cut up into sections, they would have afforded information which can be obtained from such specimens alone.

The literature of the decidua is far too vast to allow of any attempt to deal with it in this short paper. I must therefore apologise to certain observers whose work and views it would be not only a duty but a pleasure to discuss, did space permit.

URINARY ANTISEPSIS.

By A. A. Warden, M.A., M.B., C.M., *Resident Surgeon, Hertford British Hospital, Paris.*

The variety of instruments required, and the conditions of catheterisation itself as an operation, necessitate antiseptic precautions altogether peculiar to urinary as distinguished from general surgery. Thus, use is made of metallic, gum-elastic, and soft-rubber instruments, and a method which suitably sterilises the one may infallibly destroy the other. Again, no general surgeon would neglect to cleanse his patient's skin for the bistoury, yet it is scarcely less necessary to prepare the way for the catheter or the bougie. Antiseptic principles happily lead to greater simplicity rather than complexity in surgical procedure, and, though a few practitioners may still be found bold enough or careless enough to take an instrument from a dusty table and pass it at once through the urethra into the bladder, they are, happily, in the minority, and the sterilisation of urinary instruments is to-day so simple as to be within the reach of all, and should be practised by every conscientious physician.

I. ONE WORD AS TO HIS PREPARATION OF THE PATIENT.—The first step is to cleanse the field of operation; and the penis, glans, prepuce, and meatus should be washed with soap and water and swabbed with cotton-wool dipped in sublimate solution, 1 in 1000, or other effective antiseptic.

The next step is to cleanse the urethra by means of the syringe, and as the anterior urethra only (in the uninfected subject) has been found to contain microbes, the solution should not be forced through the membranous portion into the posterior urethra or bladder.

The lips of the meatus should be separated by the thumb and index-finger of the left hand, and a strong jet sent in from some little distance, gradually approaching the syringe until the urethral orifice is closed by means of Janet's glass olive at its tip. Gently pushing the piston, the anterior urethra is filled with some tension, shown by the forcible escape when the syringe is withdrawn. The little manœuvre should be repeated several times with either boiled water or boracic solution.

These precautions are indispensable, for the præputial region in men and the vulva in women have been shown to be hotbeds of the *Bacillus coli communis*. Bacteriological evidence has also shown that no number of injections will free the urethra from microbes.[1] Clinical experience happily, however, shows that even the most receptive patients may, after reasonable and practical precautions, be sounded without danger.

II. CATHETERISATION BY PATIENT HIMSELF.—So many patients, especially those suffering from an enlarged prostate, are obliged to use the catheter themselves, and, of that number, so many sooner or later succeed in infecting their bladders, that it behoves every surgeon to suggest some simple plan of campaign by which an event, so dire in its immediate and ultimate consequences, as urinary infection, may be best avoided. Practically the only antiseptic measures within the reach of every patient are those obtainable with soap and boiling water, and happily these suffice. He should, however, have in stock several catheters ready for use, and he should always employ a catheter of large calibre. Thin-walled gum-elastic catheters with double eyeholes are therefore preferable to the single-eyed soft-rubber instruments which, as a rule, have smaller calibre and are less effective in washing out and emptying the bladder. The steps of the simplest and the safest procedure are the following:—(1) Wash the catheter with soap and warm water. (2) Boil it for five or ten minutes in an ordinary enamel pan. (3) Replace the boiling water by boracic solution. (4) Open the vessel containing the lubricant required. (5) Wash hands, meatus, prepuce, and glans penis, and all is ready for a safe catheterisation.

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[1] Guyon, "Leçons clin.," tome ii. p. 20.

When finished, wash the instrument again with soap and warm water, and put it away to dry in lint.

The pomade Guyon uses is composed of equal parts of glycerin, water, and powdered soap, with 1 per cent. of phenol or beta-naphthol added. Prepared aseptically, it keeps pure for weeks, is easily soluble in water, and is actually cleanly, unlike vaselin, glycerin, and various oils in present use.

III. STERILISATION OF INSTRUMENTS.—Many and various have been the methods and substances used far this object. None, especially in the sterilisation of gum-elastic instruments, has hitherto been found free from objection. The methods to be referred to are three, namely—(1) By boiling, (2) by antiseptic solutions, and (3) by formol or rather trioxymethylene vapour. The instruments to be sterilised may also be divided into three classes, namely (1) Metallic, (2) soft rubber, and (3) gum-elastic; and this double group of three may not unfitly be considered together.

1. *Boiling.—Metallic bougies* may thus be sterilised as other surgical instruments, namely, by washing with soap and water, and then boiling for not less than ten minutes in water made alkaline by the addition of carbonate of soda. Before use or during use they should be kept in boracic solution or boiled water. This method is so simple that it may be carried out whenever such instruments are needed.

Metallic catheters are, it is to be hoped, gradually dropping out of use. If a patient have retention, partial or complete, whether it be due to congestion, spasm, stricture, or an enlarged prostate, the best instrument to use is either the soft-rubber (the worm-catheter) or the gum-elastic instrument. If the stricture be a tight one, a small metallic catheter is the most dangerous instrument that could be chosen, and the retention will be far more easily and far more safely relieved by a gum-elastic filiform bougie.

Soft-rubber instruments, if of good quality and well vulcanised, bear ebullition well (provided no carbonate of soda be present), but they should always be first thoroughly washed and syringed through with soap and warm water. This latter is an effective antisepsis, and reduces the time necessary for ebullition from half an hour to ten minutes. These catheters should be thin-walled, and beyond the eye there must be no *cul-de-sac*, that end of the catheter being solid.

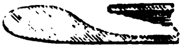

FIG. 1.—End of rubber catheter.

Gum-elastic instruments may also be thus sterilised, but only a limited number of times, as they gradually become rough and therefore unserviceable.

Antiseptic solutions.—To be effective such solutions must be strong, and a gum-elastic or rubber instrument cannot remain long in such a solution without becoming unfit for use. Of all antiseptics the most injurious is that in most frequent use—carbolic acid. Boiling water is so effective and simple a sterilising agent for metallic instruments, that antiseptic solutions are, in this case also, of little value. Boracic acid may be used temporarily to keep metallic and other instruments already sterilised, but any solution undoubtedly in time alters rubber sounds, and rapidly destroys gum-elastic ones, so that it may be laid down as a general rule, that *all urinary instruments should be preserved dry.*

3. *Formol and trioxymethylene vapour.*—Having found, in boiling water, an excellent procedure for the sterilisation of metallic and rubber instruments, it may be said at once that trioxymethylene vapour is as satisfactory for gum-elastic instruments. Since all that is required is an air-tight box or case in which to sterilise the sounds, and a small quantity of the formol solution or the trioxymethylene powder, we may be pardoned going somewhat fully into the experiments that have placed its extraordinary antiseptic properties beyond question.

To Miquel, Trillat, Berlioz, Franck, and Janet are due our knowledge of its properties and of its value, the experiments made by the two latter in Berlin and Paris respectively being similar in result and absolutely conclusive.

Formol is an aqueous solution, containing about 40 per cent. of formic aldehyde. If made more concentrated by evaporation, a white powder, trioxymethylene, is obtained, which gives off in the form of vapour the original formol.

Formol in liquid or in gaseous form injures in no way metallic, gum-elastic, and rubber instruments, but it is in dealing with gum-elastic bougies and catheters that recourse should be had to it.

Janet made two series of experiments—in the *first*, suspending the catheters from the rubber stopper of glass tubes, of three different sizes, at the bottom of which was a tampon of absorbent gauze soaked in formol, in solutions of varying strengths; in the *second*, that which is more important for us, because the experiments were made with the box we recommend for use, the formol was spread upon a piece of lint in the bottom of a metallic box, which closed hermetically, and upon the several shelves of which were laid the catheters to be sterilised.

These cases are made by Gentile of Paris, but they are expensive; and if the principles they imply be kept in mind, a simpler mechanism will probably meet the requirements of most practitioners. The box should be air-tight, to prevent the escape of the trioxymethylene vapour, which is our disinfecting agent; and it should be so arranged that the formol or trioxymethylene may be sprinkled in lint, in such a way that the catheters to be sterilised do not come in contact either with each other or with the lint.

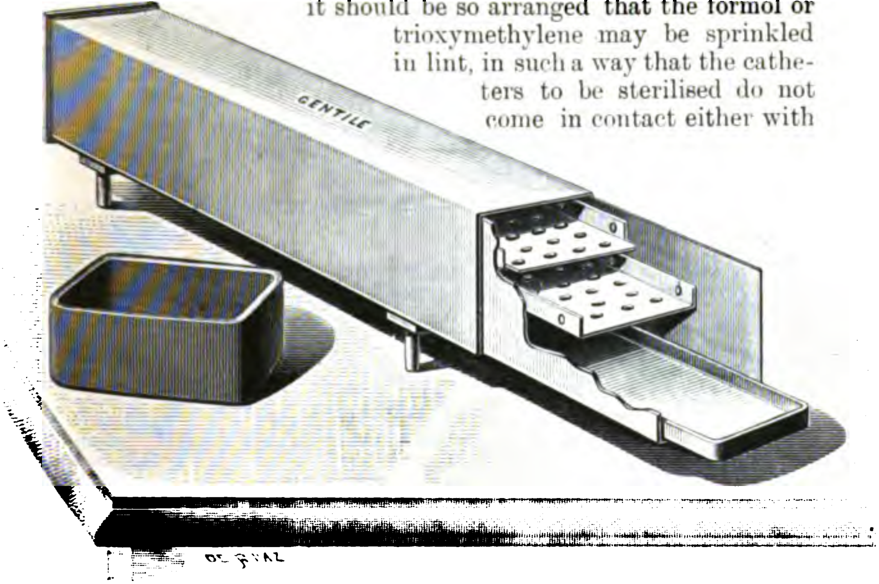

Fig. 2.—Gentile's catheter case.

The sounds and catheters experimented with were infected with bouillon containing purulent urine, were dried in the air, and then introduced (some washed with soap and water, others unwashed, into the formol apparatus, from which, after a noted period, they were plunged into sterile bouillon. Of Janet's many and careful experiments, to which reference has already been made, I shall only quote the results with formol and trioxymethylene in the metallic case made by Gentile, and as regards catheters of ordinary size and of the smaller sizes, which, of course, become more difficult to sterilise.

1. *Ordinary catheters and formol.*—Experiment 1.—Five infected catheters, unwashed, are left in the case with pure formol poured upon the lint, for twenty-four hours, at a temperature of 17° C. (about 62° F.).

Result.—These five catheters fail to cultivate.

Experiment 2.—Three infected catheters, washed through with soap and water, are placed in the case containing pure formol, at 17°, for twenty-four hours.

Result.—These catheters do not cultivate.

That is to say, an absolute sterilisation takes place in each of these eight trials, whether the catheters be washed or not.

2. *Ordinary catheters and trioxymethylene*; the latter being thinly sprinkled upon lint.

Experiment 3.—Four catheters infected, unwashed, are placed in the metallic case for twenty-four hours, at 17°.

Result.—Three do not cultivate. One cultivates at the end of forty-eight hours.

Experiment 4.—Three catheters, infected, washed in similar conditions.

Result.—They do not cultivate.

That is to say, six out of seven trials are successful, the one failure being an unwashed catheter.

Very small catheters, instillators, etc., and formol and trioxymethylene.—These were infected, by injecting through them with a syringe, infected bouillon. Afterwards, those that were washed, were syringed through with soap and water.

Experiment 5.—Two instillators, infected; one washed, the other unwashed; for twenty-four hours; temperature, 17°; formol.

Result.—Both cultivate, the unwashed one in twenty-four hours, the washed one in forty-eight hours.

Experiment 6.—Eight instillators, one unwashed and seven washed, for forty-eight hours, in case with formol; temperature, 17°.

Result.—The unwashed instillator cultivates in forty-eight hours, the other seven do not cultivate.

That is to say, all fine instruments that had been washed were absolutely sterilised in forty-eight hours.

Janet's conclusions may be thus formulated—

1. Spread the formol or the trioxymethylene over a large surface.
2. Keep the temperature as near as possible about 15° C. (about 58° F.), never below 10° C. (about 50° F.).
3. Sterilise ordinary-sized catheters for twenty-four hours, small ones for forty-eight hours.

Of the two, the solution of formol or the powder of trioxymethylene, the latter is much the more convenient, is as efficacious, does not soften the instruments, provided they be introduced *perfectly dry*, and, of course, does not evaporate.

From the steriliser the sounds or catheters may be laid in boracic solution during or shortly before use, on account of the slightly irritant effect of formol upon the mucous membrane of the urethra. Undoubtedly the best way to keep sounds, once sterilised, is in flat glass dishes, containing small quantities of chloride of calcium to dry them, and of trioxymethylene to keep them aseptic. They are thus free from contact with each other, and have also plenty of air, which prevents their becoming soft.

Or, if frequent use is not made of catheters and sounds, they may be kept indefinitely in the metallic case containing the trioxymethylene.

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REVIEWS OF BRITISH AND FOREIGN LITERATURE.

Contributions to Clinical Medicine. By T. M'Call Anderson, M.D., Professor of Clinical Medicine in the University of Glasgow. Edited by James Hinshelwood, M.A., M.D. Edinburgh and London: Young J. Pentland. 1898.

The Professor of Clinical Medicine in the University of Glasgow has been well known in the profession for more than thirty years. His first successes were in the field of dermatology, in which he did admirable service in the way of making known the best teachings of Hebra, von Barensprung, and the great skin doctors of Paris. It was clear, even in his earliest writings, that he was gifted with the faculty of succinct and lucid statement, and that, while he mastered and stated well the opinions of others, he used his own judgment in regard to every question, and showed no little discretion in its use. During all the years that have elapsed since his early books on parasitic diseases and eczema were published, he has gone on working steadily at clinical medicine. And so, now that he is a man of large experience, both as a physician and as a teacher, the profession has the advantage of learning from him, not only in his great work on skin diseases, but in many other departments of clinical medicine.

The volume before us, entitled "Contributions to Clinical Medicine," is edited not by the author but by Dr. Hinshelwood of Glasgow. It might have been interesting to some of us, who have watched his various contributions as they appeared, to see them reprinted in order of time; but, on the whole, a better plan is adopted, for the papers are grouped according to their subjects. We begin with some fifty pages on general diseases. These are followed by two hundred on diseases of the nervous system. These again by considerably more than a hundred on diseases of the circulation, while the digestive and the urinary follow. There may occasionally be a little difficulty as to the principles which determine the allocation of articles to these groups, as, for example, when Addison's disease figures among the maladies of the urinary system; but, on the whole, the arrangement is good, and information is readily got at.

I do not propose to go over the whole list of papers, but shall rather refer to one here and there which seems to me of special interest.

The second chapter, which discusses some of the less common forms and modes of treatment of the syphilitic infection, is well worthy of perusal; giving, as it does, examples of that malady affecting the nervous system, simulating Hodgkin's disease, inducing persistent thoracic pain affecting the eyes and brow, the knee-joints, the cornea, and many more. In regard to treatment, his results do not afford much encouragement to the use of antisyphilitic serum, but amply confirm our faith in the remedies generally recognised.

His article on the treatment of myxœdema, by means of thyroid extract and otherwise, is admirably illustrated.

Considering Dr. M'Call Anderson's special skill in skin diseases, one naturally turns to his article on hæmidrosis, in which he discusses the whole subject in a learned way, and in seven succinct paragraphs sums up the general conclusions at which he arrives.

The value of antipyrin in chorea is remarked by many writers, but Dr. Anderson speaks also warmly of its value in epilepsy. He recommends that it should first be given in small doses, and gradually increased, very large doses being administered before the medicine is discontinued.

The article on intracranial tumours will well repay perusal, both in respect of the clinical facts observed and the medical and surgical treatment adopted. The same remark is true of the chapter on spinal irritation.

An admirable discussion of syphilitic diseases of the nervous system, much of which was communicated to the medicine section at the last Glasgow meeting of the British Medical Association, shows that the author has had great experience of these affections, and has used his opportunities well. It is impossible for us to linger over his articles on aneurysm of the aorta, but I should desire to direct special attention to what he says of its treatment by electrolysis.

The first article on respiratory diseases is to a large extent a reproduction of a valuable little book which our author published many years ago, on the "Curability of Acute Phthisis (Galloping Consumption)." It contains many interesting and, I believe, well authenticated facts, and at its close gives some interesting expressions of opinion from high authorities in the profession, including Sir Robert Christison.

The chapter on mediastinal tumours throws valuable light upon the often difficult question of differential diagnosis between these and aneurysms.

Among the articles dealing with urinary subjects, I have specially to commend those on cases illustrative of hemiplegia, the result of the cirrhotic form of Bright's disease, and on two cases of hydroa æstivale in two brothers, complicated with the presence of hæmatoporphyrin in the urine.

The book as a whole is a valuable record of able and diligent work, and is well worthy of the reputation of Professor M'Call Anderson, as one of the foremost clinicians in Scotland. T. GRAINGER STEWART.

Holden's Human Osteology. Edited by CHAS. STEWART, F.R.S., and R. W. REID, M.D., F.R.C.S. Eighth edition. London: J. & A. Churchill. 1898.

IF we mistake not, Luther Holden was the first to introduce the method of indicating by means of coloured lines the areas of muscular attachment on the bones, a method which has practically revolutionised the teaching of human osteology, and has filled our bone rooms with the gaily painted specimens which make them so attractive. Holden always emphasised a point if he could by telling a tale. *À propos* of the frontal sinus, who can forget the story of the man who, having slept in barns, was afflicted with pains in the forehead, which were only relieved after he had discharged from his nose a worm, belonging to that class which spoils the corn; or of the soldier who was wounded at the battle of Talavera, and had a bullet removed from his frontal sinus. The present is evidently an expurgated edition, for the story of the soldier of Talavera alone remains to point the moral.

A gifted artist, Holden's drawings were always refined and effective; whether it be that the original stones or the transfers from them have become worn out, we cannot say, but apparently the plates have been re-drawn, and, to our way of thinking, have not been improved thereby. Take, for instance, Plate xxxv. in the present edition and compare it with the corresponding plate in the earlier editions, the inner-malleolar width nearly equals the combined width of the upper extremities of the tibia and fibula, and gives one an altogether false idea of the proportions of the bones; in fact, we set much greater store on our second edition, from an artistic standpoint, than its more up-to-date representative; we prefer, too, the arrangement of the plates in the earlier editions, and do not regard the introduction of so many folding plates as an improvement. But whilst we cling thus lovingly to our old friend, we do not fail to recognise the many improvements which have been introduced by Professors Stewart and Reid, and which have brought the work up to the standard required for the higher examinations. For those who can indulge in the luxury of a separate work on human osteology we can confidently recommend "Holden on the Bones" as a reliable authority.

ARTHUR THOMSON.

Lectures on the Malarial Fevers. By W. S. THAYER, M.D., Associate Professor of Medicine in Johns Hopkins University. London: Henry Kimpton. 1898.

SPECIAL works on malarial fever, in our language, are so few that Dr. Thayer's treatise will be warmly welcomed, and all the more so that he is widely known for his original and valuable researches on the subject. "We have in this country," the author says, and his remark applies with as great force to England as to America, "been lamentably backward in fully appreciating the clinical value of the advances in our knowledge of malaria, which have followed Laveran's discovery of the parasite sixteen years ago." Dr. Thayer has undertaken the task of presenting the whole subject in the light of modern discoveries.

As the work is in the form of lectures delivered to students, it is practical in its character, and based mainly upon the author's own observations and experience in Baltimore. Disputed points, and questions of purely scientific interest, such as the structure of the parasite, its position in the zoological system, and its life outside man, scarcely come in for discussion. The value of the book depends upon the extent to which it gives expression to the author's personal and original work. This is also its defect, viewed as a manual for those practising in warm climates, for the graver forms of malaria met with in tropical regions, which are absent from the latitude of Baltimore, are only cursorily alluded to. Still, it is a work which the tropical practitioner cannot afford to overlook.

The first chapter gives a short account of the disappointing search for the contagium vivum of malaria, by Salisbury, Klebs, Tomassi

Crudeli, Lanzi, Terrigi, and Schiavuzzi, during the quarter of a century preceding the grand discovery of Laveran, and of the more recent and fruitful labours of Golgi, Marchiafava, Celli, Bignami, and others, giving references to the original works and monographs, which will be of great service to the student.

The methods of examining the blood, and of staining it for clinical purposes, and the precautions to be observed, in order to obtain satisfactory results, are then described. We think that the simple, rapid, and, upon the whole, satisfactory methods of single and double staining recommended by Laveran, so extremely useful for clinical purposes, deserve to have been more fully described.

The second and third chapters are devoted to a description of the hæmatozoa of malaria, and it may fairly be said that no better account of their characters and evolution can be found in any language. The recent researches of Ross, and the views of Manson respecting the rôle of the mosquito, are summarily dismissed as being "seriously lacking in foundation, and scarcely to be regarded as more than an interesting hypothesis." If we mistake not, it is a hypothesis which is bound to lead to a great advance in our knowledge of the infection in the immediate future, although the results may well be very different from those now anticipated. The section dealing with the conditions under which malarial fevers prevail contains little new. There is, however, an interesting table of the types of fever in 542 cases observed at Johns Hopkins Hospital, during the first and second parts of the year, which may be of value in reference to the question of the unity or the plurality of the parasite. Dr. Thayer hesitates to recognise the small parasite quotidian as a distinct variety, but regards it only as a modification of the summer-autumn tertian. He is the first who has ventured to dispute on this point the conclusions of the Italian School, and we think his reasons for doing so are valid. Ziemann, in his recent work, adopts the same view. The process of multiplying the forms of fever has gone too far.

Space prevents us doing more than to say that the chapters on the clinical forms of malarial fevers, on their sequelæ and complications, on their morbid anatomy and general pathology, are well up to date, full, and yet concise. The chapter on treatment is good, but is unfortunately restricted to the therapeutics of the milder forms of fever met with in temperate climates; the treatment of the bilious remittent, hæmorrhagic, typhoid, and adynamic forms of tropical countries, appear to have been outside the scope of his work. Prophylaxis is most inadequately treated in twenty lines.

Although the work, as a whole, is admirably written, there are one or two paragraphs in which a singular inconsequence in reasoning has in some way crept in. What can one, for instance, make of the following paragraph, from page 91:—

"Grassi and Feletti allowed healthy individuals to drink dew collected from malarious regions without ill effects. They also caused healthy men to drink fresh blood from malarial patients, and fed birds of prey on infected birds, without obtaining any satisfactory results. This is, of course, no proof that the parasite in some form or other may not live, and even multiply in water. It is, however, strong evidence that the infection does not ordinarily take place in this manner." Certainly the fact that "no *satisfactory* results" followed the drinking of fresh blood from malarial patients, or from feeding birds of prey on infected birds, is no proof that the parasite does not live in water, but this goes without saying, and we may be quite sure that Dr. Thayer did not mean to say so. The few flaws of this kind which we have come across, and which do not materially detract from the merits of a very valuable work, will no doubt be removed in a second edition.

ANDREW DAVIDSON.

Diseases of Women. By GEORGE ERNEST HERMAN, M.B.Lond., F.R.C.P. London: Cassell & Co. Ltd. 1898.

IN placing before us this work, the author is to be congratulated in having completely forsaken the conventional lines upon which most gynæcological text-books are written, and having followed an eminently more practical and interesting mode of description. In this he at once displays an originality and resource which alone can be founded on a large experience, and which must, therefore, at once stamp the information contained as of value. It is refreshing in the earlier chapters not to be confronted with the time-honoured description of anatomical details and methods of examination, such being interspersed throughout the volume as necessity demands. It will thus be evident that the object of the author has in the main been to produce a thoroughly practical work, and in this he has been eminently successful. To the teacher and practitioner alike its perusal cannot but be acceptable and beneficial, but to the undergraduate student who is reading for examination, the method of classification adopted is hardly calculated to convey in the most simple manner what is required of him.

The method of classification adopted throughout is entirely symptomatic. The initial chapters are devoted to the consideration of vague conditions, such as hysteria and neurasthenia in their relation to disease of the generative organs, while several chapters are taken up with a most original description of varieties of pain depending on similar causes.

The individual pathological conditions are grouped according to their main symptoms. Thus chronic oophoritis, subinvolution of the uterus, and uterine displacements, are discussed under the heading of chronic pelvic pain; and in like manner cancer of the cervix and submucous fibromyomata naturally are described under hæmorrhage, while subperitoneal fibroids are referred to the group of abdominal tumours. This arrangement, to one already acquainted with gynæcology, is easy to understand and appreciate, but to the tyro must be involved and difficult. The clinical features and treatment of the various conditions are fully and ably described, and, though operative procedures are largely recommended, we are pleased to note that conservative surgery is strongly upheld. The description of surgical methods adopted is perhaps hardly full enough, and tends to convey to the reader a conception of simplicity hardly warranted; this applies as much to the passage of the sound as to vaginal hysterectomy. The curative method of action of the hot vaginal douche, as a pelvic poultice, might also have been more fully dwelt upon. We are pleased to see a description of the signs and symptoms of pregnancy; why this is so generally left out of gynæcological works it is difficult to understand, considering how large a part the elucidation and diagnosis of pregnancy play in the routine work of the gynæcologist.

As is to be expected in a work of such an eminently practical nature, pathological appearances are by no means fully described. In this department we cannot agree with the description of deciduoma malignum, practical experience having shown it to be a proliferation of apparently true decidual cells.

The volume is well printed and illustrated, features which, when taken with the valuable information contained and the delightful style in which it is written, must recommend it to the profession, and ensure that success which it merits.

F. W. N. Haultain.

A Handbook of Hygiene and Sanitary Science. By George Wilson, M.A., M.D., LL.D. Eighth edition. London: J. & A. Churchill. 1898.

Dr George Wilson has issued an eighth edition of his well-known "Handbook." Of the lesser books on hygiene, this is one of the oldest, as it is one of the best. Within the last ten years or so, it has almost doubled in size. These facts mean that this book has been found fairly adequate to its intended uses. The additions to this issue, however, are not all undoubted improvements. The primary purpose of a "Handbook" is to expound a doctrine, if a school of thought is concerned; or to organise accepted knowledge, if, as here, there is no school, but many doctrines. Now, Dr. Wilson's addition on bacteriology and its relation to infectious disease is essentially polemical, not expository, and, as a student of his earlier editions, I feel that, by this chapter, he has, apart from merits, given his book a wrong bias. To name only his discussion on diphtheria and antidiphtheritic serum—the impression that a beginner in public health would take away is that bacteriology is a somewhat confused medley of uncritical practices and results, not, as we know it to be, a system of most exact methods for the study of most delicate phenomena. Then we read: "The *rationale* and warranty, so to speak, of all these protective antitoxin inoculations are based on the protection afforded by vaccination against small-pox" (p. 425). Dr. Sidney Martin and Dr. Sims Woodhead might as well not have written. The "rationale" of vaccination itself is anything but a closed pathological question, and to deny the "analogy" of vaccination and the very precise sequence of processes necessary to form "antitoxin," is to force an open door. But, on the merits, Dr. Wilson seems to me neither adequate nor conclusive. To one unversed in the origin of "tuberculin" and "antitoxin," the discussion of them together must be misleading. The evidence *for* antidiphtheritic serum is not adequately represented. The argument *against* it is somewhat spoiled by non-practicality. Dr. Lennox Browne's criticism was based on definite, precisely described clinical observations; but it is difficult to believe that the "four reasons" for the "lowered death-rate" (p. 436) could come from a practical physician with actual cases in his view. The reasons are suspiciously allied to the "sanitation" argument against vaccination. At least, they equally apply to most drugs in common use. Again, the "not a few who doubt whether all the agonies inflicted on animals sacrificed . . . have saved one single human life," does not cohere well with the immediately preceding sentences on "Jenner's great discovery" and the remarks elsewhere on "calf" lymph. Altogether, this chapter is unfortunate,—not because it is "heterodox" (there is no "heterodoxy" in matters of "evidence"), but because it is not thoroughgoing. In the matter of disinfection, Dr. Wilson mentions "formalin gas." He makes no mention of the Spray Method,—used in Paris and rapidly spreading in Scotland, where "sulphur" is, for practical reasons, falling into discredit and disuse. Many other points might be noted,—for example, the non-reference to the difference between "saturated" and "superheated" steam-disinfectors (p. 531); but in so good a book, already well crowded, minor omissions are of little import.

W. Leslie Mackenzie.

Respiratory Exercises in the Treatment of Disease, notably of the Heart, Lungs, Nervous and Digestive Systems. By Harry Campbell, M.D., B.Sc.Lond., F.R.C.P. London: Baillière, Tindall, & Cox. 1898.

Some little time ago the author of this work published a voluminous treatise on headaches,—so voluminous, indeed, that anyone who attempted to determine under which form any special headache he might meet with in practice should be placed, found himself in a position of considerable perplexity and doubt, unless he treated the case before him as one of the general genus headache, and proceeded to satisfy himself as to the exact species at leisure.

It speaks well for Dr. Campbell's industry—literary at any rate—that he has been able to furnish the last-mentioned volume in 1894, other previous books in 1889, 1890, and 1891, and this treatise in 1898. Such variable fecundity may be praiseworthy when viewed from certain aspects, but is apt to lead to the enunciation of opinions which a diminished celerity in production would probably round off and soften, or might even alter in a radical manner.

The indefatigable manner in which the author hunts up his references, and ascertains the various facts germane to his subject, is highly commendable; but although they are ingeniously dovetailed into each other, the result reminds one perceptibly of a too ambitious menu, the *pièce de resistance* is surrounded with too many made dishes, *entrées* and *relevés*. This does not indicate that the chief dish in the menu is at all badly conceived. The chief item evident is well worthy of exposition, but its flavour would be better appreciated if the attendant sauces and commonplace side-dishes were less lavishly served up with it.

It would surely save time and conduce to lucidity if, instead of devoting separate sections to secondary subjects (some of the chapters barely extend beyond a page and a half, as Chapter xii. for instance; several others fill only two or three pages), the main divisions of the subject were presented in longer separate divisions, with the dependent matter shortly tabulated in the form of sub-sections.

The first subject taken up, "The Elasticity of the Lungs," loses much of its force because of its entire severance from the succeeding examination of the intra-abdominal tension, and from the following chapters on the "Elasticity of the Thoracic Cage," surely indivisible from the first, and on the identical subjects of the factors determining the mean size of the chest, and the mobility of the thoracic cage. The letterpress in each of these, and indeed in the next seven chapters, overlaps that of the others, and could be cut down with immense advantage both to the intelligent reader and to the laborious author. The main subject is not a difficult one; it depends largely upon the simpler laws of physics, and can be grasped, when some inkling of anatomy is possessed, by anyone of common-sense; requires only to be briefly predicated (as it commonly is in text-books on clinical medicine), and its sequelæ clearly pointed out.

Is it worth while to dilate on the fact that active individuals with strong muscular systems have more capacious chests than the weak and sedentary? Surely the average schoolboy knows that the "cock of the school" has more "wind" than he has? Or that a singer invariably endeavours to increase his power of sustaining a high B, by exercising the muscles directly and indirectly connected with his thorax? No one can deny that filling the lungs with air (unless it be contaminated) constitutes one of the most powerful means of enhancing bodily health, whatever be the state of the individual. To those who are still unacquainted with such facts, Dr. Campbell's monograph will be a useful and thorough enlightener; to the others, we are afraid it will prove superfluous—an elaborate conveyance of coals to Newcastle.

A. LOCKHART GILLESPIE.

Traité de l'Allaitement et de l'Alimentation des enfants du premier age. Par le Dr. A. B. MARFAN, Professeur agrégé a l'Université de Paris. Paris: G. Steinheil. 1899.

IN this volume the author places before the reader, in a most elaborate and exhaustive manner, the latest views on the management of infant feeding. The work may be confidently recommended as a scientific treatise on the subject, written in a thoughtful manner, by one who has arrived at his conclusions as the result of careful investigation and of wide experience. The book is divided into two sections—the first part being theoretical and devoted to the chemical analysis of milk, and to the physiology of the digestive organs; while the second section deals in a practical manner with the feeding of infants, and lays down definite rules for the guidance of the practitioner. The microbes and the different ferments met with in milk are fully discussed, and the description of the transmission of infectious diseases by the pathogenic micro-organisms with which milk has become contaminated, leaves little to be desired.

In the chapters devoted to breast feeding, the author deplores the growing tendency of substituting artificial feeding for breast milk, which, being the natural, is clearly the proper method of rearing children.

The management of dairies upon hygienic principles is very fully considered, and the importance of submitting every cow admitted to those establishments to the tuberculin test is insisted upon.

The author is sceptical of obtaining an absolutely pure milk even when the best sanitary conditions are observed, and recommends the sterilisation or Pasteurisation of milk, preferably by Soxhlet's apparatus; and in his experience Barlow's disease rarely, if ever, occurs in children fed upon milk prepared in this manner. He advises that the milk should be submitted to the action of heat, as soon as possible after being drawn from the cow, as, if more than a few hours have been allowed to intervene, especially in summer, the milk has often undergone lactic acid fermentation, and coagulates when heated.

The plan of feeding children from birth upon pure and undiluted milk, as recommended by Budin, Comby, and other French authorities, is condemned, and the author advises that for the first five or six months the milk ought to be diluted in varying proportions, according to the age of the child, after which time it may be given pure.

After an extended trial, it was found that although some of the children fed upon pure undiluted milk appeared to thrive and grow fat, the greater number suffered from gastro-intestinal disturbances, and in many, manifestations of rickets developed.

The merits of humanised, peptonised, and condensed milk are considered, and the book closes with several chapters devoted to the feeding of congenitally feeble children, and of those suffering from acute disorders, which are full of valuable information, and contain many practical suggestions. G. H. MELVILLE DUNLOP.

Transactions of the Glasgow Obstetrical and Gynæcological Society. Vol. I. Glasgow: James Maclehose & Sons. 1899.

THE Glasgow Obstetrical and Gynæcological Society is to be congratulated on having published a most valuable and interesting volume of *Transactions.* The volume is most elegantly and even luxuriously produced, and it does infinite credit to a Society which is a comparatively young one. It was founded in 1885, under the presidency of the late Professor Leishman, and since then has been an eminent success. There have been ample important papers produced, and many interesting discussions have taken place. Its honorary presidents, besides Professor Leishman, include Mr. Lawson Tait, Sir Spencer Wells, Dr. Robert Barnes, Sir John Williams, and Professor A. R. Simpson of Edinburgh.

The volume of *Transactions* is full of most interesting, short, and able communications on all branches of obstetrics and gynæcology, but besides there are printed in the appendix many valuable contributions *in extenso.* Amongst these several demand special attention, for example, Professor Simpson's presidential address on "The Jubilee of Anæsthetics in Midwifery"—an address teeming with historical facts, and a resumé of the development of anæsthesia of universal interest. Dr. Edgar's paper on "Ectopic Pregnancy" is a most valuable and suggestive one, especially on points of diagnosis and treatment. Dr. Malcolm Black has focussed a year's work at the Glasgow Maternity Hospital in a most interesting manner. Dr. Robert Jardine's paper on ante-partum hæmorrhage, with fifty-one cases of placenta prævia, is a valuable addition to the literature of the subject. Dr. J. W. Ballantyne, with his usual industry and perseverance in the department which he has made specially his own, has written a lucid and careful address on the pathology of ante-natal life.

The reports of the meetings are extremely well given, but the only objection to be offered is that the papers and the subsequent discussion are not recorded consecutively. The whole production is a most successful one, and reflects great credit on a young and flourishing Society.

J. HALLIDAY CROOM.

Die Krankheiten der Harnwege. Von J. C. FELIX GUYON. Translated by Dr. OSCAR KRAUS and DOCENT DOCTOR OTTO ZUCKERKANDL. Vols. II. and III. Wien: Alfred Hölder. 1899.

IN a former number of this Journal (July 1897) the first volume of the German translation of Guyon's work was reviewed at some length, and it is with pleasure that we bring under the notice of the profession the fact that the work is now completed. Vol. II. was published early this year, and well maintains the standard of work done by the translators in Vol. I. It is divided into two parts—the first comprising a complete consideration of the different forms of urinary poisoning, and the second those methods of direct examination of the urethra and bladder other than catheterisation.

Vol. III. is in all respects worthy of commendation. The third volume of the French edition was published about the middle of 1897, and it is greatly to the credit of the authors that the German edition should have been so quickly finished. It is in many ways the most valuable, practically, as it deals with the important subjects of catheterisation, cystoscopy, and anæsthesia, local and general, in urinary diseases. It teems with facts stated in an easily appreciated and attractive form. The authors have carefully followed the French work, and give at the beginning of each chapter a short synopsis of the contents, and further at the commencement of the book a résumé of the whole volume, thus facilitating reference to any special subject. The illustrations are not numerous, but they are well chosen and carefully executed, and in these volumes, as in the first, they are from the original plates. They do not in any way fall short of the originals in their accuracy and delineation. The plate of cystoscopic fields at the end of Vol. III. is very beautiful.

Guyon's work is so thorough and practical in its character, and gives so well the methods of the Neckar School of Paris, of which he has been so long the revered head, that we believe this translation will be received with approbation by all those interested in genito-urinary surgery. At present, as in the past, the Neckar school leads the way in this important branch of surgery, and it is fortunate that its teaching has been brought more widely before the profession, and that those in Germany and in this country, who cannot readily read the French work, have now the opportunity to familiarise themselves with the work of that school.

DAVID WALLACE.

Twentieth Century Practice: An International Cyclopædia of Modern Medical Science. By leading Authorities of Europe and America. Edited by THOMAS L. STEDMAN. Vols. XIII. and XIV.—"Infectious Diseases." London: Sampson Low & Co. Ltd. 1898.

THE first of these volumes commences with a most exhaustive article by Dr. Victor C. Vaughan on "Ptomaines, Toxins, and Leucomaines." All the poisonous products of the saprophytic and infective bacteria are carefully classified, and their action on men and animals indicated. Considering the great difficulty of the subject, Dr. Vaughan is wonderfully clear and readable, and we have no doubt that his article will remain for some time the standard work of reference on the subject for the general practitioner. His section on food poisoning is particularly interesting. But surely it was unnecessary to coin so many new terms for the future confusion of unhappy students who have not been blessed with a Greek education. In no way does the term "sitotoxismus" appear preferable to the more homely "vegetable food poisoning." Surely the writer suffers from some intoxication by medical terms, a condition which perhaps we ought to express by some word like "logotoxismus," or even "technico-logo-toxismus." But apart from this minor defect, there is no cause for complaint in Dr. Vaughan's paper, and the concluding section on leucomaines, and their relation to autogenous diseases, well repays careful perusal.

"Infection and Immunity" are discussed at length by Professor Ernst, who gives all the more recent theories on these subjects, and pays full attention to the question of immunisation by serums and antitoxin treatment. The possible value of high temperature in the natural cure of disease is also indicated more clearly than in some recent works. Of the remaining articles in the volume, "Water-borne Diseases" are treated of by the late Mr. Ernest Hart and Dr. Solomon Smith, the "Incubation of Disease" by Dr. Dawson Williams, and "Smallpox" by Dr. J. W. Moore. This last article is, as is everything written by Dr. Moore, very complete, and abounds in useful references—the part devoted to treatment being particularly full. The red light treatment is given full prominence, and the writer seems to be satisfied that it should be used where practicable. Contributions by two distinguished French writers close the volume, M. Brouardel writing on "Vaccinia," while M. Jules Comby gives an exhaustive account of "Mumps."

The fourteenth volume opens with an admirable paper on "Scarlatina" by Dr. Forchheimer, who devotes more space than usual to the pathology of scarlatinal nephritis. He is, however, a little severe on those of us who believe that a chill may predispose to or cause that condition. He denies that the inflammation of an organ can be caused by external cold, but surely the vitality of a tissue may be so lowered by a chill that its power of resistance to pathogenic bacteria may be greatly lessened or altogether abolished. Many of us have learned by bitter experience that nephritis in many cases is the result of a cold draught of air on the back, and in some hospitals ventilators have had to be blocked up because it was found that the patients in the beds beneath them almost invariably got nephritis. However, it is interesting to note that Professor Forchheimer keeps his patients in bed three weeks, if not to preserve them from cold, at least to secure a constant and gentle action on the skin.

Other points of interest in this article are the emphasis the writer lays on the statement that scarlatina is not infectious in its invasion period, and the fact that he feeds his patients on an ordinary mixed diet during the first week, not commencing a milk diet till after the eighth day. The same writer has also a paper on "German Measles," but there is no particular light thrown upon that obscure disease, and the difficulty of its diagnosis is, we think, somewhat under-estimated.

Those of us who have much to do with the treatment of whooping-cough are naturally always on the outlook for any hints on this subject. The mortality of this disease is sometimes very high; indeed, in the Edinburgh City Hospital it has not been unusual for its death-rate to exceed that of diphtheria, since the discovery of antitoxin has robbed the latter disease of half its terrors. We therefore turn with interest to the suggestions of Drs. O'Dwyer and Norton. These writers give prominence to the method of administering carbonic acid gas by the rectum. The rationale of this treatment is not stated, but it appears to have given admirable results in the New York Foundling Hospital. The arrangements for carrying it out are so simple that the system should be given a trial, especially as there are few diseases in which we are so absolutely helpless as in bad cases of pertussis. The second suggestion worth noting is intubation of the larynx in bad cases. This absolutely prohibits the glottic spasm, which is the cause of the distress of the cough, though the paroxysm is uninterfered with. It would seem, however, to be only practicable in hospitals.

Dr. Rumpf of Hamburg has been entrusted with the articles on "Asiatic Cholera" and "Cholera Nostras," which are very complete. It is gratifying to find that he speaks hopefully of Haffkine's method of inoculation. He also does full justice to the value of the agglutination test, in cases where there is doubt about the bacteriological diagnosis. Another interesting article is that of Dr. Netter of Paris, on "Miliary Fever," a disease at present unknown in this country, but which he identifies with the "English sweating sickness" of the Middle Ages. To close the volume, the editor has been fortunate in securing a paper on "Malta Fever" by Major Bruce, the discoverer of the *Micrococcus melitensis.* This interesting fever is fully described, and its differential diagnosis from enteric fever laid stress upon. The value of serum diagnosis is also pointed out. But what is perhaps the most readable part of the article, is the description of Malta, its water supply and its sanitation.

We have said enough to show that these volumes are likely to take a high place as a work of reference on infectious diseases. There is a good bibliography appended to each article, and a glance at the names of the writers shows how international the work is.

CLAUDE B. KER.

RECENT ADVANCES IN MEDICAL SCIENCE.

MEDICINE.

UNDER THE CHARGE OF

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Hemianæsthesia.

Stein (*Deutsches Arch. f. klin. Med.*, Leipzig, Bd. lx. Heft 1) records the case of a man, æt. 23, who, two months after fracture of the ulna on the left side, experienced various sensory, motor, and trophic disturbances in that arm, followed by left-sided complete anæsthesia, "lalling" speech, squint, anosmia, and atrophy of the left side of the tongue. No good result arising from partial excision of callus at the seat of fracture, the left arm was amputated. The ulnar nerve was found to be inflamed, and to be embedded in the new bony tissue. The removal of the arm was rapidly followed by disappearance of many of the symptoms. The others became less marked. Stein regards the case as one of syringomyelia, arising from a peripheral traumatic neuritis, but his grounds for so doing appear to be rather shadowy.

Naamé (*Rev. de méd.*, Paris, 1897, quoted *Centralbl. f. innere Med.*, July 1898) has noted a case of ataxic astasia abasia with hemianæsthesia.

A case of hemianæsthesia alternans is described by Bernhardt (*Centralbl. f. innere Med.*, 19th Feb. 1898). The patient, æt. 64, previously healthy, suddenly vomited and fainted. On recovering from this, various nervous lesions were manifested, such as paræsthesia in the area supplied by the second part of the left trigeminus, and over the right leg; complete loss of sensation was present over the first-mentioned area; incomplete loss, over the areas of the first and third parts of left trigeminus and over the right leg. No other symptoms were detected. The probable lesion was placed in the pons.

Partial Hemihypertrophy.

Duplay (*Gaz. hebd. d. méd.*, Paris, 1897, No. 45) records an interesting case of partial hemihypertrophy in the person of a man, æt. 40. His parental history was excellent, but only one of his five brothers and sisters survived, three dying in infancy in convulsions, one of phthisis at the age of 19. There appeared to be no connection between the family history and the symptoms presented by the patient. On entry, he was found to be suffering from callous ulceration of the left leg, due to a severe burn three years before. This ulceration was of little importance, except in so far that it drew attention to the marked increase both in bulk and length of the left leg, to the presence of extensive nævi, stretching from the foot to near the perineum, and to various trophic disturbances manifested by the appearance of the surface of the limb, including the aforesaid ulceration. Investigation established the fact that from early childhood his left leg was longer and larger than his right, and grew more rapidly. Measurement gave the excess in length as 10 cms. (3·9 in.) for the whole limb, as 2 cms. for the foot. The circumference of the left thigh was 5 cms., the leg 7 cms., and the foot 2 cms. greater than that of the right leg. Both the bones and the muscles contributed to this excess. The nævi were only present over the left leg, with one on the left side of the scrotum. Elsewhere no trace of nævi could be discovered. The trophic alterations comprised great induration of the skin over portions of the foot, increased development of hair over the skin unaffected by nævi, and patches of yellow skin over the foot. No absolute alteration of the various forms of nervous sensibility and sensation could be precisely distinguished.

Duplay proceeds to analyse the historical references to such a rare case as this, after drawing attention to the co-existence of hypertrophy of a limb and the presence of extensive nævi over its surface. Trélat and Monod (*Arch. gén. de méd.*, Paris, 1869) first noted such cases; while Buzéa (1885), Masmejean (1888), and Saint Hilaire have published accounts of it. The excess in length of the limb on the affected side (generally the inferior limb, sometimes in both) has been found to reach as much as 19 cms. (7 in.), according to Trélat and Monod. Of the various theories advanced to explain the pathogenesis of this affection, Pollosson's lymphatic and Barwell's arterial hypothesis have given place to the opinion that it is entirely nervous in origin. Despite negative post-mortem appearances, the mathematical limitation of the nævi and enlargement to one side only of the median line of the body renders a nervous origin certain.

The prognosis is uncertain, and depends upon the gravity of the trophic lesions. Treatment by elastic pressure, proposed by Holmes, has given good results at the hands of Ædard, but proved unsatisfactory under Dubreuilh. Martin, in 1890, published notes of a case benefited by forcible extension of the sciatic nerve.

IDIOPATHIC ATROPHY OF THE SKIN.

NEUMANN (*Wien. med. Bl.*, 1897, No. 14) noted the occurrence of numerous isolated areas of marked atrophy of the skin over most of the body in a patient of 32 (two years ill), and in another who had first observed the condition ten years before. The origin of the atrophy is undoubtedly nervous; the recorded cases are rare, and little can be argued from the facts up to now observed in them.

PARALYSIS AGITANS.

KRAFFT-EBING (*Wien. klin. Wchnschr.*, 1899, No. 4), referring to the influence of local conditions in the causation of paralysis agitans, and to its invariable commencement in the upper limbs, usually in the right, provided no local injury has occurred causing the first symptoms to appear in the legs, notes that in eighty-eight cases, not traumatic in origin, the right arm was first affected in fifty. Unfortunately, the question of right- or left-handedness was not inquired into. The onset of paralysis agitans is first displayed by the part of the body which has been made most use of, especially for the more complex and delicate operations, this relation between the different parts of the body bearing no relation to the amount of stress imposed upon the same parts in individuals, comparing one with another. Paralysis agitans often follows weakening illnesses, continued mental or physical strain, prolonged emotional disturbances, the disposing cause for its first appearance lying in overstrain of some weaker nerve area, already overworked; or in its actual injury, past or at the time, the less resistant nerves supplying the area yielding to the irritation rising from the injured or faulty spot.

Krafft-Ebing sums up, by the expression of his opinion that paralysis agitans may have a physical origin, and, like trade neuroses, writer's cramp, etc., follow local over-exertion.

ALKAPTONURIA.

ALKAPTONURIA is a symptom of interest rather to the pathological chemist than the clinician, save in so far as it interferes with the evidence of sugar in the urine by reduction of copper salts. The bodies in the urine which are included under one name, as giving rise to the clinical signs of alkaptonuria, are supposed to be at least two, it may be three, in number. Along with various other derivatives of the broken-down proteid molecule, such as, for instance, creatinin, glycuronic acid, pyrocatechin, they possess the property of reducing Fehling's solution. Futcher (*New York Med. Journ.*, January 15, 1898) gives a very full historical account of this condition, with an exposition of its nature; the text to which these are appended being an original case met by him. As he remarks, the chief interest attaching to the subject is the fact of possible fallacy in testing for sugar. Bödeker it was who termed one of the bodies excreted alkapton, from which the generic name is derived. The word alkapton is a compound of alkali, and the Greek for "to absorb greedily."

The positive and negative signs present in the urine of patients the subjects of this complaint are—(1) Deepening in colour on exposure to the air; (2) rapid reduction of cupric salts; (3) peculiar aromatic odour; (4) no reaction to the fermentation test, to Böttger's bismuth reagent, or to phenylhydrazin; (5) no deviation of the rays of polarised light, or, if any, only slightly lævo-rotatory; (6) no evidence of bile pigments; (7) marked darkening of urine on addition of an alkali; (8) normal specific gravity; (9) diminution of uric acid excreted; (10) instantaneous reduction of ammonio-nitrate of silver, with deposition of metallic silver; (11) a temporary bluish green colour, with perchloride of iron. The actual substance present has been identified at different times as alkapton, as above, pyrocatechin (Ebstein and Müller, *Virchow's Archiv*, Bd. lxv. S. 394), protocatechinic acid (Smith, *Dublin Journ.*

Med. Sc., 1882, vol. i. p. 465), urrhodinic acid (Kirk, *Brit. Med. Journ.*, London, 1886, vol. ii. p. 1017), glycosuric acid (Marshall, *Med. News*, Phila., 1887, p. 35), uroleucinic and uroxanthinic acids (Kirk, *Brit. Med. Journ.*, London, 1888, vol. ii. p. 232), and homogentisinic acid (Baumann and Wolkow, *Ztschr. f. physiol. Chem.*, Strassburg, Bd. xv. S. 228), with the formula $C_8H_8O_4$. But the actual body is less important than the fact that if tyrosine is given by the mouth, the characteristic appearance of the urine is intensified. By several observers, indeed, it is regarded as a symptom of intestinal fermentation, in which tyrosine and allied bodies are split up by a special organism.

Moraczewski (*Centralbl. f. klin. Med.*, Bonn, 1896, No. 7) describes the symptoms of alkaptonuria as they were present in a female with tubercular phthisis and peritonitis, in whom the urine, shortly before death, became dark brown on standing, addition of an alkali intensifying it. Homogentisinic acid was separated from the urine, while tyrosine given by the mouth increased the amounts both of this acid and of indican. He regards it as probable that other aromatic bodies are also present.

Hirsch (*Berl. klin. Wchnschr.*, No. 200, Bd. xxxiv. S. 866) notes the case of a girl of 17, in whom a temporary appearance of these symptoms accompanied an acute attack of gastro-enteritis. They very soon disappeared.

Deniges, in the *Journ. de pharm. et chim.*, Paris (6), tome ii. p. 50, gives the details of a rapid method for the estimation of the aromatic bodies which appear to be concerned in the production of alkaptonuria. They should be read in the original.

SURGERY.

UNDER THE CHARGE OF

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REMOVAL OF CYSTIC ADENOMA OF THE PANCREAS.

VON BRACKEL describes this case, which was operated upon by Von Manteuffel. The patient, a single woman of 34, had observed slight pains in the belly between the middle line and left costal margin for two and a half years. These developed especially after meals. A tumour, the size of a goose's egg, was observed in the same situation eighteen months before her admission to hospital. Three months before, she had had an attack of threatening obstruction, with colic and vomiting. The tumour was about the size of a man's head, somewhat movable and tensely elastic, and it was situated in the left mesogastrium and hypochondrium. When the colon was distended with gas, the tumour was displaced upwards and to the left. The diagnosis was doubtful: hydronephrosis or pancreatic cyst. On opening the abdomen, the tumour was invested with peritoneum, and projected from above and to the left downwards between the layers of the mesocolon, insinuating itself between the stomach and colon. The colon and omentum were displaced upwards. After dividing the anterior layer of the mesocolon, the tumour could be separated and brought out at the wound. It was then recognised as a cyst of the tail of the pancreas, and was tapped, giving exit to more than a gallon of clear, yellowish green, slightly opalescent, viscous fluid. The pedicle, represented by the pancreas, was ligatured beyond the tumour, and the latter removed. Convalescence was retarded by an abscess in the lumbar region of the wound, but ultimately the patient made a complete recovery, and has since married and borne two children. The tumour, when examined, proved to be a proliferous cystoma or cysto-adenoma, of which only fourteen have been recorded out of a total of 128 pancreatic cysts subjected to operation. The point of practical importance lies in the fact that the ordinary procedure of opening the cyst and stitching its wall into the parietal wound, would not suffice, as a fistula would probably persist, and the tumour continue to grow. The author inveighs against the tapping of pancreatic cysts in general, and recommends an exploratory laparotomy in order to discover the anatomical relationships, and to be in a position to deal with these by incision or excision, according to the conditions in each individual case.—*Centralbl. f. Chir.*, Leipzig, 14th January 1899.

Perforating Ulcer of the Duodenum.

The diagnosis and treatment of diffuse septic peritonitis caused by the perforation of a duodenal ulcer is made the subject of a report by Dr. M. E. Schwartz. As regards the situation of the ulcer, it is nearly always found in the first part of the duodenum (within 5 cms. of the pylorus in 242 out of 262 cases). In twenty-six cases there was more than one ulcer; in the remainder, it was solitary (85 per cent.). The anterior wall is its favourite seat. It has a much greater tendency to perforate than the corresponding ulcer of the stomach. Collin estimated that perforation takes place in 69 per cent. of the cases, and it usually occurs into the general peritoneal cavity. Preliminary symptoms of disease in the duodenum are exceptional. The clinical features of perforation are similar to those of perforated gastric ulcer: severe pain in the epigastrium or beneath the right costal margin; rapid development and progress of general illness; the temperature rarely rises above 101°, and may remain subnormal; the pulse is quickened; vomiting is frequently absent, especially in the earlier history of the illness; nothing can be learnt by palpation, on account of the early rigidity of the abdominal muscles. The liver dulness is replaced by a tympanitic note. Nothing is passed per rectum. The condition is very rarely diagnosed with accuracy; it is frequently mistaken for appendicitis or obstruction of the bowels. Operative interference has usually been delayed until it is too late. Out of twenty-five cases, only three recovered from the operation, and of these, one died two months later of intestinal obstruction caused by adhesions, and another died six months later from the perforation of a second duodenal ulcer. The only hope of success lies in operating at the earliest possible moment. On account of the frequent accumulation of pus in the right iliac fossa and underneath the liver, one is apt to be misled, and to regard the lesion as appendicital in origin. In view of the very high mortality which attends duodenal perforation, Tuffin recommends that the ulcer should be submitted to surgical treatment before it perforates, and that this treatment had best take the form of a gastro-enterostomy.—*Bull. et mém. Soc. de chir. de Paris*, tome xxiv. p. 3.

THE TREATMENT OF ANEURYSM BY SUBCUTANEOUS INJECTIONS OF GELATINE.

IN aneurysms which are beyond the range of direct surgical interference, and especially those of the thoracic aorta, any method of treatment which is likely to bring about improvement without endangering life, is worthy of serious consideration. Since 1896, when Dastro and Floresco showed by experiment that the coagulability of the blood was increased by the addition of a solution of gelatine, the method has been put into practice by Lanceraux and others, with varying results. Lanceraux maintains that when gelatine is injected subcutaneously, it is absorbed into the blood through the lymphatics, and confers upon the blood a greater tendency to coagulate; and that in a sacculated aneurysm, in which the stream is slow and the wall of the sac uneven, this may result in clotting of the blood within the sac. Lanceraux has employed the method with a fair amount of success in five cases. Boinet, on the other hand, reports a case of aneurysm of the ascending part of the arch of the aorta, in which the patient died from pressure on the superior vena cava and pulmonary artery by a mass of hard clot which had formed in the lower part of the sac; and Barth lost a patient from acute anæmia of the brain, the result of rapid clotting in the left common carotid, in a case in which the innominate artery was already obliterated.

Huchard, while believing in the coagulating power of gelatine solutions, points out the risks which attend their employment in any strength greater than 1 per cent., and at intervals of less than eight to ten days. He has employed the same treatment on a patient with severe and uncontrollable hæmorrhage from the lungs, with marked benefit. Lanceraux recommends a solution of 4·5 grms. of white gelatine dissolved in 200 cc. of a 7 per cent. solution of common salt, carefully sterilised by heat (120° C.), and warmed to the temperature of the body. The injection is repeated once a week. The direct injection of gelatin into the sac of the aneurysm is a dangerous procedure. Lanceraux' views have been adversely criticised by Laborde, Gley, and Camus. Putting theoretical considerations on one side, it appears to be established that subcutaneous injections of solutions of gelatine, along with rest and suitable diet, may bring about rapid, certain, and, on the whole, safe coagulation of the blood in a sacculated aneurysm, and it is to be desired that this recent addition to our therapeutic measures should be more extensively tested and practised.—*Centralbl. f. d. Grenzgeb. d. Med. u. Chir.*, Jena, 2nd January 1899.

CALCULI IN THE REGION OF THE PROSTATE.

M. GUYON made the above a subject of the recent clinical lecture at the Necker Hospital. The patient presented was a man, æt. 58, who had had several attacks of gonorrhœa and who developed stricture, for which an internal urethrotomy was performed in 1878. Since 1891 he has been in the habit of passing instruments on himself, and in January 1896 he hurt himself in so doing. Extravasation of urine ensued, necessitating perineal section, and the insertion of a drainage tube in the bladder. Since then a fistula has persisted, and latterly he has presented symptoms of cystitis. On examination per rectum, crepitus was felt in the prostate, caused by hard bodies moving under the finger. On passing a sound into the bladder, it did not strike anything, neither in the bladder nor in the prostatic urethra; but on passing a curved probe into the fistula, it strikes a stone and then passes on into the bladder. The treatment consisted in a median perineal cystotomy; the calculus was felt with the finger lying in the left lobe of the prostrate, and in a cavity which communicated freely with the prostatic urethra. When extracted, it was found of a yellow colour, and of the size of a nut; four or five smaller ones were also removed. They were all found to consist of phosphate of lime, and of a small amount of organic matter and of ammonio-magnesium phosphate. M. Guyon referred to the rarity of calculi in the region of the prostate, and to the difficulty of exactly localising them. Two types are to be met with—(1) Those in the substance of the prostate, and (2) those in the prostatic urethra, and confined to the latter or projecting also into the bladder. In the former, the pouch or cavity of the prostate in which the stone lies, may or may not communicate with the urethra. Wherever situated, they vary in shape, size, and number. When multiple, they are facetted. When lodged partly in the prostatic urethra and partly in the bladder, they present the characteristic shape of a sandglass, with a constriction, corresponding to the neck of the bladder, dividing it into two segments, of which the larger occupies the greatly dilated prostatic urethra.

The diagnosis is made by rectal examination, and by passing an instrument along the urethra. In exceptional cases, where there is a perineal fistula, information may be gained by probing the latter.

For exploration by the urethra, a gum-elastic instrument with an olivary head is to be selected. It is either arrested by the calculus, or it transmits to the fingers the sensation of friction as it passes over the stone, or it misses the latter and enters the bladder; in the latter case it may touch the stone on being withdrawn. A metal instrument, *e.g.* a sound, is more apt to displace the calculus and may prevent its recognition. The symptoms complained of by the patient are seldom characteristic. The perineal route is to be employed for removing the stone or stones, in preference to that above the pubes, as giving more direct access and better drainage.—*Ann. d. mal. d. org. génito-urin.*, Paris, January 1899.

CATHETERISATION OF THE URETERS IN RELATION TO THE SURGERY OF THE KIDNEY.

A PAPER by Casper on the therapeutic results of ureteral catheterisation gave rise to an interesting discussion at the Medical Society of Berlin, by Israël, Kutner, Landau, Litten, Posner, Richter, Wossidlo, Mainzer, Dührssen, Neumann, and Casper. Among those who joined issue with Casper in his sanguine views as to the curative results of catheterisation of the ureters in pyonephrosis, we find Israël, Kutner, and Litten. The first-mentioned pointed out that the method of treatment so strongly advocated by Casper was only possible under very exceptional conditions. The ureter must be patent. The pus-containing cavities, whether in pelvis, calyces, or renal substance, must communicate freely with one another; the pus must not be too thick to pass through the narrow lumen of a ureteral catheter; there must be no fibrinous or other masses which will plug it; there must be no stone formation, such as Israël has found in one-third of the total cases of pyonephrosis which have come under his observation; cases in which there is a tubercular factor in the causation of the disease, or in which there is perinephric suppuration, must be also excluded,—and this is often quite impossible until the kidney has been exposed by operation. Out of a total of eighty-one cases personally observed and treated by Israël, there were only three in which the method of drainage or irrigation through a ureteral catheter, as recommended by Casper, could have resulted in any benefit to the patient, while in several others the method would have been injurious, either by the introduction of septic infection by means of the catheter or by causing loss of time, during which the condition of the patient would have deteriorated so as to prejudice the results of more direct surgical interference. With regard to the diagnostic value of catheterisation of the ureters, and especially as to the information obtained in cases of proposed nephrectomy of the condition of the other kidney, Israël maintains that it is often inefficient and often misleading. The demonstration of disease in the other kidney is not sufficient to contra-indicate nephrectomy. The absence of abnormal constituents in the urine withdrawn from the other kidney may be no guarantee that it alone will be able to take on the function of both organs. The presence of blood may be due to some damage to the ureter in introducing the instrument, and not to any disease in the kidney under investigation. There is always the risk of conveying infection to a kidney which is healthy. For the other points touched upon in the discussion we must refer our readers to the original article in the *Berl. klin. Wchnschr.*, 9th January 1899.

GONORRHŒAL STRICTURE OF THE RECTUM.

DR. W. BERNDT reports eighteen cases of the above from Mikulicz' clinique in Breslau. Four of the patients were of the male sex. The stricture arises independently of ulceration, and is the result of chronic gonorrhœa, with inflammatory infiltration of the entire thickness of the wall of the rectum. The condition was preceded by an inflammation of Bartholin's gland in five cases. There was an additional history of syphilis in six cases. Mikulicz holds, however, that the majority of cicatricial strictures of the rectum are of gonorrhœal origin, whether one succeeds in finding the gonococcus or not. In five patients the infection of the rectum was favoured by a very relaxed condition of the sphincter ani. The stricture is usually found from 1 to 4 in. from the anus, but the stenosis may extend as high as the sigmoid flexure. The lumen of the stricture feels tough and scar-like, with a smooth lining, the rectum is often firmly fixed to the surrounding tissues. Hæmorrhage is a symptom in nearly 75 per cent. of the cases, and appears to result from ulceration above the stricture.

The treatment consists in regulating the bowels, in employing antiseptic douches, and in dilating the stricture with bougies, which are to be left *in situ* as long as possible. The patient may learn to pass the bougies. Division of the stricture by simple posterior incision was performed upon three occasions, but was not successful. A radical operation was preferred in the majority of cases; it consisted in making a preliminary artificial anus, and then excising the rectum. In two patients the artificial anus was omitted, and one of them died of peritonitis. The artificial anus may be closed at a later period, if the new rectum is found to perform its functions satisfactorily.—*Centralbl. f. Chir.*, Leipzig, 3rd December 1898.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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TORSION OF THE UTERUS.

SCHULTZE (*Ztschr. f. Geburtsh. u. Gynäk.*, Stuttgart, Bde. xxxviii. and xxxix.) has collected thirty-two cases, of which six are recent ones, of this uncommon condition. In fifteen of the cases the torsion was caused by fibroids, while in the remaining seventeen cases the accident was due to ovarian cyst. The puerperal state seems to predispose to the condition, and the author also believes that the resistance offered by the pelvic walls to the growing tumour may lead to torsion of the uterus. The symptoms are those of circulatory obstruction, congestion, hæmorrhage, necrosis of the tumour, and possibly peritonitis. The diagnosis is far from easy, and Schultze suggests that the absence of the ovaries from their normal position, along with the above-mentioned symptoms, should lead to a suspicion of twisting of the uterine body.

TETANUS PUERPERALIS.

KUEHNAU (*Berl. klin. Wchnschr.*, 1898, Nos. 27–29) reports a case of the above. A multipara had a normal delivery, in which no vaginal examination was made. The puerperium was normal till the sixth day, when she left her bed, and also took a vaginal douche. Two days later there was a foul-smelling vaginal discharge, and in other two days she complained of headache and difficulty in swallowing, which gradually increased. On attempting to drink water she became cyanotic, fell to the ground, and was seized with tetanic convulsions, which lasted a few minutes, and returned when again attempting to swallow. The jaws were firmly fixed, the face distorted, and speech or the introduction of food were impossible. There was tenderness on abdominal pressure. The symptoms increased, and the patient expired while attempting to ask for water.

In this case the muscles of the pharynx and larynx were chiefly affected, resembling a case reported by Heyse, in which attacks were produced only on swallowing or attempting to do so.

Post-mortem.—Puerperal endometritis was found, and thrombosis of the veins adjoining the uterus.

Bacteriological examination revealed the presence of tetanus bacilli in the endometrium, the spleen, and the blood, but none were found in the heart or kidneys, which both showed degenerative changes. Besides the tetanus bacilli, streptococci and colon bacilli, as well as other micro-organisms, were found in large quantities.

THE TREATMENT OF CHRONIC PUERPERAL INVERSION OF THE UTERUS.

MÖLLER (*Centralbl. f. Gynäk.*, Leipzig, 1898, No. 46) reports a case where the placenta was torn away by a midwife, followed by severe abdominal pain, vomiting, and hæmorrhage. Three months later the patient was admitted into hospital, exhausted from continuous hæmorrhage. There was found complete inversion of the uterus, which manual reposition, under chloroform, failed to return. A month later a transverse incision was made through the posterior fornix, and a vertical one through the posterior uterine wall, as performed by Küstner. Attempts at reposition still failing, the vertical incision was extended through the whole length of the posterior uterine wall; both halves of the split wall were fixed and folded away from each other, so as again to meet on the anterior aspect of the uterus. In this way the posterior uterine wall came in contact with the anterior vaginal wall. In this position the sutures were introduced from below upwards; the uterus was then pushed up through the opening in the posterior fornix, so that the anterior uterine wall, which in the vagina was in contact with the posterior wall, was directed forwards, the uterus being now anteverted. The vaginal opening was then closed.

The stages of the operation are represented by diagrams.

Möller discusses the usual methods of treatment, and points out the difficulty of reducing the inversion after involution of the uterus is complete, when the muscle regains its tone, and the inversion ring, at first lax and wide, becomes hard and undilatable. Manual reposition is tried first, both in acute and chronic cases; in the latter it is generally useless, requiring continuous elastic pressure by the colpeuryter alone, or combined with iodoform tampons (Hofmeier). In those cases where continuous elastic pressure failed, up till 1893, it was usual to amputate the uterus, although Sims in 1866 suggested incisions into the uterine wall, to overcome difficulty in reposition. The introduction of Küstner's method of reduction, by vertical incisions, into the posterior uterine wall, has led to the successful treatment of cases which would otherwise probably have required hysterectomy. Six cases are recorded treated by this method, with four successes. These are given in detail.

Möller advocates Küstner's operation in cases irreducible by other means, and if this fails, as in the two cases of the six recorded, he would perform the operation described in the above case, before resorting to amputation or hysterectomy.

AN UNUSUAL COURSE OF THE URETER IN A CASE OF PELVIC TUMOUR.

RÜHL (*Centralbl. f. Gynäk.*, Leipzig, 1898, No. 36) comments on the accident of ligature or injury of the ureter in the removing of large pelvic tumours by laparotomy or vaginal hysterectomy, most frequently the result of unusual fixation or anatomical relations. He describes a case observed during vaginal extirpation of a myomatous uterus and intraligamentary fibroma. After dividing the mucous membrane covering the cervix, Rühl found that the bladder could not be pushed up, on account of being fixed to the tumour in the broad ligament by a band, which on dissection was found to be the ureter surrounded by connective tissue, which appeared to have penetrated the tumour for a distance of 7 cms. He refers to a similar case recorded by Chrobak, where the ureter had to be dissected out of a fibroid, through which it had extended a distance of 9 cms. As the ureter can only be pushed out of its course, and not penetrate such tumours, Rühl explains its unusual course in the above cases, either as a commencing fibromatous condition of the ureter (most improbable), or from the tumour originating in chains of small fibroids, which subsequently fusing together accounted for the appearance of direct penetration of the tumour.

Rühl further points out the impossibility of passing a catheter into the ureter in such cases, and emphasises the importance of carefully palpating all bands felt passing between the bladder and broad ligament tumour before passing and tying a ligature.

THERAPEUTICS.

UNDER THE CHARGE OF

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QUININE IN MALARIAL HÆMOGLOBINURIA.

WOLDERT has contributed a long article on this subject (*Phila. Med. News*, 1898). He states that for many years the discussion regarding the use of quinine in malarial hæmoglobinuria has lain dormant, the majority of physicians of large experience being in its favour. Bastianelli and Koch have, however, again raised the question, and in the minds of others there seems to be an impression that quinine may be the cause of the hæmoglobinuria, and that its administration is distinctly productive of harm. Tomaselli and others have recorded cases in which hæmoglobinuria, occurring during an attack of ague, was attributed to the quinine; and Karamitza speaks of a case in which it could be produced in a student after giving 5 grs. Plehn, Richardson, and others hold to the opinion that quinine may cause hæmoglobinuria, while Stendel and Küchel contend that it checks its development. It is admitted that this action is extremely rare in Italy, but Bastianelli states that it may occur during an actual attack of ague, or afterwards, if quinine be given, and that extremely small doses are sufficient. If hæmoglobinuria occur during a malarial paroxysm, and parasites are found in the blood corpuscles, Bastianelli holds that quinine, nevertheless, should be given; if no parasites are found, it should be withheld. It is known that very large doses of quinine have been given without affecting the urine—such as 600 grs., 180 grs., and more, in short periods of time. In fatal poisoning cases from very large doses, hæmoglobinuria has been absent. In Algiers and the Southern United States experience is against its occurrence, and Anders states that malarial hæmoglobinuria disappears when quinine is freely administered. Osler and Thayer say that quinine hæmoglobinuria is unknown in the neighbourhood of Baltimore; nor has it been met with in Cuba during the recent war. Further evidence is given by Woldert at some length, his conclusion being that quinine is curative in malarial hæmoglobinuria, and that it does not tend to bring on or to aggravate such a condition, except in a few individuals.

Formalin in Sweating of the Feet.

Orth, in 1896, recommended weak solutions of formalin in this condition. Gerdeck (*Therap. Monatsh.*, Berlin, Dec. 1898) recommends, however, that it should be used in much stronger form. After washing with a 3 or, better, 5 per cent. solution in water for several days, the skin becomes hard, and the secretion is very much diminished. This is continued according to the effect produced, and improvement lasts for several months. In severe cases a 10 per cent. solution may be employed; but it has to be used very cautiously, as it causes the epidermis to peel off.

Dermatitis from Boric Acid and Borax.

R. B. Wild (*Lancet*, London, 7th January 1899) records the case of a man to whom 10 grs. boric acid was administered thrice daily for a bladder affection, and in whom it caused baldness and a skin eruption. After taking it for about six weeks, he noticed that the hair on his head was falling out and that the scalp was red and scaly. The hands and forearms became red, slightly swollen, and presented scaly patches on the flexor surfaces. This disappeared when the boric acid was discontinued, and simple local remedies applied. Later the boric acid was resumed, when the eruption again appeared in a more aggravated form, and widely spread. There was great swelling and desquamation, and the hair over the body was affected. Recovery again took place on discontinuing the acid and applying soothing applications, while the hair grew again.

Another somewhat similar case is described in which boric acid and borax were given for epileptic fits. The literature of the subject and other recorded cases are noticed briefly.

PATHOLOGY AND BACTERIOLOGY.

UNDER THE CHARGE OF

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Hypertrophic Tuberculosis of the Colon.

Tubercle is very variable in its manifestations, and often comes clinically to resemble other diseases. The ordinary common form of tuberculous ulceration of the bowel is simple enough, but Bezançon and Laponite (*Presse méd. belge*, Bruxelles, 10th and 18th May 1898) have drawn attention to another form affecting the colon, and mainly its two ends, the cæcum and rectum, causing considerable thickening of the wall of the bowel from diffuse infiltration, and to narrowing of the lumen. Clinically, they cause obstruction, and may simulate stenosis from other causes, or resemble new growths. In one of their cases the rectum was excised for carcinoma, and it was afterwards discovered to be affected with tubercle. When microscopically examined, there is seen to be tuberculosis mingled with much chronic inflammatory thickening. The mucous membrane is replaced by tuberculous granulation tissue, which spreads diffusely, invading or replacing the muscular coat, and setting up much fibrous change in the surrounding fat. Bacilli are very difficult to find. Their paucity and also a probable attenuation of their virulence may account for the nature of the lesion, as they are only able to produce an attenuated form of tuberculosis. Laponite showed that with his four cases; there were altogether nine cases recorded of tuberculous inflammation of the rectum resulting in stenosis. Benoit (*Gaz. d. hôp.*, Paris, 2nd and 9th April 1898) has studied carefully chronic tuberculosis of the ileo-cæcal region, and shown that it chiefly attacks males towards middle life, and may give rise to conditions which clinically resemble typhlitis or appendicitis, or still more frequently a tumour growth.

Retroperitoneal Cyst simulating Pancreatic Cyst.

Sarwey (*Centralbl. f. Gynäk.*, Leipzig, April 1898) described a case in a female, æt. 11. It was a large fluctuating tumour in front of the transverse colon. It was believed to have originated in the pancreas, chiefly from its anatomical position. It apparently did originate in the region of the pancreas, but not from it, as the chemical examination of the contents showed. These looked like milk, both to the naked eye and microscopically, and were found to resemble lymph, being devoid of pancreatic ferment. It had thus originated in the peritoneum, and forced its way through the gastrocolic omentum, so that the greater part of it lay in front of the transverse colon. It was successfully and without great difficulty removed, and the patient made a complete recovery.

A CHYLOUS CYST OF THE ABDOMEN.

JOPSON (*Proc. Path. Soc. Philadelphia*, 1st November 1898) records the case of a female, æt. 50, who had suffered from a tumour in the abdomen for two years. It had, with occasional remissions, steadily increased in size, and she had gradually lost flesh and strength. There was no history of vomiting or jaundice. Examination of the abdomen revealed a tumour in the right hypochondrium about the size of an orange, which was freely movable, apparently not connected with the liver. It gave a sense of fluctuation. It was thought at first to be a tumour of the kidney. Operation disclosed the presence of a cyst occupying the retroperitoneal space, and springing from a position corresponding to the first lumbar vertebra. It projected at first forwards, and then to the right. The right kidney could be felt behind it, and entirely free from it. The cyst was incised, and about 200 c.c. of milky fluid was withdrawn. The patient died of cardiac failure on the third day after operation. On post mortem examination the cyst was found collapsed around the drainage tube. The peritoneum was readily stripped from the cyst, which could be traced backwards to its origin opposite the first lumbar vertebra. The receptaculum chyli could not be seen, but several lymphatic radicles could be traced to the cyst from below. The cyst measured about 3 in. in diameter in its collapsed condition, and was lined by a smooth membrane, endothelial in nature. The fluid from the cyst was alkaline, and filled with fat globules. It resembled milk, but coagulated spontaneously. It had no digestive action on either albumin or starch, but gave the test for albumose. He regarded the origin of the cyst as probably lymphatic, either the dilated receptaculum chyli or one of the lymphatic ducts.

SUBDIAPHRAGMATIC ABSCESS CONSEQUENT UPON PERFORATING APPENDICITIS.

SPILLMAN (*Presse méd.*, Paris, 7th September 1898, p. 138) relates the case of a man, æt. 17, who, after a long ride, began to complain of pains in the abdomen, diarrhœa, vomiting, a rigor, and dyspnœa. Physical signs indicating diaphragmatic pleurisy appeared, and a puncture was made in the eighth intercostal space, but no fluid was found. The abdomen became swollen, and the region of the cæcum painful. His symptoms gradually became worse, and he died after an illness of about eighteen or nineteen days. On post mortem examination, a large abscess, containing about 2 litres, was found between the right lobe of the liver and the diaphragm. It had not penetrated the latter, though it had produced considerable thinning in places. In the peritoneal cavity, close to the cæcum, a small fæcal mass was found. The appendix was large, and perforated a little distance from its base. The terminal part was of a violet hue. The cæcum was surrounded by false membrane, and behind it and the transverse colon there was an abscess holding about 250 to 300 grms. of pus. A fistulous track led from it to the subdiaphragmatic abscess.

THE PATHOGENIC RÔLE OF MICROCOCCUS TETRAGENUS.

CARRIERE (*Presse méd.*, Paris, 26th October 1898, p. 249) gives particulars of a case in which interesting and important lesions, due to this organism, terminated fatally, after six months' duration. The infection had entered by the bronchi, and determined an intense right-sided bronchitis. After an interval of eleven days, pulmonary congestion followed, and persisted for nearly a month. Presently a serous, and then a purulent pleurisy appeared, which was peculiarly persistent, being uninfluenced by the usual antiseptic remedies until its fatal termination. There was mostly considerable fever, the temperature oscillating greatly, and rising occasionally as high as 104° F., and rarely falling to normal, except for a few days preceding death, when it was subnormal. There was also albuminuria, which he attributed to the toxins of the *Micrococcus tetragenus*. This organism was found in the sputum, in the fluid drawn from the lung by puncture, and in the pus within the pleural cavity. He considers that the particulars of this case strongly support the view that this organism is not an ordinary harmless saprophyte, but an active pathogenic microbe.

THE BONE MARROW OF CANCER PATIENTS.

VILLY (*Journ. Path. and Bacteriol.*, Edin. and London, Jan. 1898) has made some observations upon cancer cases, which negative many of Snow's conclusions. He has given particulars of the bone invasion, and the implication of the bone marrow, in two cases where the primary cancer was present in the breast, and he has given shorter notes upon other thirteen cases of all kinds of primary cancer. He believes that multiple medullary (bone marrow) cancerous nodules occur as a result of general metastasis, along with wide dissemination of growths in the viscera, and never by the lymph channels, as Snow believes to occur in the humerus and sternum in breast cases. Further, that they have no special mechanism of production or significance in the pathology of recurrence after operation, and that softening of the bones may occur when there is no medullary infection present at the seat of softening, being probably due to a fault of nutrition. He believes that such cancerous infection of the bone marrow is not the main cause of cancerous cachexia, and that the bone and other pains of cancer patients may have several causes, among which secondary tumour formations is not the common one.

PECULIAR BODIES FOUND IN THE CENTRAL NERVOUS SYSTEM.

EDSALL and Sailer recently observed (*Proc. Path. Soc.*, Philadelphia, 15th Feb. 1898) peculiar connections in two cases, which, while they in some respects resembled the usual types of degeneration, do not respond to all the characteristic tests for any one. In the one case (tuberous gliosis) they were found within small papillary elevations in the floors of the lateral ventricles, consisting of dense masses of irregular fibres. The bodies form rounded irregular masses, sometimes measuring 1 mm. in diameter, often distinctly concentric in structure. They are not apparently connected with the blood vessels, as the tissue in which they lie is almost devoid of blood vessels, and is usually entirely without cells. The tissues were hardened in Muller's fluid, and unstained sections showed the bodies as ill-defined homogeneous masses, paler than the surrounding tissue. After staining with iodine solution, they remained pale, but became more distinct by contrast with the partially coloured surrounding tissue. They stained a deep blue or brown with different hæmatoxylins, and a clear red with both Van Giesen's and Rosin's stains. They stained a deep red with gentian-violet, but failed to give the waxy reaction with iodine and sulphuric acid. Peculiar bodies have also been described in other cases of tuberous gliosis. Bruchner found large glassy round cells, between which were chalky deposits, and Pozzi found colloid-looking masses in the midst of the neuroglia. In the other case (tubercular meningitis) there was a large area of degeneration in the left internal capsule, involving the optic thalamus and caudate nucleus, etc. In the midst of this area numerous round-like masses were found, which did not stain with iodine alone, or combined with mineral acids, but stained a faint bluish purple with Delafield's hæmatoxylin, and a bright red colour with gentian-violet. Thionin stained them a reddish violet, but they did not stain with Van Giesen's or Rosin's stains, or with carmine. Dagonet has figured very similar bodies, and regarded them as examples of hyaline degeneration. They seem to be distinguished by their staining reactions from hyaline, colloid, and waxy degenerations; and the authors are inclined to look upon them as degenerative products, situated chemically somewhere between waxy and hyaline substance, but not exactly identical with either.

A MINUTE MICRO-ORGANISM IN CASES OF PROGRESSIVE PORTAL CIRRHOSIS.

PROFESSOR ADAMI (*Montreal Med. Journ.*, July 1898) has made observations upon a minute bacillus, found in the ordinary hobnailed liver, which are likely to attract considerable attention. At the recent meeting of the British Medical Association in Edinburgh, he described it more carefully in a paper read by Professor Osler. It closely resembles that found pathogenic in the infective cirrhosis of cattle. It is somewhat polymorphic, being seen as a minute micrococcus, chiefly in pairs (diplococci), with a faint halo around them, or longer elements, more like diplobacilli. Many of the forms resemble closely minute editions of the gonococcus. Since certain of the forms are distinctly bacillary, he considers that the organism should be placed among the bacilli. The following method of staining gave him satisfactory results. The sections cut with a freezing microtome are placed for a minute or two in a weak solution of acetic acid, washed in water, and placed in absolute alcohol. They are then transferred to a solution of methylene blue in anilin oil (a half-saturated solution), and left for half an hour. They are then placed for a quarter of a minute into a mixture of two parts of anilin oil to one part of xylol, then rapidly into xylol and mounted. Many of the microbes are probably dead. Many are seen within the liver cells.

THE BACTERIOLOGY OF THE SIMPLE POSTERIOR BASIC MENINGITIS OF INFANTS.

THIS is a distinct disease occurring chiefly within the first year of the child's life. Still (*Journ. Path. and Bacteriol.*, Edin. and London, May 1898, p. 147) has given a study of the bacteriology of eight consecutive cases, which goes so far to prove that it is a specific disease caused by a particular micro-organism. He cultivated it artificially, and gives photographs and photomicrographs of it. It is a diplococcus, which is almost identical with the diplococcus described by Weichselbaum and Jaeger. The disease is a sporadic form of epidemic cerebro-spinal meningitis. The periarthritis which occasionally complicates the simple posterior basic meningitis of infants is due to the same diplococcus that is found in the meningeal exudation.

ON PSEUDO-TUBERCULOSIS.

MUIR (*Journ. Path. and Bacteriol.*, Edin. and London, May 1898, p. 160) has contributed a valuable paper on this subject, which gives the result of a large number of observations and experiments. His results specially refer to birds, but he also gives the results of experiments upon rodents. The pseudo-tubercle bacillus is of small size, 1·5 to 2 μ in length, and about a quarter of that in thickness. Shorter forms, like cocci, and longer forms, up to 5 μ, may occur. They are met with singly, in pairs, in short chains, and often grouped together into dense masses. They stain readily with basic dyes, but are easily decolorised, hence Gram's method is inapplicable. The bacillus is easily cultivated upon gelatin or agar, or on most other moist media, at ordinary room temperature, though more rapidly at incubator temperature (37° to 38° C.). Growth takes place both in aërobic and anaërobic conditions, though it is more vigorous in the former. It is visible in twenty-four hours in the gelatin stab culture as a whitish line, which increases in thickness, the deeper part consisting of a row of spherical white colonies. Usually on the third day a delicate surface growth forms a round disc of transparent appearance around the puncture. The growth is closely similar to that of the typhoid bacillus. The deeper part of the growth becomes brownish yellow, and the superficial part more opaque. A stroke culture on agar gives a whitish layer of growth, which is somewhat translucent. In milk it grows readily, without producing any visible change. On potatoes the growth is always slow, and sometimes does not take place at all. In small singing-birds it causes lesions, which are chiefly seen in the spleen and liver, the former being practically always affected in an extreme degree. It is swollen to many times its normal size, and studded with numerous yellow nodules, the largest of the size of a large pin-head, the smallest being just visible. Congestion and hæmorrhage may be present around the nodules. The liver shows them in smaller numbers usually, and the lungs may contain a few. These nodules are proved microscopically to consist almost entirely of masses of bacilli. Their distribution is by the blood vessels, but they have remarkably little effect upon the surrounding tissues, and thus differ widely from the tubercle bacillus. When injected subcutaneously, or by feeding with pure cultures, the disease is reproduced; in the former, owing to the severity of the local lesion, death takes place when the general infection is at an early stage; in the latter, there is a complete reproduction of the disease, as it occurs naturally. His experiments upon rodents were made upon guinea-pigs, rabbits, and white mice, by means of intraperitoneal, intravenous, and subcutaneous injections. Feeding experiments have so far given no success. The lesions produced differed considerably from those of birds, those following subcutaneous injection being much more localised, of a subacute or chronic character, and with a tendency towards early caseation. In the internal organs the differences are even more striking; for while in birds there occurs an enormous growth of bacilli, in masses within the vascular system, attended by comparatively little change in the surrounding tissues, in rodents there occurs a well-marked tissue reaction, accompanied by necrosis of the cells of the organ, whilst the bacilli gradually diminish in number. In all animals used, the spleen and liver are the organs in which the lesions specially occur. He believes that this pseudo-bacillus is the same organism as the pseudo-bacillus of Nocard, Pfeiffer, and others.

PHYSIOLOGY.

UNDER THE CHARGE OF

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HISTOLOGICAL CHANGES IN THE MAMMARY GLAND DURING THE SECRETION OF MILK.

BEFORE giving an account of some recent work that has been done on this subject, it may be as well to describe shortly the different theories that have been held as to the nature of the process of milk secretion.

Virchow, in his "Cellularpathologie," expressed the view that the secretion of milk was a form of fatty degeneration in the epithelial cells of the mammary gland acini. With the death of the cell, the fat globules that had formed in it became free, and went to form the milk globules. He regarded milk secretion as similar to sebaceous secretion, believing, therefore, that there was more than a single layer of cells lining the acini in the mammary gland. This view had to be given up after it was shown that there was only a single layer of cells lining the alveoli. In 1866, Stricker drew attention to the important fact that the colostrum corpuscles were amœboid at body temperature, and that they could discharge the fat droplets from their cell protoplasm. He believed that these cells were derived from the epithelial cells of the acini, and that the milk globules were therefore primarily derived from the latter.

Langer believed that the epithelial cells in all probability did not succumb during lactation, but that the milk was a true secretion of these cells. Rauber, in 1879, brought forward the view that leucocytes wandered through the epithelium, underwent fatty degeneration, and so furnished the milk. He was the first to describe the infiltration of the interstitial connective tissue with leucocytes, and their appearance also in the deepest portions of the tubules, where they were present either in unaltered form or with swollen cell bodies, containing at first small, then large, fat globules.

Heidenhain, in Hermann's "Handbuch," and Partsch, in his dissertation published in 1880, denied absolutely that the leucocytes played any part in the formation of the milk, regarding the secretion entirely as the result of the activity of the epithelial cells. They described certain alterations in the form of the cells during activity: for example, their enlargement until they appeared as very tall cylindrical cells with rounded free ends projecting far into the lumen of the alveoli. During secretion, the inner projecting part of the cell with the fat contained in it was discharged into the lumen. The cell protoplasm surrounding the fat droplets was dissolved in the milk, while the nuclei, if any happened to be present in the discharged portion of cell, also passed into the milk and were dissolved. They regarded this solution of the nuclear matter as the mode in which the caseinogen of the milk was formed. They saw no signs of mitosis, as one would expect to find, in order that the discharged nuclei might be replaced. Neither did they observe direct division of the nuclei. That the form of the epithelial cells was not due to the amount of distension of the alveoli, they showed from the fact that both narrow and distended alveoli showed tall as well as flattened epithelial cells. This solution of the nuclei was denied by various workers, especially Bizzozero and Vassale.

Michælis, in a very recent paper (*Arch. f. mikr. Anat.*, Bonn, Bd. li. S. 711–747), sums up in a short résumé what he regards as points which have been settled:—

(1) *That the secreting epithelium of the mammary gland is in a single layer. During lactation there are no, or at least exceedingly few, signs of mitosis.*

(2) *The fat globules are formed in the epithelial cells, and are discharged into the lumen of the alveoli.*

The points which he regards as still doubtful, and which he goes on to discuss in his paper, are grouped together in the following questions:—

1. Is the alteration in the form of the epithelial cells, dependent on their secretory activity, or is it due simply to degrees of distension of the alveoli from more or less milk being present?

2. Is there a breaking down of the nuclei of the epithelial cells, and, if that be the case, how are these nuclei replaced?

3. What part do the leucocytes play in the milk secretion?

4. Is the fat which forms in the epithelial cell a degeneration or secretory product of that cell?

Michælis first describes the minute structure of the secreting gland in a pregnant guinea-pig (embryos about 1½ cm. long). The epithelial cells were cubical, or rather flatter even than that, and always distinctly arranged in one layer. Their nuclei were rounded or oval, often, however, irregular in shape. No mitotic figures were to be seen. In the lumen of the alveoli there were very numerous cells, each with a round isolated or lobulated nucleus, often smaller than the nucleus of an epithelial cell, now and then larger, however. The cell body sometimes was in the form of a narrow zone around the nucleus; frequently, however, the cytoplasm was greater in extent, and the cell contour was often very irregular. These cells were undoubtedly leucocytes. In the interstitial tissue there were numerous lymphocytes, and also between the epithelial cells. There were also eosinophilous cells, with two or three small round nuclei, present in large numbers in the interstitial connective tissue. He stained the granules in these cells in different ways, in order to make certain that they were really eosinophilous in type.

The milk secreted by pregnant guinea-pigs was also carefully examined, with the following results:—When colostrum cells and leucocytes were present together, all possible intermediate forms could be made out between them. The former are amœboid, possess a nucleus, and the cell body more or less loaded with fat. They arise from the uninucleated lymphocytes which take up milk globules, and not from a fatty degeneration of the leucocytes. The polymorphic leucocytes break down, the broken-down chromatin either dissolving or being taken up by the colostrum cells. Michælis saw no sign of a wandering of the colostrum cells backwards into the lymphatics. He also found in the colostrum milk a granular proteid, which was easily soluble in potassium hydrate solution, and was not blackened with osmic acid.

He next examined the glands of three guinea-pigs that were killed a few hours, up to one day, after parturition. In one animal he found here and there signs of mitosis in the epithelial cells. Fat globules were present in the epithelial cells, most markedly towards the free ends, never close to the membrana propria. The protoplasm was densely filled with coarse granules that had an affinity for the acid anilin stains. The nuclei varied in size and shape, the larger being irregular and very pale in appearance. All kinds of leucocytes, with one or more fat globules in their cytoplasm and different types of colostrum cells, were present in large numbers in the lumen of the alveoli. There was a marked infiltration of the interstitial tissue, with lymphocytes and eosinophilous cells. He next examined the mammary glands after the colostrum stage had passed. The interstitial tissue had now diminished greatly in amount, and the eosinophilous cells and lymphocytes had now practically entirely disappeared from it. Very few leucocytes were to be seen in the lumen of the alveoli and ducts, but accumulations of free nuclei were frequent. These nuclei had all the appearances of having been derived from epithelial cells. Michælis noticed shortly after parturition direct division of the nuclei of the epithelial cells of the alveoli. The forms of the epithelial cells were mainly cubical and long cylindrical. During lactation the cells pass through the following forms:—Cubical, cylindrical (papillated) cubical, and lastly, flattened.

The question as to the mode of formation of the fat is now discussed by the author. He merely disproves the old idea that it is a degenerative process, especially by referring to the gradual appearance of fat globules in the epithelial cells at their free ends, and the removal of those portions, while the rest of the cell lives and secretes afterwards.

The fact that during the colostrum period practically no nuclei of the epithelial cells break down, and that in colostrum milk a much smaller amount of caseinogen is present than in ordinary milk, leads one to believe that the nucleo-albumin of the milk arises from the nuclei of the alveolar epithelial cells.

If one removes the young animals from the mother, then one can examine the glands in the involution stage. Here the author found that at the outset the secretion was again of the colostrum type. Eosinophilous cells appear suddenly again in the interstitial tissue, leucocytes wander through the epithelium, break down in the alveoli, or become colostrum corpuscles.

It does not matter at what time lactation be interrupted, the same alterations appear during the involution period.

SOME MICRO-CHEMICAL REACTIONS.

SAINT-HILAIRE (*Ztschr. f. physiol. Chem.*, Strassburg, Bd. xxvi. S. 102) refers to certain micro-chemical reactions which he studied under Kössel. He, in the first place, tried to fix uric acid in the tissues in the form of the cuprous urate. The method which he employed was the following:—The preparations were hardened in alcohol, imbedded in celloidin, and cut in the usual way. The sections were then placed in a 5 to 10 per cent. copper sulphate solution, and then for one or two minutes in a boiling saturated solution of sodium bisulphite. In this way the moderately insoluble cuprous urate was formed. The preparations were carefully washed and then treated with ferrocyanide of potassium solution. In all cases the uric acid deposits were stained red. All the alloxur bodies (uric acid and the nuclein bases) give this reaction. The nuclear network was also stained red. This is due to the fact that many nuclear substances are precipitable with cuprous oxide, as, *e.g.*, histon and protamin. Saint-Hilaire next tried the action of copper sulphate in alkaline solution (for details, the paper may be consulted) on sections of different organs. In this method the copper salt acts as in the biuret reaction. The nuclei were, in the majority of cases, stained a bright pink. After fixation, if the sections were subjected to the action of boiling water or weak acids, the substance or substances which gave this reaction were removed.

Alkalies, especially ammonia, intensify the stain. Everything points to the probability that the nuclear constituent which gives this reaction is nucleo-histon.

DISEASES OF THE EYE.

UNDER THE CHARGE OF

GEORGE A. BERRY, M.B., F.R.C.S.Ed.,
OPHTHALMIC SURGEON, EDINBURGH ROYAL INFIRMARY;

AND

W. G. SYM, M.D., F.R.C.S.Ed.,
ASSISTANT OPHTHALMIC SURGEON, EDINBURGH ROYAL INFIRMARY.

The Ocular Manifestations of Leprosy.

All authors insist on the frequency with which the eye is attacked in leprosy, particularly in the tubercular form of the disease. The skin of the lids is affected much as the skin of other parts of the face, but ulcers of the lids are rare. Jeanselme and Morax find—and here they differ from some other observers—that the conjunctiva is but rarely infected primarily, and that the conjunctival sac does not contain a large number of Hansen's bacilli. The chief seat of primary ocular leprosy is the anterior segment of the sclerotic; in the cornea, leprous infiltrations are of two types,—one in which a small mass forms in the corneal substance, resembling a tumour; the other and more common, in which a keratitis almost indistinguishable from interstitial inflammation occurs. The iris is somewhat frequently attacked, usually in the form of an iritis, but the two authors from whose paper we are quoting have seen a condition hitherto undescribed, in which the surface is speckled with a number of small grey dots, suggestive of, but more minute than, the miliary granulations seen in tubercular iritis. Invasion of the ciliary body and anterior portions of retina and choroid are also frequent, though clinically, on account of the opacity of the cornea, they are difficult of diagnosis. There seems to be no doubt that the infection of the globe is always of endogenous origin; the foci in the anterior part of the sclerotic are certainly of embolic mechanism.—*Ann. d'ocul.*, Paris, November 1898.

Ocular Troubles in Diabetes.

These are both numerous and frequent, and some of them are of great importance from a prognostic point of view; in a recent paper Dianoux deals with three of these. In regard to *cataract*, he is doubtful whether in senile patients, diabetes has any more than a predisposing action, and finds the prognosis after operation in such persons to be not materially worse than in the non-diabetic of similar age. This is not true, however, of the juvenile patient, who is apt to die of pulmonary apoplexy about the seventh or eighth day after operation; and if he escapes this fate, to die of his malady in some twelve to fifteen months. Retinal *hæmorrhages*, says he, though somewhat too confidently, are unknown in young patients, and among the older persons affected by this, one always finds other symptoms of arterial degeneration; many of them are liable to a form of retinitis resembling that of Bright's disease, though not exactly the same, and in truth these patients have albuminuria as well; in such persons he considers the expectation of life to be not longer than six to twelve months. Central *scotoma* in diabetics has a very evil signification as regards prognosis; unless it be merely an accidental occurence of toxic amblyopia in a person whose resisting power against nicotine is lowered by diabetes, the onset of coma is to be expected before long. As far as concerns the last of these points, at least, all observers are practically agreed.—*Ann. d'ocul.*, Paris, October 1898.

BACTERIOLOGY IN CONNECTION WITH EYE DISEASE.

THREE papers have been published very lately, dealing with various eye diseases in their relation to bacteriology, all of which are of considerable interest. Morax and Petit, to take theirs first, deal with certain of the conjunctival inflammations; they say there are three well-defined forms of conjunctivitis, each dependent on a special organism for its cause, implantation of which bacillus will infallibly lead to the development of the same disease in a fresh eye; of these two, namely, acute contagious and subacute conjunctivitis, are peculiar to the eye; but the third, gonorrhœal ophthalmia, may affect other membranes. Of these three forms the causative organisms are Weeks's bacillus, a diplococcus, and the gonococcus respectively. It is very interesting to note the differing relations which microbes bear to mucous membranes in general; indeed, a classification of them based on this consideration is quite feasible; they divide themselves into three groups: In the first may be placed those which are conveyed from mucous membrane to mucous membrane, require no other condition than their mere presence to set up inflammation there, and are not found on normal membranes; such are gonococcus, Weeks's bacillus, and diplo-bacillus. The second group includes those which are habitual inhabitants of mucous membranes, and which are capable of setting up inflammation, but only in presence of certain conditions not as yet fully understood; such are pneumococcus and certain streptococci. The third group contains those which are able to grow and set up inflammation only when the soil has been prepared by another infection; such are the diphtheria bacillus, staphylococcus, and streptococcus. Thus, to put the matter in another form, deposition upon a normal mucous surface of a culture of the first group will set up the corresponding form of inflammation; a culture of the second may or may not do so; while a culture of the third will certainly not do so. It appears, too, that, apart from mucous surfaces, microbes of the first group fail to find the albuminous substances necessary for their subsistence;

if, therefore, we were able to prevent the contamination of one mucous membrane by another, we could, theoretically speaking, stamp out the diseases due to their action. So far as our present knowledge carries us, no classification of conjunctivitis depending upon the demonstrated presence of various microbes can be satisfactory, for two reasons, namely, that germs are frequently found in the conjunctival sac which have nothing to do with the inflammation in existence; thus oftentimes the (true?) diphtheria bacillus is to be cultivated though there is no diphtheria; and that a double infection may exist, as when a patient suffering from trachoma acquires gonorrhœal conjunctivitis. The ordinary acute ophthalmia seems to be caused practically always by Weeks's bacillus, though perhaps occasionally by another microbe, as we shall see. Subacute conjunctivitis is, in the opinion of the two authors, due to the diplo-bacillus, though all observers are not agreed on this point; indeed, some deny the existence of subacute or angular conjunctivitis as a separate entity. A form of acute contagious conjunctivitis is due to the pneumococcus, which is then present in immense numbers in the secretion; it must be remembered that this microbe is found in the normal conjunctival sac, but only in small amount, as a saprophyte. What is the explanation? The authors believe that there are two varieties of this form of inflammation, one in which, owing to some unexplained reason, this same germ becomes able to grow from an auto-infection from some conjunctival or lachrymal lesion,—this form is not itself infectious; and the other, essentially contagious, caused by the entrance from without of a special pneumococcus.

Is phlyctenular ophthalmia caused by microbic infection? The bacteriological history of this disease dates from 1885, when Burchardt asserted that phlyctenular conjunctivitis and keratitis were practically the same disease as eczema of the face; that, like it, they are infective in their nature, and that they were caused by one and the same organism. Michel is of opinion that phlyctenular ophthalmia is due to the same microbes as are to be found in the normal conjunctiva. Bach believes the *Staphylococcus pyogenes albus* and *aureus* to be the cause of eczematous conjunctivitis; but he thinks that before they can obtain a foothold, there must have been a loss of substance. Axenfeld says phlycten may be due to infection with diplo-bacillus, pneumococcus, or Weeks's bacillus. Michel has attempted to cultivate from the fluid obtained by puncture of a phlycten; in eighteen cultures he found *Staphylococcus pyogenes aureus* ten times, *Staphylococcus pyogenes albus* seven times. Nine times the *aureus* was found pure, on the tenth a diplo-bacillus was found along with it; five times the *albus* was found alone. Thus, out of eighteen occasions, *Staphylococcus* was demonstrated seventeen times. Attempts to reproduce the disease in animals were made by infecting the corneal substance, and were successful four times out of eight, but then in five out of six inoculations with other organisms, phlycten also resulted, so that it is probable that it is due to no one organism, though most frequently to the staphylococcus. The same observer found that other non-bacterial causes of irritation and reaction, such as toxins and foreign bodies, were incapable of producing phlyctenular ophthalmia.

Moll's article in the *Centralblatt* deals with the question of sympathetic ophthalmia. He started from the dictum laid down by Schmidt-

Rimpler, that the irritation of the ciliary nerves of an injured eye only sets up a disposition or tendency to inflammation of the second eye by a derangement of circulation and nutrition reflexly induced; he endeavoured to approach the subject then by first producing in animals a septic infection of marked bacteriological type, and then setting up a local lesion. He injected into the marginal vein of a rabbit's ear a cultivation of the bacillus of blue pus, and then applied various local stimuli. Pure nicotine failed because the animals so readily succumbed to its toxic influence; sheet copper and croton oil acted better. Bach worked at this subject also, though his methods were slightly different; but one important conclusion to which he came, was that no mischief occurred in the other eye unless one particular symptom, namely, pain, had persisted. Moll, working with his piece of copper and injection into the ear-vein as above indicated, found that after twenty-four hours the aqueous humour of the other eye gave in the great majority of instances typical colonies of the pyocyaneus with green coloration of the culture medium; the tapping of the anterior chamber was effected by means of a prepared canula, and great care was taken that it should not touch the iris. Out of thirty-six animals infected, fourteen were lost by excessive action of the injection, but of the remaining twenty-two, seventeen showed the bacillus in both eyes, and in all of these bacterial examination of the blood gave a positive result, while in five the dose seems to have been too small or the process too transitory to allow of the other eye becoming affected; in these five, examination of the blood gave a negative result. For further details, reference must be made to the original paper, but the bearing of this upon sympathetic ophthalmia may be briefly indicated. It appears that, given a certain condition of the system, irritation of the ciliary nerves on one side is capable of producing an exactly similar state of matters in the other; persistent irritation of one ciliary region is capable of altering the aqueous humour of the other eye, and if the lesion be severe enough and long continued, inflammation may occur, while, if evil influences be circulating in the blood, true sympathetic ophthalmia is likely to occur. By this suggestion, the long interval between the original lesion and the onset of sympathetic mischief, and the special danger should the patient be attacked in the meantime by any fever, are explained, for if he has a "suitable" injury, all that is needed for the development of sympathetic mischief is just that toxæmic condition which fever supplies.—Morax and Petit, *Ann. d'ocul.*, Paris, September 1898; Michel, *Ann. d'ocul.*, Paris, October 1898; Moll, *Centralbl. f. prakt. Augenh.*, Berlin, December 1898.

REPORTS OF THE MEDICAL SOCIETIES.

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Edinburgh Medico-Chirurgical Society.—1*st February* 1899.—Sir **John Batty Tuke**, President, in the chair.—Dr. **R. A. Fleming** showed a mother and four children apparently suffering from Addison's disease. The pigmentation began in the mother seven years ago, and had grown deeper with each successive pregnancy. There was no discoloration of the mucous membranes. In addition, the constitutional symptoms of Addison's disease — causeless vomiting and diarrhœa and excessive languor—were well marked. In the children, the pigmentation began four years, three years, six months, and two months ago. If these were cases of Addison's disease they were probably unique.—Mr. **Cotterill** and Dr. **Russell** showed a patient after mastoid disease and empyema. The former had been secondary to the latter condition—an unusual sequence of events.—Dr. **Shennan** showed —(1) Vessels from a case of tubercular meningitis; (2) early tubercle of the urethra from a case of general tuberculosis; (3) organism in cancrum oris. — Dr. **R. A. Fleming** showed—(1) Two stomachs showing mammillation, and (2) a heart with aneurysm of one of the sinuses of Valsalva.—Drs. **W. Stewart** and **John Thomson** showed a specimen of congenital malformation of the œsophagus.—Dr. **P. M'Bride** read a paper on "The Treatment of Ozæna with special reference to Cupric Electrolysis," which appears in this number of the Journal.—Dr. **Wallace** read a paper on "Movable Kidney." He could not agree with the ordinary accepted opinion that movable kidney was due to undue laxness of the abdominal parietes, the result of repeated pregnancies. Almost all of his cases had been in nulliparæ. Attention had recently been called to the fact that floating kidney was more common in childhood than was generally supposed, and this too went against the above theory, and in favour of the view that the condition is due to some congenital defect. The existence of movable kidney might easily be missed, as the organ may be lying low. Mr. Wallace had performed the operation of nephropexy on fourteen occasions, and in all, save one, the symptoms had been completely removed. In slighter cases a pad should be worn; but the point to be attended to, in the use of all these pads, was that the organ must be in proper position before they were applied.

Edinburgh Obstetrical Society.—*Wednesday, 8th February* 1899. —The President, Dr. **Halliday Croom**, in the chair.—The **President**, Professor **Simpson**, Dr. **Haultain**, and Dr. **Lackie** showed specimens.

—Dr. J. W. **Ballantyne** read a paper on "The Place of Pessaries in Modern Gynæcological Practice," and discussed their use in the various displacements of the uterus. In partial prolapse he found the ring pessary useful, but when the condition was complete, he always advised operation. In forward displacements, he found vaginal pessaries of little avail, and in acute anteflexions he hesitated to use the stem pessary, as advocated by some. With regard to backward displacements, no doubt a vaginal pessary frequently kept the stems in position, and relieved all the symptoms. Dr. Ballantyne discussed shortly the operations performed for the different displacements, and concluded by saying that, though some had described pessaries as a necessary evil, he believed they were really necessary in gynæcological practice, and had a useful sphere. — The President, Dr. **Halliday Croom**, read a paper on "Vaginal Hysterectomy for Cancer," which appears in this number of the Journal.

Royal Medical Society.—*27th January* 1899.—Dr. **Herring** in the chair.—Dr. **Cooper** brought up two cases for diagnosis. One was of a posterior dislocation of the radius and ulna at the elbow. The other case, a boy with a swelling in the lower third of the left femur, presented some difficulty; Dr. Cooper suggested that it might be a case of Paget's quiet necrosis.—Mr. **Stuart** exhibited a case of acute myelitis, somewhat improved, and Mr. **Preston** a man suffering from diffuse myelitis.—Dr. **Rhodes** then read his dissertation on "Vomiting," pointing out its great importance as a symptom in the diagnosis of many diseases, and reviewing the various theories regarding its physiology and etiology.

3rd February 1899.—Dr. G. **Simpson** in the chair.—Dr. **Boyd Jamieson** described and showed a case of double Charcot's disease at the hip in a man, æt. 45. Both femora were dislocated upwards on the ilia. The right cervical sympathetic was paralysed, and he had a peculiar ossification in the left tensor fascial tendon. There was a history of a fall and some injury to the hip region over three years ago. But, previous to the accident, patient had experienced shooting pains and other evidences of the onset of locomotor ataxia, from which he now suffered. —Dr. **M'Nair Scott** then read his dissertation on "Sleep." He reviewed the various theories as to its cause and that of its several phenomena. The vascular theory seems to be the most generally accepted one. That the brain is anæmic during sleep has been conclusively proved, but whether the cause or the effect is open to question.

10th February 1899.—Dr. **M'Nair Scott** in the chair.—Dr. **John Thomson** gave a lantern demonstration on the "Diagnosis and Progress of Mental Defects in Children."—Dr. **Simpson** described a case where septicæmia followed parturition. Antistreptococcic serum markedly relieved the symptoms, but failed to avert a fatal result, owing to probable thrombosis of the left lenticulo-striate artery.—Dr. **Purvis** then read Dr. **Edwin Bramwell**'s dissertation on the "Pathology of Locomotor Ataxia," showing that opinion is much divided. There is some reason for believing that the disease is primarily located in the nerve cells of the posterior ganglia.

Glasgow Pathological and Clinical Society.—*13th February* 1899.—Dr. **Fleming** in the chair.—Dr. **Fleming** delivered his presidental address upon "Some Statistics of Amputations, with Clinical Remarks." The records of the Glasgow Royal Infirmary were consulted for the past hundred years, and an interesting comparison made between antiseptic and pre-antiseptic results. To take one example, that of amputations above the wrist and ankle. During and between the years 1794–1838 (Laurie) there were 271 cases operated upon, with ninety-eight deaths, a percentage of 36·16. Between 1838–48 (Steele) there were 284 cases, with 122 deaths, a percentage of 42·95. Between 1849–73 (Thomas) there were 970 cases, with 344 deaths, a percentage of 35·7. Between 1885–95, 774 cases, with 132 deaths, a percentage of 17. Dr. Fleming's own cases between these years number 160, with twenty-five deaths, a percentage of 15·6. Other statistics were given, but these prove the greatly lessened mortality, if such proof be necessary, within recent years. Dr. Fleming has never acquired confidence in asepticism, and prefers for teaching purposes the antiseptic method, because of the difficulty in general practice of carrying out a thoroughly aseptic plan.—Dr. **M'Gregor Robertson** and Dr. J. **Souttar M'Kendrick** read a paper on metatastic pyæmia from trivial causes, illustrated by a case originating in a boil. A report on the pathological condition was submitted from Dr. L. R. **Sutherland**. Death occurred ten days after the onset of the acute symptoms. Numerous metatastic abscesses were found post mortem.

The Forfarshire Medical Association.—*Dundee, 3rd February* 1899.—Dr. **M'Gillivray** in the chair.—Professor **Kynoch** showed a solid tumour of the ovary, associated with multilocular cyst, weighing 7½ lb., removed on account of rapid ascites.—Dr. **Halley** showed a uterus from a case of ruptured extra-uterine gestation, with a fœtus in utero. The patient was in an extremely collapsed condition, with severe pain, which had come on eight hours previously. She stated that she was seven weeks pregnant. The abdomen was filled with blood escaping from a ruptured right Fallopian tube, which was ligatured as rapidly as possible and the wound closed. The patient scarcely rallied from the excessive bleeding, and died in ten hours.—Dr. J. W. **Miller** read notes of a case of lateral sclerosis under his care twenty years ago, with abdominal pain, which recurred at intervals and was followed later by tetanic contractions, at first in the right arm, and later extending. The patient lived three and a half years, having been bedridden three months before her death, with paralysis of upper and lower extremities.—Professor **Stalker** gave an account of a case of Bright's disease with uræmia, which had been treated by venesection, with the result that the convulsions ceased. He thought venesection was of distinct benefit in cases of kidney disease, where there was very little dropsy and persistent headache, and also in some cases in which cardiac distress was at its worst.

OBITUARY.

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Professor Henry Alleyne Nicholson, M.D., D.Sc., F.R.S.; born at Penrith, 11th September 1844; died at Aberdeen, 19th January 1899.

Through his text-books, as well as by his more direct work as a teacher—in Edinburgh, Toronto, Durham, St. Andrews, and Aberdeen—Professor Alleyne Nicholson has made many students of zoology and geology his debtors; and it is safe to say that of those who were directly his students, there is no one who has not felt that his death was that of a friend.

His father, Dr. John Nicholson, was an Oriental scholar, whose teaching gifts and historical sense reappeared in his son's professorial success and enthusiasm for palæontology. His early days were spent in the Lake District, rambling round Penrith, and at the Grammar School of Appleby, and to see his great love for his native countryside was part of the delight of accompanying him on his excursions in that stimulating geological environment. When he was overworked, as he undoubtedly was too often, by the pressure of what were really two chairs rolled into one, he found in a few days at Appleby the best of restoratives.

His student days were spent at Göttingen and at Edinburgh, and of these he had many a good story to tell, always ending in a burst of rich, infectious laughter, whose echoes sadden us to-day. He graduated as M.D. in 1869, gaining the Ettles Scholarship, and a gold medal for his thesis on "The Geology of Cumberland and Westmoreland." Immediately after graduation he became an extra-mural lecturer in Edinburgh, and rapidly gained a succession of appointments as professor of Natural History, at Toronto in 1871, Durham in 1874, and St. Andrews in 1875. In 1882 he was appointed to the chair of Natural History in Aberdeen, succeeding Professor Cossar Ewart, who was transferred to Edinburgh. Of recent years the strain of his double duty was lessened by the able assistance of Dr. Brown and Dr. Gibb; but Professor Nicholson's record remains as a marvel of industry and endurance, especially when we remember his numerous papers and memoirs, and the continual re-edition of his text-books. His memoirs on Stromatoporoids and Graptolites are highly esteemed by those competent to judge; and his most successful text-book—that on palæontology, with the third edition of which Mr. Lydekker co-operated—stood for many years quite alone of its kind.

As a naturalist, Professor Nicholson was fondest of investigating concrete things, of practical work, and of field geology; he had a horror of subtleties, and looked askance at Weismannism. As a teacher he was eminently successful,—patient, genial, and clear, and with a wonderful power with the blackboard. As a friend he has left us all mourning.
J. A. T.

Joseph Coats, M.D., Professor of Pathology, University of Glasgow.

The death of Professor Joseph Coats on 24th January has cast a sad and deep gloom over the present medical session in Glasgow University. Although he had been in failing health for some considerable time, a fatal issue was hardly feared, even towards the end. The real nature of his illness was late in disclosing itself. Two years ago he suffered from fitful attacks of fever, which led malaria and tubercle to be suspected. He remitted little or nothing of his labours during this period, and attended with rare self-abnegation to his many duties. His medical advisers at last insisted on his taking a complete rest; he resided therefore during the summer of 1897 in Guernsey, and took a voyage round the world in the autumn of the same year. For a time after his return he appeared to be stronger, and was able to resume his work at the beginning of the present winter session. He was, however, compelled to relinquish it again shortly afterwards. A malignant tumour in the neighbourhood of the descending colon was at last discovered. The second stage of colotomy was about to have been performed, when an unexpected and sudden collapse set in, and he quietly and without apparent suffering breathed his last.

It is difficult to estimate the loss which the University has suffered in his far too early death. Distinguished, able, cultured, righteous, gentle, and generous, he was beloved by all, students and colleagues alike. Thousands of students now scattered over the world mourn his loss. He was their esteemed, revered, and respected teacher. They recognised in him a man who knew and loved his work, who spared neither pains nor trouble in their behalf, who laboured with single and earnest heart to give them that knowledge of the principles and truths of pathology without which no man can hope to be a good physician. Himself no mean clinician, he taught pathology as a living science, and his finely balanced and carefully trained judgment enabled him to place before his students the facts he had to teach, in their due proportion and importance. His teaching was suggestive, in the highest and best sense. Alike by practice and teaching, and care devoted to their practical training, he taught his students to use their own brains, and stimulated them to think out questions for themselves. He was a master of his subject, to which he may be said to have devoted the chief energies of his life.

After going through a long and thorough education, he was appointed Pathologist to the Western Infirmary in 1874; he organised and established a combined system of University and Infirmary teaching. In 1893 he became *de nomine* what he had so long been *de facto*, the first

Professor of Pathology in Glasgow University. His work of creation and organisation was not yet complete, for he had conceived the noble ambition of establishing an Institute of Pathology, which, by the joint action of the University authorities and the Infirmary managers, he soon saw carried to fruition. Alike in its broad plans and its minutest details, it is the product of his brain. Alas that he should have been spared for so short a time to rule in the kingdom of his creation! Space does not permit even of short reference to the many contributions to the literature of medicine and pathology which we owe to him. His chief work is his "Manual of Pathology," one of the best books on the subject in the English language. As a writer, he shines as a careful and experienced thinker, who expresses his results in a clear, lucid, and graceful manner. All who know his public life know how admirable it was, but the smaller circle who were privileged to know his private life as well, can say, indubitably, that Coats was at his best in his own happy home. His name will remain among the most highly honoured of the sons of Glasgow University, so long as her records shall last.

W. H. Murray, M.D.

At the early age of 45, Dr. Murray of Galashiels died in January. A son of the manse, he passed directly from school to the study of medicine, and graduated in Edinburgh in 1874. For nearly a quarter of a century he practised in Galashiels, and filled an important place in every phase of public as well as professional life. The whole community of the Border town in which he laboured mourns his loss.

Thomas Cooke, F.R.C.S.

By the death of Mr. Cooke, which took place suddenly on 9th February, a unique personality has been removed from the medical world of London. Forced by circumstances, over many of which he had no control, to assume the position of a private teacher, he founded and conducted for many years the School of Anatomy and Physiology where for nearly thirty years he continued to labour in the instruction of youth.

William Rutherford, M.D., F.R.S.

We regret to learn, as we go to press, that Professor Rutherford died on the morning of 21st February.

ANALYTICAL REPORTS.

MÖLLER'S HYDROXYL-FREE COD-LIVER OIL.

MÖLLER'S cod-liver oil has long been well known and highly esteemed, while an assurance that it is made under the most able chemical supervision can be unhesitatingly insisted on, when the remarkable depth of the knowledge of organic chemistry, as revealed by Möller's able volume on organic chemical formulæ, is borne in mind. The originator, in 1853, of the steam process of preparing cod-liver oil, Peter Möller has lately been enabled to further improve the oil. It is common knowledge that cod-liver oil is apt to "repeat," and that this causes much of the distaste for it evinced by so many patients. Möller attributes this "repetition" to the formation of hydroxylised compounds of fatty acids, produced by the access of free oxygen to the oil during manufacture. He has succeeded in preparing an oil almost completely free from such unpleasant after-effects, by conducting all the processes necessary for its extraction from the liver cells in an atmosphere of carbonic acid gas alone.

Undoubtedly this method of manufacture improves the flavour of the oil, and banishes the unpleasant after-eructations. Möller's hydroxyl-free cod-liver oil can be described as absolutely pure, most scientifically prepared, free from unpleasant taste, and of great utility. Each bottle sent out is dated with the season during which its freshness and purity can be depended upon; while chemists left with old stock can always exchange it gratis for a fresh supply.

CHLOROBROM.

(Burgoyne, Burbidges, & Co., 16 Coleman St., London.)

The makers announce that they will be pleased to furnish samples free to any medical man on application.

CHLOROBROM is in the form of a dark brown liquid, possessing a taste like that of liquorice, and containing both bromide of potassium and chloralamide. Chloralamide is well known to be one of the safest hypnotics, chiefly because it rather stimulates than depresses the cardiac mechanism; and the combination of this body with bromide of potassium, originally brought forward by the late Professor Charteris of

Glasgow, has had happy and successful results, both in the treatment of simple insomnia and of the dread *mal de mer*. Numerous witnesses speak to its efficacy in the prevention of the latter, and it is well worth a trial at the hands of all those who go down upon the sea in ships, and whose cerebral centres of equilibration rapidly disturb the associated centres of nausea and vomiting.

HEROIN.

(Elberfeld Farben Fabriken Co. Ltd., 19 St. Dunstan's Hill, London, E.C.)

IN noticing the preparation sent us, under the name of Heroin, we are unable to draw any conclusions from personal clinical experience as to its actual effects on the body.

Heroin is an ester of morphina, with somewhat similar anodyne properties, but with no ill effects upon the cerebral faculties, while exerting a specially marked action upon the respiratory mechanism. The dose recommended is small—0·005 grm. (about $\frac{8}{100}$ gr.)—but may be increased to ten times that—$\frac{4}{5}$ gr.—in phthisis and asthma. To taste, it is mildly bitter; to smell, it is inactive; in water soluble with difficulty, but in diluted acetic acid it dissolves up rapidly. According to published observations, Heroin appears to be an admirable soporific, as an anodyne to be inferior to morphina, but as a respiratory calmative to be superior to that alkaloid and to codeina. In its action on the respiration it slows the breathing, without affecting the pulse; it allays cough, and relieves the dyspnœa of cardiac valvular disease, and these without producing constitutional disturbances. Further investigations into the properties of the body will be fraught with the greatest interest, and, if corroborative of the statements already made about it, will prove that in Heroin we should have offered us a most valuable addition to the ranks of our hypnotics, and one which ought to be of great benefit in the treatment of many forms of illness.

PHENALGIN.

(Etna Chemical Co., New York.)

ONE of the latest examples of synthetic analgesics has appeared in the substance termed Phenalgin. Like many others, it also acts as an antipyretic and a hypnotic, but differs from most in being a vascular stimulant, as well as in possessing some antiperiodic properties. The chief cause of phenalgin exerting a stimulant effect upon the heart and circulation is to be found in the fact that it is an ammoniated compound. It forms a white, fine powder, with a strong taste and odour of ammonia, completely soluble in water, and administered in the form of tablets or in capsule. It is claimed that it contains ammonia in a nascent form, which is liberated on entry into the stomach, and hence acts as a stimulant. Chemically, it is an amido-benzene or ammonio-phenyl-acetamide. From the published reports of the effects following its administration, it appears to possess no toxic properties, to act vigorously in the various directions noted above, as well as in many other allied ways. The doses advised vary from 2½ grs. to 20 grs., according to circumstances. If future experience corroborates the early high estimates of its properties, phenalgin will approach very closely to the ideal synthetic analgesic, antipyretic, hypnotic, antimalarial, etc., longed for by pharmacologists. Samples may be had free, on application to the agent in this country, E. J. Reid, 11 Dunedin House, Basinghall Street, London.

ROYAL LEAMINGTON SPA MINERAL WATERS.

(Bottled at the Spa.)

SPECIMENS of the well-known mineral and saline waters from the Royal Leamington Spa have been sent us. The Leamington Spa has regained much of its old celebrity as a health resort, and, quite apart from the merits of its waters, is well adapted to afford to those who go there to undergo the course the accessories indispensable to all such resorts—a beautiful environment, interesting places near at hand to visit, and most attractive grounds and parks within its own area.

The principal spring at Leamington supplies a chalybeate saline water, shown by Dr. Tidy to contain 56·15 grs. of chlorine per pint, sufficient to yield about 93·2 grs. of the chlorides of sodium, magnesium, and calcium, 25·05 grs. of sulphuric acid, or equal to about 35·2 grs. of the sulphates of the same metals, along with some of the carbonates of calcium and of iron, and traces of bromine, potassium, lithium, and silicon. The total is given as 127·79 to 132 grs. in the pint, or 1·4 per cent., in addition to pure carbonic acid gas.

The saline water sent us is a concentrated form of the sample noticed, with the same relative composition, but of greater efficacy as regards its action on the emunctories. The combination of carbonate of iron with salines in not too strong a proportion, allows of the ingestion of considerable amounts, and thereby promotes the flushing out of the body by the water taken in, affording one a rational preparation for use in the treatment of many constitutional maladies, a natural remedy cunningly dispensed by nature, in accordance with the best pharmacological laws and pharmaceutical methods.

MEDICAL NEWS.

THE CHAIR OF PATHOLOGY AT CAMBRIDGE.

ON Saturday, 11th February, the electors proceeded to fill the vacancy caused by the lamented death of Professor Kanthack, and appointed Dr. G. SIMS WOODHEAD to the chair. The Professor brings a great reputation to the new sphere he has been called to, and will, without doubt, adorn it, as he has every former office he has held. In offering our hearty congratulations to Dr. Woodhead, we may express the hope that his work at Cambridge may be as valuable and as successful as it has been in the past.

CONFERENCE ON TUBERCULOSIS.

THE Provost, Magistrates, and Council of the City of Edinburgh are to be congratulated on their enlightened public spirit in regard to the prevention of pulmonary tuberculosis. This was manifested by a conference, held on 9th January, in response to the invitation of the Lord Provost, at which Bailie Pollard received a number of leading medical men, including Sir Henry Littlejohn, several members of the Senatus Academicus, the Presidents and representatives of the Royal Colleges, and the leaders of veterinary science, in order to discuss the questions connected with the prevention of the disease.

COATS' "MANUAL OF PATHOLOGY."

A CONSIDERABLE portion of the work of revisal necessary in the preparation of a fourth edition of this text-book had been already accomplished by the author in the four months immediately preceding his lamented death.

It is in response to his expressed wish that the editorial duties are now assumed by Dr. LEWIS R. SUTHERLAND, senior assistant to the late Professor. It is expected that the work will be ready for publication in the early autumn.

INTERNATIONAL CONGRESS OF GYNÆCOLOGY AND OBSTETRICS.

THE third International Congress will be held in Amsterdam, 8th–13th August 1899. Four subjects have been arranged for discussion:—1. Treatment of Fibro-myoma; 2. Value of Antisepsis; 3. Influence of Posture on the Pelvis; 4. Indications for Cæsarean Section.

The Secretary for the United Kingdom is Dr. Giles, 37 Queen Anne Street, London, W.

CELTIC PRIZE.

WE are glad to observe that the Caledonian Medical Society is about to institute an annual prize for an essay on some Celtic subject. This year a prize of five guineas is offered for an essay on "The Ethnology of the Scottish Gael." Full particulars may be obtained from Dr. Macphail, Rowditch, Derby.

MONTHLY NOTES
ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,

MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

Meteorological Means for the East of Scotland District, for the Five Weeks ending January 28, and for the Year from January 1.

	Temperature.			Rainfall in Inches.		Sunshine. Hours.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees. Above 42°.	Accumulated Day-Degrees. Below 42°.	Actual.	Accumulated from Jan. 1.	Actual.	Accumulated from Jan. 1.	
Mean .	39°·8	28°	178°	2·6		32		3°·2
Observed	35°·8	4°	185°	3·7		41		3°·35
Difference	−4°·0	−24°	+7°	+1·1		+9		+0°·15

Vital Statistics.

Mortality in Four Large Towns in the East of Scotland, during January. Actual Numbers and Rate per 10,000 per annum.

	Total.		Constitutional Diseases.		Circulatory System.		Nervous System.		Respiratory System.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891-1897 . .	1,258	220·0	207	36·2	117	20·4	137	23·9	298	52·1
Actual Deaths and Rate per 10,000	1,308	229·0	235	41·1	140	24·5	126	22·0	268	46·9
Difference from Mean	+50	+9·0	+28	+4·9	+23	+4·1	−11	−1·9	−30	−5·2

	Digestive System.		Urinary System.		Pneumonia.		Phthisis.		Cancer.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891-1897 . .	71	12·4	26	4·5	97	17·0	103	17·8	47	8·2
Actual Deaths and Rate per 10,000	91	15·8	28	4·9	122	21·3	118	20·4	67	11·7
Difference from Mean	+20	+3·4	+2	+0·4	+25	+4·3	+15	+2·6	+20	+3·5

In January the mean temperature was 4° below the normal, the rainfall heavy, but the sunshine recorded considerably over the average. The number of deaths recorded during January in the four larger towns in the East of Scotland was rather above the average, the most noteworthy relative figures, showing increase under the particular heads, being those under the circulatory and digestive systems, under pneumonia and cancer; the excess in the case of the digestive system reaching 28 per cent., of pneumonia 26 per cent., and of cancer as high as 42 per cent. Nervous cases and total respiratory disorders caused fewer deaths than usual. Influenza was returned as the cause of four deaths. January, therefore, characterised by a heavy rainfall, a mean temperature below the normal, a marked decrease in the wind pressure, decreased sunshine, an increased variability of temperature from day to day, but without any occurrence of pronounced cold, shows an increased mortality.

EDINBURGH
MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

ON THE PREVENTION OF VALVULAR DISEASE OF THE HEART.

By Richard Caton, M.D., F.R.C.P., *Honorary Physician, Royal Infirmary; Emeritus Professor of Physiology, University College, Liverpool.*

No physician who has had a lengthy experience of hospital or private practice can fail to be impressed with the frequency and the seriousness of the cases of organic heart disease which come under his notice. He cannot but be saddened by the enfeebling and shortening of valuable lives from this cause, and particularly so, when, as so often occurs, the patient is youthful, and when, in place of keeping step with his fellows, he is compelled to fall out of the ranks, and see not only the hopes and aspirations of youth fade at the outset of his career, but even to experience the loss of all capacity to earn his daily bread. How often has the writer, in his thirty years' hospital experience, received into his wards a young man who has been driven from some laborious calling by mitral disease, with its various results, dyspnœa, bronchitis, and dropsy. After the usual treatment, the last three may disappear for the time, and, notwithstanding warnings, the patient perhaps believes himself well, and returns to his labour, until sad experience teaches him the narrow limitations of the work his crippled heart will stand.

As we all know, in the vast majority of such cases, acute rheumatism was the starting-point of this sad disablement.

Such experiences as these led the writer many years ago to ask himself the question, Is it impossible to render cardiac complication in rheumatism less frequent? Must the practitioner always remain a mere passive spectator of this, the most formidable and terrible phenomenon of acute rheumatism? Are there no means of prevention? When endocarditis occurs, we know that sometimes it subsides spontaneously, leaving no evil result, without any aid from treatment: can we in no way imitate nature so as to render this happy issue more frequent? Can we in no way help nature to produce as complete a cure in the heart as in the joints?

The following paper is intended as an answer to these questions:—

For the last eighteen years, while treating in hospital about three hundred and fifty cases of febrile rheumatism, these important inquiries have always been before me. The first principle which a somewhat lengthy experience has impressed upon my own mind is this: We should regard each case of acute rheumatism not merely and chiefly *as rheumatism*, but first and before everything else, as a case of *impending peril to the heart*, which may, or may not, mar the whole life of the patient. Apart from this risk, the rheumatism, *per se*, the mere pain, fever, and general discomfort, which, with the means at our disposal, we can soon mitigate, are comparatively unimportant. The mere cure of the rheumatism is in itself secondary to the great question: "Will the heart escape damage?"

In my opinion, then, the physician will make this question his first care, and will note the heart's condition daily, until the patient is quite well, in order to prevent mischief, if he can. If mischief occurs, he will do all he can to arrest it; if he cannot at once arrest it, he will give it every chance of subsiding during later convalescence.

At the present time, most members of our profession regard such effort as this as a hopeless task. In the spirit of the fatalist Moslem, they believe that if heart complication comes, it comes; it may possibly subside, though usually it does not, and they conceive themselves powerless to influence it. This, I confess, was my belief and my practice during the first dozen years of my professional career, but eighteen subsequent years of observation, directed especially to this question, have securely founded the conviction that much may be done to prevent and to arrest rheumatic endocarditis. So clearly has the good to be done become evident to my eyes, that I should now feel it a distinctly immoral action on my own part to omit certain precautions and certain method of treatment.

1. *As regards methods of prevention.*—Rheumatism, when once existing, seems to me to be aggravated by exposure to cold, and by checking (through chills) the profuse perspiration incident to the ailment: this is probably the opinion of most physicians. I keep in my wards a stock of warm flannel garments expressly for the use of these cases. Every patient is clothed from head to foot in warm flannel (with sufficient changes of the same) during the whole of the acute period. He is kept in the most absolute rest in bed. He is usually treated with gentle cholagogues and with

one of the ordinary salicylate preparations; the diet is a light and simple one, excluding for a considerable time all red meat. The heart is examined daily. If the pain fails to yield in any joint, it is quickly dispelled by the use of one or two small blisters applied locally, and followed by poultices.

Anyone who has tried Dr. Herbert Davies' method of treating rheumatism by small blisters to the joints, knows how effective that treatment is.

Uusually without this adjuvant, however, the pain and fever speedily subside, but, to prevent relapse and endocardial trouble, I always maintain the remaining treatment, with the exception perhaps of slacking off the salicylate. The prolonged rest, warmth, and quiet seem to me very important as means for preventing mischief, for cardiac trouble not unfrequently tends to come on late—even after all pyrexia is over. Under this treatment, 82 or 83 per cent. of my cases get well without any cardiac trouble at all. The remaining 17 or 18 per cent. who are attacked, form, I venture to think, a low average.

2. *Efforts to arrest cardiac valvular mischief when it has begun.*—If endocarditis shows itself by the development of an apex *bruit*, and by accentuation of the second sound at the pulmonary valve, or more rarely by a *bruit* at the aorta, I adopt the following measures:—To all appearance the salicylates have little or no beneficial effect on cardiac rheumatism. I therefore adopt the measure which is found effective for a refractory joint, namely, the application of small blisters, along with other means.

The reader may probably ask the question: Is there evidence that stimulation of the surface skin can in any way influence organs lying in the body cavity? All will agree that there can be no direct influence. If there is any effect at all, it must be by a reflection through spinal or sympathetic ganglia, *viâ* intercostal and visceral nerves. Is it possible that counter-irritation through these channels can do for the heart what it undoubtedly does for the joints? In reply, I can only give proof that certain nerve impulses do travel round by this route. Lengthy and laborious laboratory experiments gave distinct evidence that stimulation of the thoracic integument produced certain changes in the viscera beneath. Space will not allow me to state details, but I may say that, in a paper read before the Physiological Society at Oxford, I showed that stimulation, either thermal, electrical, mechanical, or chemical, of the thoracic skin produced changes in the calibre of the arterioles of the lung, also that similar stimulation modified the electrical potential of the intestinal wall, as shown by the galvanometer. These experiments did not prove any therapeutic influence,—it would scarcely be possible to prove that by such a method,—but they proved that an influence was transmitted round by the nerve communications extending between the surface skin and the organs within. By analogy it may fairly be assumed that likewise a trophic effect, a stimulation of nutritive processes, may be expected to follow such stimulation in an organ whose nutrition was perverted. Familiar examples of such an action might be quoted, *e.g.* what obstetrician does not know the effect of a sudden dash of cold water on the thoracic skin of a newborn child in quickening respiration? In a faint, is not a like application of avail in stimulating the heart?

The treatment I am advocating is in part founded upon this theory. As soon as a *bruit* is perceptible—in fact, as soon even as what the French call *assourdissement*, the muffling of the first sound at the apex, occurs, which is the usual precursor of the *bruit*,—we apply in the course of the third, fourth, fifth, or sixth intercostal nerve, small blisters, not much larger than a shilling, in succession, one at a time. They give little pain or inconvenience. The patient is kept in bed, absolutely recumbent and as quiet as possible, for several weeks. It is important, in my judgment, to keep the heart's action as quiet as we can. Moreover, we give, in addition to the salicylates, 8- or 10-gr. dose of sodium or potassium iodide thrice daily. This is given in the hope that it may help to absorb inflammatory products.

Results.—In about five-sevenths of my cases the *bruit* sooner or later gets soft, then becomes variable, being sometimes heard and sometimes not. A stage often exists during which the *bruit* is heard when the patient is recumbent, while it is absent when he sits up for a moment. At length it vanishes, and the patient goes out of the hospital with a sound heart. In about two-sevenths of my cases the *bruit* is persistent. Before I adopted this method the *recoveries* were less than two-sevenths, and the cases of persistent valvular disease more than five-sevenths—a very different result!

Probably the prolonged quiet and rest are important. The work of the heart and apparently the blood pressure are reduced to their normal minimum,—surely this must be a help to the reparative process. Possibly this is the essential part of the method; but whether it is so or not it is at present impossible to say.

One distinctly unfavourable fact I must mention. When, after an endocarditis, the apex *bruit* has disappeared, along with its accompanying accentuation of the second pulmonary sound, the whole almost certainly comes back again if the patient has a relapse, or a second attack of rheumatism within a few months.

In several cases a repetition of the treatment has a second time effected its removal; in others, again, the morbid condition has unhappily come back to stay. I need scarcely say that in *bruits* of long standing, in fact in all that are not quite recent, this method is absolutely useless.

My great difficulty in pursuing this inquiry has been to get a sufficient number of cases from which to make an adequate and thoroughly sound generalisation. Happily, as I have said, most of my acute rheumatisms do not develop any heart mischief at all, and the number that do is consequently not great.

I am naturally anxious that others should try this method. Some of my friends have done so, with success. The healthy scepticism, the slowness of belief in regard to all that is new, which in our profession helps us in general to sift the true from the false, necessarily leads men to doubt and to resist novel doctrines.

The method I advocate needs much patience, and I admit that sometimes a little persuasion is required to induce a convalescent, whose pain and fever are gone, to lie quiet in his bed long enough to give his heart a chance of recovery, for this may be four or five weeks. At the worst, the treatment, if it fails, does no harm, and certainly, in my view, it is worth while to do almost anything to save the sick man from the miseries of a permanently crippled heart.

I confess few things give me more pleasure than to observe day by day the gradual waning, and final disappearance, of all signs of valvular mischief in a patient in whom its commencement has been only too distinctly manifest. To send him home with a sound heart, and from time to time to test and observe the action of the organ, to make sure that all has gone on favourably, is one of those gratifications which compensate the physician for much that is hard and trying in his lot. To me it certainly is a pleasurable and a comforting thought, that very few cases of valvular disease have of late years had their origin in my wards.

ON THE TREATMENT OF SOME OF THE MORE COMMON EYE AFFECTIONS.

By G. A. Berry, M.B., F.R.C.S.Ed., *University Lecturer on Ophthalmology; Senior Ophthalmic Surgeon, Royal Infirmary, Edinburgh.*

(*Concluded from* Vol. IV., *page* 344.)

Glaucoma.—Though fortunately not a very common eye disease, glaucoma is yet one which occurs sufficiently frequently to be met with every now and then in the course of the work of every practitioner of any experience. The great importance which attaches to this disease is its curability when recognised and treated in time. A failure to diagnose glaucoma, and to adopt proper means for checking its progress, results inevitably in the disease leading to hopeless blindness. The later, too, that treatment is begun, the greater is the amount of permanent damage which is done to the sight of the affected eye, and, generally speaking, the less likelihood is there of the treatment resulting in even arresting its progress and preventing further destruction.

There are, therefore, two reasons why an early diagnosis is desirable. On the one hand, it enables one to treat the disease at a stage when, according to the severity of the glaucoma, little, or comparatively little, permanent damage has been done to sight: and, on the other hand, at which, altogether independent of this, it is most amenable to treatment. Except in cases of the more acute congestive variety of glaucoma, the result of successful interference is only a checking of the disease, without restoration of what has been lost.

Whilst, then, these considerations suffice to show the importance of making a diagnosis, it must be admitted that the diagnosis is by no means always easy. In acute congestive attacks, the more or less marked symptoms of general disturbance are apt to take one's attention from their real point of origin. In non-congestive glaucoma, on the other hand, the disease is usually of a slow, insidious nature, and as such is apt to escape observation. Indeed, there are some cases of the so-called "simple" variety of glaucoma which even the experienced ophthalmic surgeon may have great difficulty in distinguishing from cases of atrophy of the optic nerve.

The general practitioner, who has not devoted very considerable attention to the clinical study of eye diseases, could hardly be expected always to diagnose glaucoma. He should, however, unquestionably know of the existence of such a disease, and, in the absence of any particular familiarity with it, it would be well if he erred rather on the side of too great than of too little suspicion of its presence, in cases the exact nature of which he did not understand. Thus, especially in elderly or old people who may complain to him of failing sight, he should always inquire as to the particular subjective symptoms which may characterise that failure. More particularly should he ascertain if there appears to be any more or less marked degree of intermittency in it. Are there, for instance, periods of greater obscuration, succeeded by longer periods of relatively better vision? Further, is there a more or less constant haze or smoky veil seen in front of the eyes, which occasionally appears to be denser than at other times? Temporary obscurations, clouded or misty vision, and the appearance of coloured halos or rings round lights, are suggestive of glaucoma, particularly in old people, and should awaken suspicion. It must be remembered, too, that these symptoms are not always very marked in the "simple" cases of the disease.

Therefore, in the presence of failing sight, which is only, it may be, subject to periodic variations, a very easy examination should be undertaken. This consists of a rough test of the scope of the field of vision. This is done by placing the patient with his back to the window or other source of light, causing him then to fix one's eye steadily, and by making movements of the fingers of the hand held at the limits of one's own field of vision in all directions—inwards, outwards, upwards, and downwards—to determine how far his field is co-extensive with one's own. When it is necessary, in order that these movements should be recognised, to bring the hand in towards the line of fixation to a greater extent than for one's own eye, there must then be a limitation in the patient's field of vision in the corresponding direction. Any restriction of this kind, particularly if it involves the lower or inner portions of the field, should awaken the suspicion of glaucoma. Great failure of vision, without any restriction in the scope of the field of vision, is rare in glaucoma. Under the circumstances, then, a free, unrestricted field of vision should be reassuring where there might otherwise have appeared to be any reason to suspect glaucoma. On the other hand, the opposite condition, viz. restriction of the field with comparatively little or no evident loss of vision, should strengthen such suspicion.

I have referred, so far, to subjective symptoms and subjective examination in this relation, because experience has taught me that fewer mistakes are liable to be made by the comparatively speaking inexperienced, than when attention is directed more to real or imaginary objective signs. Such objective signs there are, no doubt, in glaucoma. Indeed, the diagnosis cannot properly be confirmed without giving attention to them as well. The most important of these signs, and they may here be referred to in what may be considered the order of their importance, are: excavation or "cupping" of the optic disc, increased intra-ocular tension, haziness of the cornea, semi-dilatation of the pupil, and enlargement of the episcleral veins.

Now, with regard to these, it may be said that if the general practitioner, who so comparatively seldom meets with a case of glaucoma at all, and who has so many other diseases to know about and to treat, is to wait till he can satisfy himself of the existence of a "glaucoma cup" before feeling reasonably satisfied that he has to do with a case of this disease, he will, generally speaking, fail to act in the best interests of his patient. There are, in fact, some cases in which a certain degree of doubt may exist as to whether a particular excavation is essentially "glaucomatous" or not. Then, as to increased tension, it is often, in the more chronic cases, difficult to feel satisfied that there is any abnormal degree of tension present. Like other signs of glaucoma, the increased tension is to a great extent an intermittent one. Indeed, there are not a few experienced ophthalmic surgeons who do not consider increase of intra-ocular tension at any time a necessary accompaniment of glaucoma. Nevertheless it is one of the most important signs to which to direct one's attention in the less frequent variety of congestive glaucoma with pain and corneal haze.

I have found that the cases of glaucoma most liable to be overlooked by the patient's ordinary medical attendant, are the chronic non-congestive ones, and the very acute congestive attacks associated with marked general disturbance. The first class of case, especially when occurring, as it usually does, in an old person, is apt, in the absence of a properly directed subjective examination, to be mistaken for cataract. The greyish reflexion which comes from the lens in old people, gives rise, when the pupil, as is often the case, is more or less dilated, to the idea that there is an actual loss of transparency of the lens. The patient is then told that he has cataract, and that nothing can be done until he has become blind. Thus the opportunity for arresting the disease is lost. I do not exaggerate when I say that I have known such a fatal diagnostic error made in at least fifty cases. A knowledge of the existence of such a disease as glaucoma, and of the various forms which it may assume, and a simple subjective examination on the lines above referred to, would strengthen the suspicion that a mistake in diagnosis might be possible, even should the ophthalmoscopic signs of cataract not be looked for. With regard to those signs, it is no very difficult matter for anyone who has once learnt to reflect the light of a candle or gas flame into the eye with an ophthalmoscope, and to observe the yellowish red reflection from the choroidal blood, to differentiate between a haze resulting from reflection at the lens surface, and actual opacity of the substance of the lens, or cataract.

In acute glaucoma, with general disturbance, it must be admitted that a mistaken diagnosis is more natural. The cases themselves are of rare occurrence, and the period, moreover, during which it might be possible to adopt a satisfactory line of treatment is much shorter. The pain in the eye and the accompanying blindness, though caused by the same excessive tension which gives rise to gastric pain and vomiting, is generally disregarded by the patient himself even. He is more inconvenienced by the general disturbance, and his medical attendant treats him for the acute gastric symptoms, and overlooks their cause. Such a mistake is not likely to occur twice to the same practitioner, yet it is one which in my experience is not infrequent. For this reason it should be generally known that violent gastric attacks may be set up in this way, and that if there be pain in one eye, preceding or accompanying such attacks, the tension of the eye should be felt and the eye examined. The tension in the case of an acute glaucoma, giving rise to symptoms of this degree of severity, will be found to be greatly increased. It is easy to satisfy oneself that the eye is "stony" hard. An inspection of the eye shows it to be congested, with the cornea steamy and the pupil dilated.

As to the *treatment* in cases of acute congestive glaucoma, the first thing to be done is to give a subcutaneous injection of morphine. Then, as soon as possible, preparations must be made for the operation of iridectomy. Owing to the great pain which this operation causes in such cases, it should be performed under chloroform. It is otherwise very difficult, even when there is enough assistance at hand, to keep the patient sufficiently quiet. The use of an anæsthetic is not only valuable in saving the patient pain, but materially aids the operator. Any uncontrollable movement on the part of the patient greatly increases the difficulty of the operation. With regard to the performance of iridectomy, the incision made into the anterior chamber should be as peripheral as possible, and be made with a thoroughly sharp knife. It is certainly most efficiently made with the triangular bent keratome, which should be rapidly introduced and then slowly withdrawn. Occasionally, owing to the eye being deeply sunk, the manipulation of this knife is a matter of difficulty. A Graefe knife may then be used, and the section made by puncture and counterpuncture, cutting afterwards as peripherally as possible from within outwards, and in one sweep preferably, that is, without any sawing motion. It is seldom that this section is as satisfactory as one made with a keratome. It necessarily, to some extent at least, entails a wounding of the peripheral portions of the iris which lie nearest the corneal wound. Yet it is one adopted in all cases by not a few surgeons in this country.

It used to be a rule to remove a large portion of iris. In my experience, a coloboma of 4 or 5 mm. diameter is sufficient. Nor is it necessary, as is sometimes advised, to forcibly tear the piece of iris excised from its peripheral attachment. On the contrary, this may in some cases do harm. Large iridectomies are necessary in cases of closed pupil from iritis. In glaucoma a moderate-sized coloboma and a large and peripheral corneo-scleral section made with a keratome is what should be aimed at.

The cases that do best are those in which the section heals quickly, leaving a barely visible scar. The bulging of the cicatrix, or a cystoid character of the union of the lips of the wound, is an indication that the operation has failed to fully relieve the increased tension. And although a cicatrix of this kind may act as a sort of safety-valve for a longer or shorter time, the condition of an eye in which it occurs cannot well be looked upon as quite satisfactory. Some surgeons, however, attach importance to the cystoid scar, and go so far as to look upon it as even desirable. For some time after the performance of iridectomy for glaucoma, it is a good precaution to drop a solution of eserine or pilocarpine into the eye two or three times daily. The amount used may gradually be diminished to one instillation daily, one every second day, and so on, when careful examination of the tension and the visual acuity shows that no recurrence has taken place. The standard as to the visual acuity should be that amount which, in the absence of complications, such as blood in the anterior chamber, exists one week after iridectomy. In the more acute cases, there is usually more, often very considerably more, than existed before operation. In chronic cases, on the other hand, it is usually much the same, and, indeed, not unfrequently somewhat less than what existed previous to the interference. What it is desirable to ascertain, however, is as to whether or not this post-operative degree of acuity is retained.

When an iridectomy which has been properly performed, and which has temporarily checked the disease, nevertheless fails to effect a permanent relief, this may apparently be due to one of two causes. Either the increased tension returns after some time (it may be with the consolidation of the cicatrix at the corneal margin), or a slow degenerative change in the retina causes blindness. The former seems to be the more common cause, and the question then naturally arises whether any further operation is called for, and if so, what. No doubt in respect to such cases there may be some considerable difference of opinion, even amongst those whose practical experience is sufficiently extensive to entitle them to hold some view on the matter. It is difficult, perhaps, in such a case not to be influenced by theoretical views as to the conditions giving rise to increased tension. I have found, however, practically that the treatment most likely to lead to a good result is to reopen the old corneal scar, and at the same time make a fresh wound and remove a further portion of iris. This operation is best done with a Graefe knife, which, being entered at the corneo-scleral margin at some distance from one end of the old scar, is passed out of the chamber at or near the other end of the scar, and the section completed by cutting outwards. A portion of iris continuous with one limb of the existing coloboma is then removed, and thus the coloboma made larger. von Graefe's plan of making an altogether fresh iridectomy directly opposite to the first one is no doubt also effective, but has the disadvantage of causing a more apparent disfigurement.

When the cause of deterioration after operation is the second one mentioned, no further operative interference is called for. The cases in which this result is mostly met with are in my experience those in which the central vision has from the first been relatively more impaired than the peripheral. Probably, indeed, in such cases the increased tension, which in any case has been comparatively slight, is also of subordinate importance in the process which leads to progressive blindness.

Iridectomy has so far proved to be the most efficient means of checking glaucoma, whether of the congestive or non-congestive variety. At one time sclerotomy was by many substituted for iridectomy, on the assumption that the beneficial action of the iridectomy depended entirely upon the incision made along the angle of the anterior chamber. There are still some surgeons who apparently adhere to the practice of sclerotomy alone. Many, however, have returned to iridectomy, which from purely practical considerations they have found to give more permanent results.

There are two varieties of glaucoma, or perhaps, more correctly speaking, two complications met with along with glaucoma, in which it seems preferable, where any operation is risked at all, to do sclerotomy. In the first place, what has been called hæmorrhagic glaucoma is a condition in which iridectomy is exceedingly risky. Sclerotomy is certainly less dangerous, but many of these cases are best left alone. The other complication is glaucoma (which is then always of a chronic type), in high degrees of myopia, with choroidal disease. In cases of this kind, I believe it is best to avoid iridectomy and perform sclerotomy instead.

There is still another class of case in which operation for the reduction of intra-ocular tension is indicated, and in which it is, comparatively speaking, unsafe to employ iridectomy as compared with sclerotomy. I refer to cases generally spoken of as buphthalmos, but which in reality must be looked upon as glaucoma occurring in early life, at a time when the coats of the eye offer less resistance to the intra-ocular pressure and become distended. In these cases iridectomy is rather apt to cause dislocation of the lens. The proper treatment is undoubtedly to perform sclerotomy, and even to repeat this operation several times, until either there is an evident decrease in the distension of the eye, or it appears quite hopeless that this can be brought about at all.

There is one other point in connection with the treatment of glaucoma which it is important to consider, and that is, how early in the process of the disease it is advisable to operate. We must recognise in most cases, mainly of the non-congestive type, a stage in which attacks of obscuration, etc., occur, which, on passing off, are found to have left no permanent injury to vision, and to be unaccompanied by any restriction of the visual field. These so-called prodromatous attacks, which characterise the premonitory stage of the disease, may only occur at considerable intervals of time. In some cases, however, they are of pretty frequent occurrence. Although some advocate operation in the premonitory stage of glaucoma, this is hardly justifiable. It seems inadvisable to undertake an operation which, though nearly free from risk, is not absolutely so, and which necessarily of itself causes some little deterioration of vision and a certain degree of disfigurement, before a careful examination reveals any permanent damage due to the disease, or, in other words, before the disease has entered upon its "confirmed" stage. In not a few cases, indeed, many years may elapse between the first and second stages.

The proper line of treatment to adopt where undoubted glaucoma prodromata are met with, is first of all to determine as far as possible the conditions under which these symptoms develop. Often it is found that they appear when the individual is exhausted or hungry, and that they pass off after a sleep or a meal. Sometimes, and particularly in nervous individuals, they are started by any undue excitement or exertion. In each case the patient should be cautioned to avoid as far as possible the particular exciting conditions. In addition to this, however, he should be given a myotic, which in most cases will quickly relieve the symptoms. Solutions of eserine or of pilocarpine are those most frequently used for this purpose. In general it is advisable not to use these myotics in too strong solution. From 1 to 2 grs. to the oz. is sufficient; and, on the whole, the preference should, I think, be given to pilocarpine.

In cases where premonitory glaucoma is at all frequent, it is well to advise the patient to have his pilocarpine drops always at hand. In that case, too, the solution should be made up with some antiseptic, such as boracic acid or corrosive sublimate, and only procured in small quantities at a time, so that it may be fairly fresh. The drops may even, in many cases, be advantageously used before the symptoms come on at all. In this way the attacks may be averted where the patient has learnt by experience to know the conditions which are liable to give rise to them. Gentle rubbing of the eyes is often also useful in dispelling the symptoms when they first come on.

Glaucoma is a disease which unfortunately does not necessarily cease after having caused destruction of sight. In the congestive type it continues for long to cause pain before the advancing degeneration leads to atrophy of the eye. This pain, which, like the other symptoms of the disease, is generally more or less intermittent in character, is often so severe, that even in this, the so-called "absolute" stage of glaucoma, operative treatment may be called for. It is seldom in this stage that iridectomy is capable of affording relief. It should, however, be tried in the first place. When it fails, the next thing to try is a scleral puncture. This simple operation consists in plunging a Graefe knife through the sclera into the vitreous at a point slightly in front of the equator of the eye, and midway between the external and inferior recti muscles. The wound made should have a meridional direction, that is, it should lie in a line drawn from the mid-point of the cornea. Should the pain still continue, it may be necessary, if it is very severe, and consequently affecting the patient's health, to enucleate the eyeball. This operation effectually relieves the pain which has its origin in the eye itself.

Where the patient is very averse to such a radical proceeding as removal of the eye, there is still another operation which, though not absolutely certain to give relief, often does so, namely, section of the optic nerve: optico-ciliary neurotomy or neurectomy. This operation is not quite easy to perform, and need, therefore, not be described here.

In addition to cases of true idiopathic glaucoma, there are many cases of injury and inflammation of the eye which may be complicated by symptoms of a glaucomatous nature. This in what is called "secondary" glaucoma. The treatment of such cases will vary according to the cause. Where the rise in intra-ocular tension is due to dislocation of the lens, it will often be necessary to extract the lens. Where the lens itself has been injured, and where, instead of absorption of the macerated fibres taking place without irritation, the tension of the eye becomes increased, the cornea steamy, and the patient complains of pain, or, as is mostly the case in children, is sick, removal of the macerated lens matter is called for. Or, again, when after an attack of iritis the synechiæ are so complete as to interfere with the proper intra-ocular circulation of the nutrient liquids, iridectomy is necessary to check the secondary glaucoma by which the sight of the eye would otherwise be lost. In other cases, as in serous irido-cyclitis in elderly people, the glaucomatous symptoms, which may then arise during the progress of the inflammation, must be combated by the performance of sclerotomy.

On the whole, the treatment of secondary glaucoma, if undertaken in time, is very satisfactory. The removal of the cause of the increased tension will at once check the process and save the eye from destruction.

THE CAUSATION AND RHYTHM OF THE CRESCENDO MURMUR OF MITRAL STENOSIS.

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(Concluded from page 248.)

Application of the Early Systolic Rhythm Theory to the Explanation of the various Clinical Appearances of the Crescendo Murmur.—The early systolic rhythm of the crescendo murmur very easily explains, not only the isolated occurrence of the bruit, but also all the combinations which it may form with other murmurs. Thus—

1. *Crescendo murmur alone.*—This is formed by blood regurgitating under pressure through the valve whilst its jaws are being closed by the rising intraventricular pressure developed by ventricular systole. An accentuated first heart sound terminates abruptly this murmur, and is due to the collision of the thick-hard jaws which guard the stenosed orifice (Fig. 1, B, D).

2. *Crescendo murmur continued by ventricular systolic murmur.*—In this case the former bruit is produced in the same way as when it is heard alone, but the coaptation of the jaws is imperfect, and fails to make the valve water- or blood-tight, so a small stream of blood continues to regurgitate through the valve, whilst ventricular systole persists and gives rise to the uniform systolic murmur. A more or less abrupt first sound divides these two murmurs (Fig. 1, C).

3. *Crescendo murmur and accentuated first sound, alternating with systolic murmur and less marked or no first sound.*—The occurrence of a crescendo murmur and sharp sound (Fig. 1, D) indicates that the intraventricular blood pressure is raised by ventricular systole sufficiently high to produce a transient regurgitant murmur before it closes the stiff valve with a snap, whilst the production of a ventricular systolic murmur, but no sound, means that (Fig. 1, E) the heart-beat is weak; sufficient pressure is not generated to close the valve anything like completely, no crescendo murmur or abrupt first sound is produced, and ordinary ventricular systolic regurgitation goes on, producing the uniform, true mitral systolic murmur. There is no change of rhythm with the alternation of these murmurs, both occurring during ventricular systole.

4. *Diastolic, crescendo, and systolic murmurs forming one long continuous bruit.*—In this case the diastolic murmur is formed by blood being driven through the stenosed orifice, under the propulsive force of the pulmonary blood pressure and of the contracting auricle, aided by the aspirating power of the dilating left ventricle; the crescendo murmur, by blood regurgitating through the *closing* valve during the earlier half of ventricular systole; and the systolic murmur, by blood escaping through a leak in the otherwise *closed* valve, during the latter half of ventricular contraction. The two last murmurs are separated by the abrupt tapping first heart sound, caused by the impact of the jaws of the orifice of the closing valve (Fig. 1, F).

Auricular systole is an event which occurs immediately before, actually excites, and is practically terminated by, the onset of ventricular systole, so there is no difficulty in understanding how a murmur (diastolic) produced by auricular systole can be apparently continuous with a bruit (crescendo), which begins to develop instantaneously with the earliest onset of ventricular contraction.

Some supporters of the presystolic rhythm theory of the crescendo murmur argue, that the fact that a crescendo murmur sometimes is apparently continuous with a mitral diastolic murmur is evidence in favour of the two murmurs being created by the same force acting with different intensities. But to my mind it is much more probable that the crescendo and mitral systolic murmurs, which are also equally often continuous, are both caused by different degrees of the same force. In the former instance, the most vigorous portion of the compound murmur is its very termination; and, to bring this about, the auricle—often dilated and powerless—must contract most forcibly when the resistance to its action afforded by the distended ventricle is greatest; whereas, in the other compound murmur, the vigorous portion coincides with the early and more forcible half of ventricular systole, and the weak portion of the murmur with the failing half of the ventricle's phase of contraction (Fig. 1, C).

AREA OF AUDIBILITY OF THE CRESCENDO MURMUR OF MITRAL STENOSIS.—The *crescendo murmur* is, as a rule, only audible over a small area, not greater than that of a crown-piece, immediately about the apex beat. When traced upwards or outwards from this area, the murmur disappears in the majority of cases, and the only trace of it heard is the terminal abrupt first sound of the heart. But cases are by no means uncommon, in which the crescendo murmur is conducted more widely; and I have myself heard it over the sternum and under the angle of the left scapula.

In Gibson's experience, this murmur, though best heard at the apex, may be conducted more widely than the systolic murmur about to be considered when the two occur in the same case.[1]

The *systolic murmur* of regurgitation through a stenosed mitral orifice is audible over an area very similar to the above, but it is supposed to be conducted more often into the axilla and to the back. Still, in these cases, the systolic murmur is more often than not replaced in the axilla and under the angle of the left scapula by the first sound of the heart. Thus, according to Steell[2] in twenty-eight out of forty-six cases (62 per cent.) in which a systolic murmur was present in the mitral area, this murmur was inaudible at the back. In the remaining seventeen (37·8 per cent.) a murmur was audible at the back; but in sixteen of these cases there was a systolic murmur over the tricuspid area, probably due to regurgitation through this latter valve, and it is quite possible that it was this murmur which was conducted to the back. In eight cases in which a systolic murmur was heard in the mitral, and only faintly or not at all in the tricuspid, area, the murmur was inaudible posteriorly.

Gibson says that this systolic murmur is of maximum intensity at or close to the apex beat, but is conducted in every direction, though more commonly to the axilla and the angle of the left scapula.[3]

The *mitral diastolic murmur* is generally audible at the apex. In the minority of cases its area of audibility is limited to the apex, but more usually the murmur can be heard above or internal to the apex beat, as well as over this latter area. Some of the faintest of these mitral diastolic murmurs are heard best, not at the apex but at the junction of the fourth left costal cartilage with the sternum, and between this point and the apex beat. It is an almost unknown occurrence for a mitral diastolic murmur to be conducted to the back.

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[1] *Op. cit.*, p. 168.

[2] *Med. Chron.*, Manchester, September 1895, p. 426.

[3] *Op. cit.*, p. 169.

In Balfour's experience,[1] "the position of maximum intensity of this mitral diastolic murmur" (*i.e.* that of mitral stenosis) "varies in each case; it is usually most distinct at the sternal end of the fourth rib on the left side, though sometimes it is heard in the mitral area, and frequently in the pulmonary area. The soft, slightly musical diastolic murmur audible in such cases in the mitral, or more faintly in the pulmonary area, is undoubtedly mitral in character and may disappear as the disease progresses."

Steell[2] says: "It is more common, therefore, to hear a diastolic murmur both at apex and above than at the apex only," in cases in which the mitral valve is stenosed, and the aortic valves healthy, as far as clinical evidence indicates. Thus out of forty cases of mitral stenosis, in which an undoubted mitral diastolic murmur was heard, either at the apex only or at and above the apex, the bruit was audible in eighteen cases in the former and in twenty-two cases in the latter situations.

In another place Steell says: "Now that I have learned to look for a diastolic murmur *above* the apex in every case of mitral stenosis, the frequency of such a murmur seems to be very great indeed."[3]

Gibson[4] says that the diastolic murmur may be heard more widely than an accompanying "presystolic" murmur is, or it may be the reverse.

In discussing the question of the areas of audibility of the murmurs of mitral stenosis, I have quoted in support of my own views the opinions held by eminent observers who differ from my belief that the crescendo murmur is ventricular systolic in rhythm; so I cannot be accused of having collected statistics to emphasise my own theories.

The foregoing facts show that—

1. The crescendo murmur is most audible at the apex beat, and is rarely conducted to the left margin of the sternum or to the back of the chest.

2. The systolic murmur of mitral stenosis is loudest at the apex beat, and only in the minority of cases is conducted to the back or to the sternum, and even in these cases it is not improbable that the murmur conducted to the back is of tricuspid and not mitral origin.

3. The mitral diastolic murmur is, in the majority of cases, audible near the left margin of the sternum as well as at the apex

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[1] *Op. cit.*, p. 129, note.

[2] *Op. cit.*, p. 431.

[3] *Loc. cit.*, p. 431.

[4] *Op. cit.*, p. 168.

beat; in some cases it is most distinct at the sternal end of the fourth rib.

These facts, I submit, show that the area of audibility of the crescendo murmur is more nearly allied to that of the mitral systolic than to that of the mitral diastolic murmur. This is contrary to the opinion of the majority of writers.

LIMITED CONDUCTION OF CRESCENDO AND SYSTOLIC MURMURS OF MITRAL STENOSIS.—The most probable explanation of the limited conduction of the crescendo and systolic murmurs of mitral stenosis lies in the fact that the streams of blood which cause these murmurs are very limited in volume, for, with few exceptions, it is impossible for any stream of blood of large volume to escape through an incompetent orifice of a stenosed valve, even were the valve to remain practically wide open during the phase of ventricular systole, instead of being more or less competently closed. The orifices of stenosed valves, in their condition of widest patency, are frequently only sufficiently capacious to admit the tip of one's little finger, *i.e.* from ¼ in. to ⅜ in. in widest diameter—they may even be smaller than this; and when closed, the area of regurgitant leakage is often no more than what one's finger-nail can go into. Even where stenosis is not so extreme, the area of regurgitation is very limited compared with that of severe muscle failure incompetence. In support of this theory, that the limited conduction of crescendo and ventricular systolic murmurs of mitral stenosis is due to the very slight volume of the regurgitant stream, may be advanced the evidence afforded by a study of the areas of audibility of systolic murmurs produced by other forms of mitral regurgitation. In incompetence of the mitral valve, resulting from acute endocarditis, we have a systolic murmur which, when it is first heard, is only audible at the apex beat of the heart. In a similar way murmurs of regurgitation, due to slight incompetence of the mitral valve, resulting from muscle failure of the heart, are heard only at the apex. Both these types of murmur are caused by the regurgitation of but small streams of blood, and it is not until the disease which is rendering the valve incompetent has made more progress, and has given rise to a considerable area of leakage, that the murmur produced by the regurgitation of blood through the incompetent valve is conducted away from the apex, through the axilla to the back of the chest. It is probable, therefore, that the want of conduction of the crescendo and systolic murmurs of mitral stenosis is not due to any want of intensity of sound, but to the smallness of the regurgitant stream which causes them.

The pitch of a murmur seems to have nothing to do with its conductivity, for shrill musical murmurs may be limited to the apex beat, and low-pitched bruits be audible posteriorly.

Gibson says that it is not always the murmur of the loudest intensity which is conducted farthest, and that it is not easy to determine what particular kind of murmur carries best.[1]

It may be argued that the systolic murmur of mitral stenosis *is* frequently conducted to the back; but, as I stated when considering the area of audibility of this murmur, there is no conclusive proof, in many instances, that such is the case, for it is quite probable that the murmur heard at the back in such cases is of tricuspid origin, and produced by regurgitation of blood through the extensive area of leakage which is generally met with in the tricuspid valve in cases in which there is regurgitation through a stenosed mitral valve. Systolic murmurs heard only at the apex beat, and not at the tricuspid area, of the heart in cases of mitral stenosis, are rarely conducted to the back; still I believe that some few cases may be met with in which the area of incompetence of the stenosed orifice is sufficiently great to allow of the regurgitation of a stream of blood of sufficient volume to cause a bruit which can be heard at the back.

I may mention here a point which, though it perhaps has no apparent bearing on determining the area of audibility of the crescendo murmur, is not without interest. It is that the true apex of the heart is not always in contact with the chest wall in severe cases of mitral stenosis. Dilatation of the right ventricle frequently develops in consequence of the obstruction to the flow of blood through the lungs which is afforded by the stenosed mitral valve. This dilatation makes the right ventricle into a kind of water pad which is capable of abnormally large distension, and as a result of this distension the apex of the left ventricle is displaced backwards from its normal juxtaposition with the anterior chest wall. With this condition of affairs present I believe that many crescendo murmurs are not heard in mitral stenosis, which would be were the left ventricle able to conduct them to the chest wall.

Our knowledge of the principles which govern the area of audibility, or the extent of the conduction of cardiac murmurs, is unfortunately still very rudimentary and unsatisfactory; but, for the reasons just given, it seems very probable that the limited area of audibility of the crescendo and systolic murmurs of mitral stenosis is due to the very limited amount of blood which, by its regurgitation, causes these bruits.

But, assuming that I have entirely failed to prove my point, it is a much more difficult task for the advocates of the diastolic rhythm of the crescendo murmur to explain why this bruit is so very rarely heard at the left edge of the sternum, where faint true diastolic bruits are often best heard.

THE MURMURS OF AORTIC DISEASE.—There is another not uncommon form of heart disease, in which a marked obstruction to the direct flow of blood through the heart exists, and which is comparable to stenosis of the mitral valve. I mean stenosis of the aortic valve; and it is interesting, and at the same time instructive, to compare the auscultatory signs met with in the two conditions.

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[1] *Op. cit.*, p. 171.

In aortic as in mitral stenosis, we hear systolic, diastolic, and, according to some observers, "presystolic" or crescendo murmurs.

The ventricular systolic murmur of aortic stenosis being caused by resistance to the normal direct flow of blood through the heart, is analogous to the bruit produced in a similar way at the mitral orifice, *i.e.* to what is commonly called the ventricular diastolic murmur.

The aortic diastolic murmur being one of regurgitation, is, on the other hand, comparable to the ventricular systolic bruit of mitral stenosis, which is also one of regurgitation.

The crescendo or "presystolic" bruit still remains to be considered, and if my contention, that in mitral stenosis this bruit is regurgitant, be true, we should expect the analogous murmur of aortic stenosis to be regurgitant also; and this is, I believe, admitted to be the case by all those observers who have described it.

We never hear a crescendo murmur at the aortic orifice during the systole of the left ventricle, that is, during the direct flow of blood into the aorta. An aortic systolic bruit, though it may be rough, is always of a decrescendo type, as would naturally be supposed when we remember that the force which produces the murmur, *i.e.* ventricular systole, is of itself failing, and decrescendo in nature.

The very powerful ventricle then never, by its contraction, produces a vigorous crescendo murmur of ascending pitch at the stenosed aortic orifice. We are therefore justified in assuming that the conditions present are not favourable for the development of such a murmur at the stenosed aortic orifice, during the *direct* propulsion of blood through the heart by ventricular systole; and, arguing by analogy, it seems very probable that such a peculiar type of murmur, which requires very special conditions for its development, should not be produced at the stenosed mitral valve by an entirely analogous series of conditions during the direct propulsion of blood through the heart by auricular systole.

The crescendo murmur of aortic stenosis can, I believe, be produced in a similar manner to its mitral analogue, that is, by blood rushing through the diseased valve whilst the latter is closing abnormally slowly. It is not at all improbable that the thickened, atheromatous, stiff valve curtains met with in aortic stenosis should require an abnormal force to bring them together and prevent the regurgitation of blood into the ventricle. Such abnormal force may be developed at a later period of the "systole" of the aortic wall than that at which the normally thin valves close; and whilst the cusps of the valve are being abnormally slowly closed, blood will regurgitate through the gradually diminishing leak in the valve orifice, and create a short murmur of ascending pitch and increasing force, which will terminate with a modified accentuated second sound appearing after the second pulmonary sound has been heard. Such a bruit would terminate at a distinct interval before the onset of ventricular systole; or the stiffened valve may resist closure by the unaided action of the systole of the aorta, and only yield when the negative pressure of the dilating left ventricle is brought to bear on it as well. In this case the bruit would be more prolonged, and its terminal ascending pitch portion be close to the first sound of the heart denoting the onset of ventricular systole.

On the other hand, with the opening of these stiff valves, a systolic murmur of diminishing pitch (*ee–oo*) will be created by blood rushing through a gradually enlarging orifice, which is opened abnormally slowly by the increasing force of developing ventricular systole. And this murmur, I contend, is comparable in characteristics and in origin with that decrescendo diastolic murmur which is produced at the stenosed mitral orifice in the first half of ventricular diastole by blood rushing through the opening valve.

For the above reasons, then, I believe that the different murmurs of aortic stenosis are produced by forces and processes which are comparable with those concerned in the development of their mitral analogues; and as the aortic bruits have a rhythmic relation to the systole of the ventricle, so have the corresponding mitral murmurs an analogous rhythmic relation to the systole of the auricle; and, such being the case, the crescendo murmurs of mitral as well as of aortic stenosis are regurgitant and not direct.

TACTILE THRILL ACCOMPANYING CRESCENDO MURMURS IN MITRAL STENOSIS.—A "thrill," synchronous in rhythm with diastolic or with crescendo murmurs, is commonly felt in cases of mitral stenosis when the hand is laid over the region of the cardiac impulse. Such a thrill may be one of two types, according to its rhythm.

1. *Diastolic thrill.*—This tactile thrill is always synchronous with the undoubted mitral diastolic murmurs from which it derives its name. It begins after the cardiac impulse has subsided, and, as timed with the stethoscope, after the second sound of the heart, and always dies away before the onset of the next beat of the heart. In character it is somewhat prolonged, not very vigorous, and is composed of fine vibrations. It is of decrescendo force, becoming less and less vigorous towards its termination, which is never abrupt, but of a gradually fading character; but, when it occurs in association with a crescendo thrill, it may not have died away altogether before the onset of the latter, in which case the two form one continuous thrill.

2. The *crescendo thrill* is synchronous with the murmur of the same name, and is termed the *presystolic thrill* by the advocates of the presystolic theory. Like the murmur from which it derives its name, one of its chief characteristics is, that it increases progressively in vigour, attaining its maximum at the time of its abrupt termination, which is synchronous with the onset of the true cardiac impulse. It is shorter, much coarser, and more vigorous than the diastolic thrill to which it may be joined, although it more often occurs alone. When continuous with the diastolic thrill, the conjoint thrill fills up nearly the whole interval between the second and first sounds of the heart.

Rhythm of the crescendo thrill.—There is no question as to the rhythm of the decrescendo mitral diastolic thrill, for it undoubtedly is developed during the phase of ventricular diastole, but opinions are at variance concerning that of the crescendo or presystolic thrill.

If my theory of the causation of the crescendo murmur of mitral stenosis be true, namely, that it is due to the regurgitation of blood through the closing jaws of the valve at a very early stage of ventricular systole, and before sufficient force has been developed to close the valve and produce the true cardiac impulse, we should expect, in those cases in which a crescendo thrill is palpable, to feel the thrill immediately before, and ending abruptly with, the heave of the apex which denotes the true cardiac impulse, and which is synchronous with the forcible contraction of the left ventricle and with the production of the first sound of the heart.

That such an event does occur, will not be denied even by the most ardent supporters of the "presystolic" theory.

There is frequently, in my experience, to be seen in cases of "crescendo" thrill, a preliminary agitation of the region of the apex beat, immediately before the true cardiac impulse is observed; and this is due, I believe, to the preliminary ventricular contraction which is trying to close the stenosed valve, and which is really an imperfectly developed cardiac impulse. When, however, the mitral valve is forced to close, and the ventricle has thereby obtained the *point d'appui* of resistance which determines its active systole, it contracts vigorously, and the true cardiac impulse is felt.

The above remarks concerning the rhythm of the apex thrill are also applicable to the generally accepted fact, that the crescendo murmur immediately precedes the throb in the carotid artery which is practically synchronous with the cardiac impulse. For as blood is slipping back through the orifice of the stenosed mitral valve whilst the ventricle is endeavouring to close it, the development of intraventricular pressure of sufficient amount to open the aortic valve is delayed, no blood passes into the aorta, and so no pulse can be felt in the carotid artery. In health, a distinct measurable period of time ($\frac{3}{100}$ second) elapses after the onset of ventricular systole, before blood is forced into the aorta; and it is quite easy to see how the escape of blood through an open valve in mitral stenosis will further delay the development of the aortic and carotid pulse. During this period of delay, in mitral stenosis, the crescendo murmur occurs, preceding the carotid pulse, as it precedes the impulse of the true, vigorous, working ventricular contraction.

THE SPHYGMOGRAPH AND CARDIOGRAPH IN MITRAL STENOSIS.—The *sphygmograph* affords no evidence of any value in an investigation of the rhythm of the crescendo murmur of mitral stenosis.

The *cardiograph* is of more use, although the tracings taken of the apex beat in health and disease are interpreted differently by different observers. But in most of the cardiograms of cases of mitral stenosis with crescendo murmur, there are to be seen waves indicating more or less marked movement of the apex immediately before the abrupt rise denoting true ventricular systole develops; and these movements of the apex beat could be accounted for by my suggestion that there is a preliminary systolic agitation of the apex of the ventricle during the time of the closing of the stenosed valve, due to the earliest attempt on the part of the ventricle to contract effectually.

CAN CRESCENDO MURMURS DEVELOP WITHOUT AURICULO-VENTRICULAR STENOSIS?—By accepting my theory, that the crescendo murmur of mitral stenosis is formed by blood rushing through an orifice which is gradually diminishing in area, we can understand how an almost identical type of murmur can arise at valves which are not only not stenosed but even of abnormally large orificial area. That a "presystolic" murmur does arise at times in these latter conditions, has been believed for many years by certain observers. The term presystolic is, however, as mentioned previously, used in rather a vague way by some writers, who apply it to any one, or to the combination of all, of the three distinct murmurs which may occupy the interval between the second and first heart sounds in mitral stenosis, and not to the true crescendo murmur only. In a valuable paper, "On Presystolic Apex Murmur without Mitral Stenosis," Phear[1] adopts this vague nomenclature, for, amongst the forty-six cases which he refers to, there are twelve in which there is no mention whatever of a presystolic murmur, diastolic being the term employed. But I believe that it is quite possible for a crescendo murmur of ascending pitch, but without a typical abrupt terminal sound, to occur when there is no valvular stenosis present.

Under my theory the essential conditions for the development of a crescendo murmur are, that the valve at which it is produced must be only *temporarily incompetent*, that the leak in it must become obliterated *during the production* of the murmur, and that the force which develops the murmur must be vigorous. Given these three conditions, it matters not what state the valve orifice may be in. Like the true crescendo murmurs, these new bruits are always regurgitant. I have shown how I think such conditions can occur in stenosis of the aortic, mitral, or tricuspid valves, and I will now give my reasons for believing that there may be only a temporary leak when these valves are incompetent from other causes than stenosis.

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[1] *Lancet*, London, 1895, vol. ii. p. 716.

Incompetence of a valve results from perverted action of the forces which tend to bring about its closure, and from structural changes in the valve appendages. Three muscular forces are concerned in the normal closure of the auriculo-ventricular valves:—

1. *Contraction of the whole ventricle.*—In health the area of the orifices which the mitral and tricuspid valves protect is greatly diminished during ventricular systole, owing to the diminution in area which the bases of the ventricles undergo during systole. According to Ludwig and Hesse, this diminution reduces the areas in question to about half of their diastolic extent.[1]

2. *The circular muscular fibres*, which surround the same orifices, aid by their contraction in the systolic diminution of the area of the valves.

3. *The papillary muscles* attached to the curtains of the valves form the last of the active muscular forces concerned in the development of a competent closure of the auriculo-ventricular valves.

Intact curtains and chordæ tendineæ are essential for the competency of a valve, but, on the other hand, valves may leak without there being any changes in these structures.

Forcible ventricular contraction is not necessary for the closure of these valves in health, but I must point out that, however much a ventricle may be dilated in disease, it is always more powerful than its auricle is, and therefore more able to produce a vigorous murmur.

Should any cause obstruct the normal action of one or more of the above forces during systole of the ventricle, incompetency of the valve will result and a murmur of regurgitation be heard. This incompetency may be permanent, that is, it may last throughout the whole of each systolic beat—an event which most generally happens—or it may be temporary, and only occupy about the first half of ventricular systole.

Permanent incompetency results in the development of an ordinary prolonged systolic murmur of regurgitation.

Temporary incompetency, on the other hand, with a murmur audible only during portion of ventricular systole, may occur in the following conditions:—

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[1] *Brit. Med. Journ.*, London, 1882, vol. ii. p. 821.

1. *Dilatation of the ventricle* from primary muscle failure, or as a consequence of aortic regurgitation or adherent pericardium. This generally leads to an enlargement or dilatation of the mitral valve, making its orifice too extensive for competent closure by the two curtains at the commencement of ventricular systole (Fig. 4, A). In cases in which the dilatation is extreme, the leakage in the valve is never corrected; but in others, in which the disease has not advanced very far, it is quite possible for the area of the valve's orifice to be diminished at an early stage of the ventricle's systole, by the coaptation of the walls of the base of the ventricle and of the margins of the orifice sufficiently to enable the curtains to cut off the leakage and its attendant murmur (Fig. 4, B and C), and make the valve quite competent. It may only require a very slight diminution in the area of the valve's orifice to permit of this leak-stopping process.

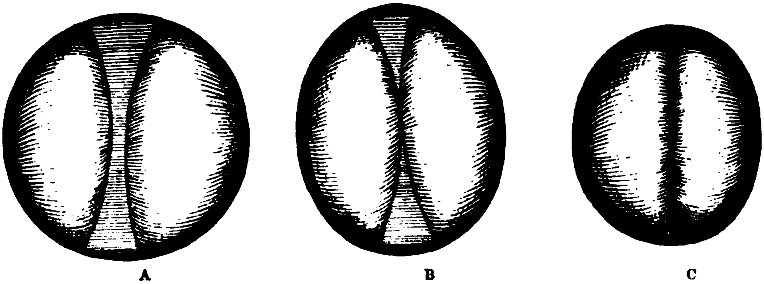

FIG. 4.—Diagrammatic representation of the leakage in a dilated mitral valve, being corrected by the diminution in the area of the valve orifice during ventricular systole.

Balfour, as mentioned previously, when writing on curable mitral regurgitation, states that in his opinion, "in slight dilatation at the commencement of ventricular systole, the valve segments are not in apposition as they ought to be, and there is some regurgitation, but this ceases as the systole progresses and the valve segments are perfectly closed." The result of this is "a more or less impure first sound, or a transient systolic whiff may terminate in an appparently normal first sound." Such a transient systolic murmur would, in my opinion, be of ascending pitch and crescendo force type, from the fact of its being created at a closing orifice and by a force of gathering intensity.

2. The *papillary muscles* also play an important part in the normal closure of the auriculo-ventricular valves. To these muscles the curtains of the valves are attached by means of the chordæ tendineæ, and it is their function, probably by contractile resistance, to limit the upward movement of the valve curtains during ventricular systole. In health, the bases of the papillary muscles are arranged just far enough apart from each other to

allow of the valve curtains moving a sufficient distance upwards to close the valve orifices when contraction of the ventricles begins. When however, as a result of disease, the wall of the ventricles to which the papillary muscles are attached, becomes stretched or dilated, and these muscles are moved abnormally far apart, especially at the onset of ventricular systole, or when the chordæ tendineæ become shrivelled and shortened, the cusps of the valves are dragged down, and have a greatly restricted range of movement, upwards and inwards. In consequence of this, the curtains cannot come as close together at the onset of ventricular systole as they do in health, even if the orifice of the valve be of normal area, but only become competent when the bases of the papillary muscles have to the requisite degree approached each other through the systolic coaptation of the walls of the ventricle to which they are attached. In such a condition of affairs, a leak would occur in the incompetent valve at the beginning of ventricular systole, and disappear at the later phase when the valve is closed up.

Such a temporary crippling of the papillary muscles may result from—(*a*) *Dilatation of the ventricle,* with the bases of the papillary muscles attached abnormally far apart, the chordæ tendineæ and valve curtains remaining normal; (*b*) *shrinking of the chordæ tendineæ* and puckering of the valve curtains, with the cavity of the ventricle remaining practically normal; (*c*) and *adherent pericardium,* which not only acts by inducing ventricular dilatation, but by hindering the systolic diminution of the area of the valve orifices. Foster[1] states that "in health the ventricle away from the apex retires from the chest wall in systole." But, when the pericardium is adherent, such a movement must be impossible, and the diminution in the basal area of the ventricle must be made by restricted cardiac movements, instead of by the free systolic coaptation of the entire basal ring. Here, again, is an opportunity for delay in the competent closure of the auriculo-ventricular valves, when the disease is not too far advanced.

I have not taken into consideration the systolic contraction which the papillary muscles undergo in health (Roy and Adami[2]); for, whether this is present or not in the diseased conditions, the results obtained would be the same.

I have now mentioned the conditions besides that of stenosis in which I consider that there may be only a temporary delay in the closure of the auriculo-ventricular valves, and especially of the mitral valve, and I have explained how, in my opinion, a leak which occurs in these valves at the onset of ventricular systole may be corrected completely at a slightly later period of this same phase of the cardiac cycle. So here we have a leak at which a murmur is developed being gradually but rapidly obliterated until it disappears, in much the same way as I maintain occurs in valvular stenosis; and though the area of the leak in the former condition may be greater than in the latter, yet I believe that the murmur produced at the vanishing larger leak will undergo the same changes in pitch as it does in stenosis. What I want to insist on is, that during the obliteration of the leak in dilated valves, a murmur will be developed which will have the character of ascending pitch imparted to it, from the fact of its being caused at an orifice of dimensions gradually lessening to total extinction; and which will be of increasing vigour—*i.e.* crescendo—because it is created during the period of developing vigour of the ventricle's contraction. Such a murmur will have an abrupt termination at the height of its vigour and pitch, but no sharp accentuated terminal sound, as is met with in the crescendo murmur of stenosis, will be heard. If such a leak remains open throughout the whole of ventricular systole, an ordinary systolic murmur of regurgitation results.

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[1] "Text-Book of Physiology," vol. i. p. 238.

[2] *Practitioner,* London, 1890.

The rhythm of the murmur just described is the same as that of the crescendo and ordinary systolic murmurs of mitral stenosis (Fig. 1, D, E).

Crescendo murmur in aortic stenosis.—I have also explained the manner in which I think a crescendo murmur may be produced in aortic stenosis, by blood regurgitating through the abnormally rigid or calcified jaws of the stenosed valve, whilst they are closing, and so receive a character of ascending pitch. A terminal sound, formed by a modified second sound of the heart and not by an accentuated first sound, would be heard in such cases.

CHANGE OF PITCH OF NON-CRESCENDO MURMURS.—My theory, that a murmur varies in pitch with the variations in size which the orifice at which it is produced undergoes, explains the change from low to high pitch, or *vice versâ*, which is frequently noticed to take place in ordinary systolic or diastolic murmurs. It is not at all rare to hear a murmur change from low (*oo*) to high (*ee*) pitch during each "beat," and form an imperfectly developed, gradually terminating crescendo murmur. Such a murmur is caused by blood being forced through a valve, in which a leak becomes lessened in extent, but not completely obliterated, during the development of the murmur. It is regurgitant in rhythm, and almost invariably produced at the auriculo-ventricular valve orifice. On the other hand, a murmur may change from high (*ee*) to low (*oo*) pitch by being developed at a gradually enlarging orifice. This type of murmur appears during the direct passage of blood through a stenosed valve, which is forced open more widely by the gathering vigour of the blood pressure behind it.

The changes in pitch here referred to have nothing to do with the variations in force which murmurs may undergo, as a result of changes in vigour of the force developing them.

THE DIASTOLIC MURMURS OF MITRAL STENOSIS.—If the view that the crescendo murmur of mitral stenosis is caused during the earliest phase of ventricular systole be accepted, the methods of causation and the positions in the cardiac cycle of the true diastolic murmurs of the same affection must be reconsidered.

There are two of these diastolic murmurs in mitral stenosis, which may be called early and late.

1. *The early mitral diastolic murmur* appears after the second sound of the heart, but in my experience there is always a short sound-free interval between the sound and the murmur (Fig. 1). The murmur is short and decrescendo in vigour and often of descending pitch. It occurs during the earliest phase of active ventricular diastole, and is caused by blood rushing through the stenosed valve into the dilating ventricle, under the forces of the pulmonary blood pressure and of the aspirating ventricle. The diminishing pitch, when it occurs, is caused by the blood rushing through the stenosed valve whilst it is opening up after having been closed by ventricular systole. The method of causation of this murmur, therefore, may be the reverse of that of the crescendo murmur.

The murmur is probably caused by the development of sonorous vibrations in the stiffened rigid valve, and to a less extent by the causation of a fluid vein within the cavity of the left ventricle.

This early diastolic murmur admits of another explanation, especially when the stenosed valve is incompetent and allows of more or less free regurgitation into the auricle. Under such conditions I believe that an abnormal amount of blood collects in the left auricle at the end of the ventricular systole, partly as the result of incomplete emptying of the cavity by auricular contraction, and partly in consequence of the regurgitation of blood into it during ventricular systole; and into this quantity of blood, more blood, under abnormally high pressure, is poured from the pulmonary veins; and before ventricular relaxation has begun, or at any rate has drawn much blood from the auricle, sonorous fluid veins develop in the auricular cavity and produce a faint, evanescent, early diastolic murmur. This murmur appears very soon after the second sound of the heart is heard, and either disappears quickly as the blood in the auricular cavity, is sucked into the ventricle, or else is continued by a murmur developed by the passage of blood through the narrow orifice, in the manner previously described.

2. *The late, or mid-diastolic murmur* in the "presystolic" theory nomenclature, occupies the second portion of diastole of the ventricle, following, and being often apparently continuous with, the early diastolic murmur just referred to. It occurs at the time of, if it is not actually produced partly or wholly by, auricular systole, and could be rightly termed an auricular-systolic murmur, at any rate

from a rhythmic point of view. It is caused by the combined forces of the blood pressure in the pulmonary circulation, the aspirating power of the dilating left ventricle, and, in those cases in which the auricle wall is not atrophied, of auricular contraction, driving blood through the stenosed valve whilst its orifice is gaping as widely as possible. The murmur is consequently of uniform pitch, but may be of decrescendo force, owing to the natural failure of the above forces, especially of the contracting auricle, when the ventricle is becoming distended with blood and offers most resistance to their action.

The method of production of this vanishing murmur of decrescendo force, but of uniform pitch, is comparable with the causation of the ordinary prolonged systolic murmur of aortic stenosis, which fades with the termination of ventricular systole.

When the auricular wall is hypertrophied and is acting vigorously, this late diastolic murmur may, with unimpaired intensity, run into and be continued by the true crescendo murmur, which it always precedes in rhythm. It is sometimes continuous with the early diastolic murmur which precedes it, and this compound murmur may be continued uninterruptedly by the crescendo murmur, thus forming one long bruit, extending from the second to the first sounds of the heart (Fig. 1 F, G).

As in the case of the early diastolic murmur, the sound of this later diastolic bruit probably results from two physical causes—(1) The rush of blood through the narrow orifice, throwing the stiffened curtains into sonorous vibrations; and (2) the development of fluid veins within the ventricular cavity. This last process is to my mind probably of more importance in the causation of the later diastolic than of the early murmur, for we have a much larger mass of blood in the dilating ventricle towards the end of its diastolic phase than at the beginning; and it seems very probable that the small stream of blood which is forced under some pressure by the systole of the auricle into this mass of blood will produce an audible fluid vein. The sound of this vein is readily transmitted to the apex beat of the heart by the medium of the fluid in the distending ventricle. Such a fluid vein murmur would die away with the failure of the force which propels the stream of blood through the stenosed valve.

THE DIASTOLIC MURMURS OF MITRAL DILATATION.—Diastolic murmurs are not uncommonly met with in cases in which the auriculo-ventricular orifices are distinctly dilated, and their valves, in consequence, incompetent. I have already mentioned how a modified crescendo murmur may arise in such conditions, but the origin of a true, non-crescendo, diastolic murmur still remains to be considered. This is, I believe, of fluid vein nature, and arises in consequence of the abnormal anatomical condition of the cavities of the heart met with in cases of ventricular dilatation resulting from various causes, in which the diastolic murmur occurs.

In these cases, the cavities of the auricle and ventricle are much dilated, and their walls are thinned, and of very deficient power. Moreover, their common orifice, which in health is of much smaller area than that of the transverse plane of the distended cavities, becomes much enlarged, rendering its valve quite incompetent, and allowing the cavities of the auricle and ventricle, which it ought to keep separate, to form during the phase of ventricular diastole practically one large, common intracardiac cavity. Owing to the imperfect contraction of the ventricle and the very incomplete expulsion of blood into the aorta, and to the free regurgitation into the auricle, which result from ventricular systole, there is at the beginning of ventricular diastole a very large abnormal quantity of blood in the common intracardiac cavity. Into this mass of blood more blood is poured from the pulmonary veins, and a condition of affairs is soon reached which is favourable to the development of sonorous fluid veins. These are set up by the rush of blood through the orifices of the pulmonary circulation, under often abnormally high pressure, into the blood in the auriculo-ventricular dilatation. The blood in this two-chambered cavity is in an abnormal state of rest, as the walls of the auricle and ventricle, through disease, have lost respectively most of their propulsive and expansile power. It is under these conditions, then, that I believe sonorous fluid veins may arise during the phase of diastole of the ventricle, and give rise to the ordinary decrescendo diastolic murmur met with in cases of cardiac dilatation, resulting from primary muscle failure, adherent pericardium, aortic incompetence, etc. The murmurs so produced may be of varying length.

Difference between aortic and mitral diastolic murmurs.—The diastolic murmurs of mitral and aortic disease, besides being often alike in nature, may both have their point of maximum intensity down the left edge of the sternum, about the level of the fourth rib. This leads to a difficulty in diagnosing the origin of the murmur in the more obscure cases. But there is, I believe, a reliable diagnostic point of difference in the rhythm of the two murmurs in question. I have for some time noted that the mitral diastolic murmur never follows so quickly on the termination of ventricular systole as the aortic regurgitant murmur may do, but that there is always a distinct sound-free interval after the second sound of the heart, or after a systolic murmur, as I have indicated in the diagram of the mitral murmurs (Fig. 1, F, G).

On the other hand, the diastolic murmur of aortic origin, as a rule, follows so quickly after the termination of ventricular systole, that it appears to continue the second sound, when audible, into a murmur, or to be continuous with a systolic murmur, forming a "to-and-fro" or "see-saw" bruit. The diastolic murmur of mitral origin very rarely, if ever, continues the regurgitant systolic murmur in a "see-saw" fashion. But I may point out, as a further analogy between the methods of causation of aortic and mitral stenosis murmurs, that a true auricular-systolic direct, *i.e.* late diastolic, murmur may be continued uninterruptedly by a regurgitant murmur produced at the same orifice.

There seems to be a simple reason for this delay in the appearance of a mitral diastolic murmur. An incompetent aortic valve is only kept from leaking and producing a regurgitant murmur by the blood pressure in the ventricle being greater than that in the aorta, *i.e.* during part of ventricular systole. But the moment the *vis a tergo* weakens with the completion of ventricular systole, the valves close, the second sound of the heart is heard, and immediately following it, the diastolic murmur caused by the incompetent valve. But whilst the intraventricular pressure falls below that in the aorta practically synchronously with the occurrence of the second sound, there is a pause before the conditions are ripe for the development of the mitral diastolic murmur. If in mitral stenosis, or in cardiac dilatation, this murmur be caused by a fluid vein in the left auricle or common intracardiac cavity, a fraction of a second may elapse before sufficient blood collects to allow of the development of the sonorous vein. If, on the other hand, in mitral stenosis the murmur arises from the blood rushing through the stenosed orifice, a pause always occurs before the ventricle begins its phase of diastolic relaxation, and a further interval may elapse before sufficient force is generated by the aspirating power of the left ventricle, and by the blood pressure in the pulmonary circulation, to drive the blood through the opening, stiffened orifice with sufficient force to generate a murmur. This interval may be more or less prolonged, varying from a very short period of time, as in the early murmurs, to the longer time met with, with later diastolic murmurs.

PERFORATION IN ENTERIC FEVER, WITH NOTES OF TEN CASES.

By George P. Yule, M.B., C.M., *Resident Medical Officer, Monsall Hospital, Newton Heath.*

The marked discrepancy between the clinical symptoms of acute peritonitis, caused by perforation in enteric fever, as usually described, and those of nine (out of ten) cases which I have recently seen, is such as to warrant a short record. The number is possibly too small to draw any definite conclusion as to the train of symptoms in perforation; but, again, it is too large for the similarity in these cases to have been a mere coincidence. The symptoms of the onset of perforation are either sudden or gradual, and, in rare cases, are so masked that the condition is first noted on the post-mortem table. Of these ten cases, only one showed the sudden onset—even this case being doubtful; the others were gradual.

The description of the majority of writers on perforation in typhoid fever is that of acute peritonitis, occurring in an otherwise healthy individual; it does not take into account the modification of the symptoms caused by the fever. That such an alteration, due to the presence of the fever, does take place, is undoubted, and this becomes more noticeable, the severer the attack the further advanced the disease, and the higher the mean temperature. It is this gradual or latent onset of perforation to which I wish to draw attention. In these cases the bowel may have been perforated for several hours without any suggestive symptom being present. It may possibly be that the absence of many of the symptoms is brought about by the blunting of sensation, consequent on the fever process; still, there is another probable factor to be taken into account, namely, the rapid effusion of fluid, produced by the escape of irritating bowel contents into the peritoneal cavity. Comparing the symptoms of acute peritonitis with those of perforation, it may be said that those symptoms which are objective, and have as their cause the presence of inflammatory products in the abdomen, still persist; while those which have their origin in the acute peritonitic pain, are lacking.

The constitutional symptoms are greatly obscured by the fever, and are only noticeable when they are practically of no importance, as they set in when operation is not advisable.

According to Murchison, "the occurrence of perforation is denoted by the sudden supervention of collapse, with or without rigors, but with acute pain and tenderness of the abdomen, which at the same time is tense and tympanitic."

I do not know what exactly is meant by "sudden supervention," but if this means that the collapse appears almost synchronously with the perforation, then this practically excludes the chance of recovery by operation. When collapse has occurred, the other symptoms of acute peritonitis being present, it is comparatively easy, in most cases, to diagnose perforation. The difficulty lies in recognising the condition before the gravity of the symptoms precludes operative treatment. That early diagnosis is possible is shown by the number of successful operations, and the experience of these cases seems to point to a delay of at least eighteen to twenty-four hours before collapse appears, giving ample time for operation, if it is deemed advisable.

Usually the first symptom of perforation is an acute stabbing pain, often accurately localised to the area between the umbilicus and the anterior-superior iliac spine. I have understood this pain as denoting the occurrence of perforation. This initial pain disappears after a variable time. Immediate primary shock was not noted in any of the cases. Hiccough, retching, and vomiting appear either singly or combined, and are often the first symptoms to attract attention, but may be delayed eight, twelve, or sixteen hours. The abdomen, eight to twelve hours after perforation, may show very little tumidity, being soft but doughy; palpation eliciting either no pain or very little. The percussion dulness of both liver and spleen may be present. The change in the pulse is usually very slight, and the patient has no great sense of discomfort, in some cases partaking fairly freely of nourishment. It is probably the absence of pain that accounts for the lack of such characteristic symptoms, as fear of physical examination, anxious facial expression, drawn-up legs, thoracic type of breathing, and hard and shotty abdomen.

As the case advances, persistent hiccough, retching, or intractable vomiting, which may become fæcal, ensues. There is usually obstruction of the bowels. The abdomen becomes greatly distended, very tense and tympanitic (even in the flanks, though fluid is present in large quantity). The liver dulness may partly or entirely disappear. The diaphragm is pushed upwards, dislocating the apex beat, and obscuring the superficial cardiac dulness. Respiration is increased, but shallow in volume. The pulse becomes very small and rapid, the patient cyanosed, and collapse gradually sets in. With the occurrence of perforation, the temperature may show a noticeable rise above the fever pyrexia, or, after an initial rise, a steady fall. Rigors were absent in these cases.

In two cases perforation occurred after the patient had been three weeks in hospital, with a steadily rising temperature from the day of admission; one, on the twenty-first day of a prolonged severe relapse (sixty-one days in hospital); the others, within eight days from admission.

Three cases are submitted, showing that perforation is not always indicated by those well-defined and characteristic symptoms found in the text-books:—

Case 1.—*Perforation, operation, relapse, recovery.*—J. W., male, æt. 37; admitted 15th November 1898. Blood reaction, positive.

History before admission.—5*th November.*—First felt ill; headache; pains in limbs. 6*th November.*—Slight diarrhœa.

State on admission.—Small, fairly muscular. Eyes.—Pupils equal, not dilated. Tongue.—Moist, coated. Abdomen.—Slightly tense; no tumidity; several rose spots. Spleen.—Not palpable; enlarged to percussion. Lungs.—Numerous rhonchi.

16*th November*, 8 p.m.—Complained of a sharp shooting pain below and to the right of the umbilicus, which passed away after a time. Temperature rose from 102° at 6 p.m. to 104°·6 at 10 p.m.

17th November, 11 A.M.—Vomited; tongue dry; perspiring. Abdomen.—Dull pain below and to the right of umbilicus; also, in the loin of the same side. Abdominal walls.—Firm. Pulse, 96; temperature fell from 104°·6, 2 A.M., to 102°, 10 A.M. 1.30 P.M.—Liver dulness completely gone. Pulse 112, regular. No movement of the bowels since admission. 6 P.M.—Vomited several times during the afternoon. Temperature, 103°. Abdomen opened at the outer border of right rectus muscle; incision 4 in. long; immediate escape of gas and opalescent serous fluid; intestinal coils greatly distended; covered with flakes of lymph; perforation, between $\frac{1}{8}$ and $\frac{1}{4}$ in. in diameter, found after a little trouble; peritonitis, general; large amount of exuded fluid. Edges of perforation inverted, and stitched up with a continuous suture. Abdomen syringed out with normal saline solution; large rubber drainage tube introduced into the pelvic cavity; wound closed. Under chloroform, fifty-seven minutes. 7.15 P.M.—Temperature, 103°; pulse, 120.

18th November.—Temperature and pulse steadily fell after operation. 2 A.M.—Temperature, 98°·6; pulse, 80. The temperature remained practically normal till 1st December, when it gradually rose, gradation fashion, to 104°·8 (8th December), maintaining a mean of 103° until 13th December, when it again reached 104°·8, after which it slowly but steadily fell to normal.

26th December.—The chart recording twenty consecutive days of high temperature before the intermittent swing first touched the normal. This was a relapse, rose spots being present in addition to the evidence of a typical chart. The patient bore the relapse, which was more severe and of longer duration than usual, exceedingly well. From and including the day after operation, the wound was syringed with normal saline solution. Drainage tube removed, 30th November; wound healed, 26th December. Patient reclined in bed, 29th December; sat up in bed, 4th January 1899; got up, 6th January; discharged, 20th January. (Sixty-five days in hospital.)

CASE 2.—*Perforation, death forty-nine hours after operation.*—A. O., male, æt. 24; admitted 1st November 1898. Blood reaction, positive.

History before admission.—22nd October.—Headache; vomited; pain in legs.

29th October.—Took to bed; diarrhœa for last two days.

Condition on admission.—Emaciated. Eyes.—Pupils equal, dilated. Tongue.—Large, flabby, moist, coated. Abdomen.—Slightly sunken, soft, numerous rose spots. Spleen.—Palpable on inspiration. Lungs.—Numerous rhonchi. *Previous illness.*—Pneumonia two years ago.

The temperature rose steadily from 102°·2 (1st November) to 104°·4 (14th November); then fell very slowly to 99° (10 A.M., 27th November). There was persistent diarrhœa of, on an average, over four stools per day, from 1st November to 26th November.

18th November.—Numerous crepitations with impaired percussion note, at both bases, posteriorly.

24th November.—Pulse.—Small, rapid, weak; improved rapidly by digitalis and brandy, and remained regular, of good volume though dicrotic till after the operation.

25th November.—Copious hæmorrhage (intestinal).

26th November.—Slight hæmorrhage. Rose spots appeared from 1st November to 27th November.

27th November, 10 A.M.—Temperature, 99°; no diarrhœa; one stool during the last thirty-six hours. Pulse, 92. Abdomen.—Sunken, soft. 9 P.M.—Temperature, 103°·2; pulse, 92; slight abdominal pain.

28th November, 12.15 A.M. — Abdominal pain became suddenly very acute. 6 A.M.—Temperature, 99°; vomited twice. 12 NOON.—No pain. Abdomen.—Tumid, slightly tense, doughy. Liver dulness present; diarrhœa ceased; no motion. Hiccoughing occasionally; no vomiting since 6 A.M. Firm palpation of abdomen elicits no pain; asked as to how he felt, patient smiled, and said he was much better. 7 P.M. — Hiccoughing, vomiting. Abdomen. — Very tumid, tense, tympanitic. Liver dulness partly gone; splenic dulness gone. Flanks resonant. Diaphragm pushed upwards, displacing the apex beat. No movement of the bowels. Face.—Sunken, if patient's attention is not roused. During the day had milk, 1 oz. hourly; brandy, half-hourly; strychnine, four-hourly, without sickness. 12 MIDNIGHT.—Incision, right linea semilunaris. On opening the peritoneum, thin serous pus welled out, greyish flakes of lymph covering the intestinal coils. Perforation found with ease by tracing the small gut from the cæcum. Perforation double, closed by Lembert sutures, abdomen flushed out with normal saline solution, large rubber drainage tube introduced into the pelvis; wound closed.

29th November, 10 A.M.—Temperature, 97°·2; pulse, 98; respiration, 32; one slight liquid stool. 3 P.M.—Pulse, 130; respiration, 50. From this time patient gradually sank, dying at 2.20 A.M. (1st December).

Post-mortem.—Peritonitis, general; intestinal coils in cæcal region thickly covered with lymph. Perforation found adhering firmly to the parietal peritoneum. Omentum glued to intestine over ulcers at several places. Numerous healed ulcers; towards the ileo-cæcal valve ulceration exposed the peritoneal coat. One suppurating mesenteric gland.

CASE 3.—*Perforation, death.*—I. O., male, æt. 23; admitted 4th November 1898. Blood reaction, positive.

History before admission.—*25th October.*—Headache, abdominal pain.

1st November.—Vomiting, backache.

Condition on admission.—Eyes.—Pupils equal, dilated. Tongue.—Dry, tremulous. Abdomen.—Several rose spots. Spleen.—Palpable. Temperature gradually rose from 102° on admission to 105° before death (21st November). No diarrhœa throughout.

10th November.—Passed several blood clots. Rose spots appeared intermittently till 18th November.

20th November, 10 A.M.—Acute pain localised to the region of M'Burney's point. Liver dulness partly gone; splenic dulness present; abdominal walls soft. No change in the pulse (100). 6 P.M.—Abdomen.—Tumid, but still soft; liver dulness gone. No pain on palpation. Says he is "quite comfortable." 10 P.M.—Pulse, 140, small and thready; temperature, 105°; appearance, cyanosed.

21st November, 1.30 A.M.—Two profuse hæmorrhages; vomiting. 10 A.M.—Still vomiting; pulse almost gone; died, 1.50 P.M., 21st November.

Post-mortem.—Abdomen filled with a thin yellow pus. Large amount of lymph covering the gut, particularly about the perforation. Omentum firmly adherent to intestine, at three places, over the ulcers. Last two feet of ileum form one almost continuous ulcer, with at places only the serous coat between the lumen and the peritoneal sac. Large amount of blood in the intestine.

SUPPURATIVE CHOLANGITIS.

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The recent success in the surgical treatment of affections of the bile ducts tends to enhance their interest, and likewise to emphasise the importance of their early recognition. The case herewith appended has been deemed worthy of report by reason of the irregular features it presents. It may therefore be not out of place to recapitulate briefly the cardinal signs and symptoms of cholangitis. Of this disease there are two varieties, infective and suppurative, and it is with the latter we have to deal. As etiological factors we have the following diseases—cholelithiasis, hepatic tumours, malignant and innocent, enteric fever, infective cholangitis, etc.

The possibility that suppurative cholangitis may supervene upon so common an affection as gall stones adds much to the gravity of the disease.

The classical symptoms are—Progressive enlargement of the liver, with possible distension of gall bladder; jaundice, usually marked and continuous; pain, usually well marked; persistent pyrexia, irregular in type, with rigors; profuse sweatings; and rapid emaciation and asthenia.

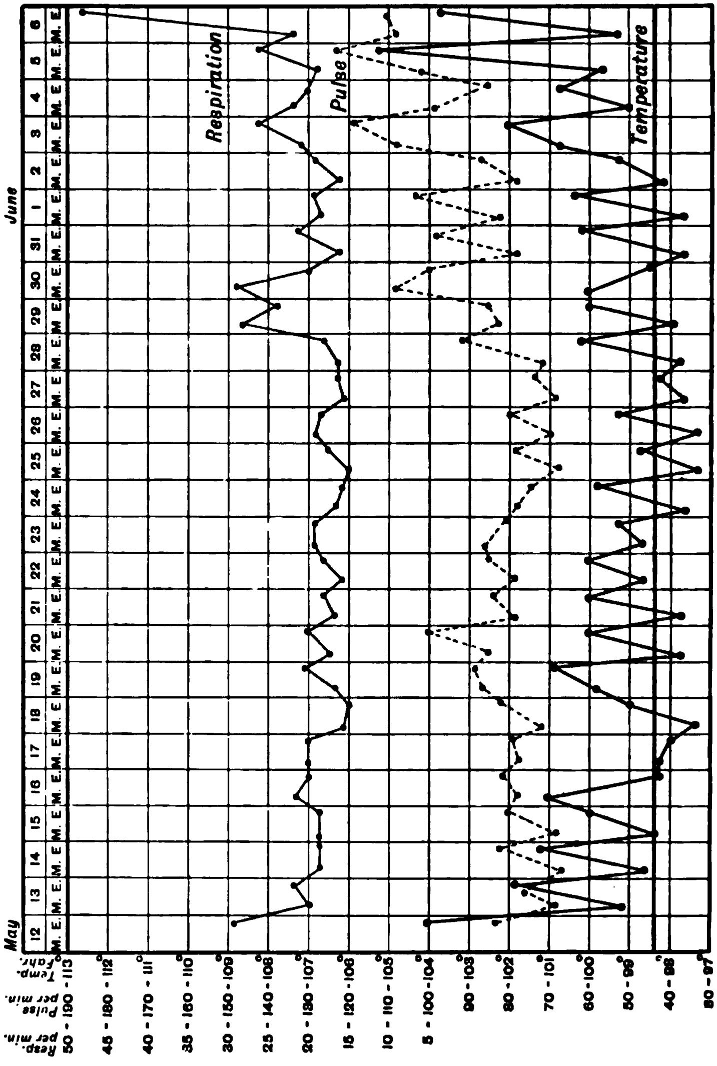
12th May.—Patient was seized suddenly with abdominal pain and faintness. There was intense cyanosis of face and hands. Pulse very feeble and irregular. Respiration shallow, temperature 104°. He was immediately conveyed to bed. Saline diaphoretics and whisky were administered. Throughout the night patient sweated profusely.

13th May.—Present state.—Patient is a thin, spare man, æt. 53. He lies on his back in a state of mental hebetude. His conjunctivæ, in common with the general surface of his body, present a subicteric tinge. His malar eminences are darkly flushed; his eyes lustreless, with moderate-sized pupils; tongue thickly furred; bowels costive. His temporal arteries are tortuous and unduly obvious.

Personal history.—Admitted to the asylum nineteen years ago, suffering from mental depression and doubtful phthisis. His state of health was characterised as delicate. No history of any illness. Family history unknown.

Chest inspection.—Skin yellow. Subcutaneous fat, small in amount. Movements shallow, especially left side; cardiac apex beat, fifth space, just external to nipple line; well-marked epigastric pulsation.

Palpation.—Left lung. Diminished movement. Vocal fremitus increased, especially over lower lobe posteriorly. Cardiac impulse feeble.



Percussion.—Diminished resonance over left lung anteriorly. Posteriorly there was an area of marked dulness fringed by normal resonance.

Auscultation (*left lung*).—Breath sounds weak. Posteriorly, over area of marked dulness above referred to, almost complete absence of breath sounds. Cardiac sounds first reduplicated.

Abdomen.—Movements fairly extensive, no tenderness to be detected. Liver and spleen apparently not enlarged.

Nervous system.—Nothing abnormal.

Genito-urinary system.—Micturition normal. Urine high-coloured; sp. gr. 1025; no albumin, sugar, or blood; indican and bile pigments present.

Progress.—13*th May.*—In the evening enema was administered, and dark brown stool passed.

14*th May.*—Patient perspiring profusely; tongue furred; breath foul; patient is very restless, tossing himself about even in sleep, his limbs being flexed and extended with almost convulsive force; stools clay-coloured; urine contains bile pigments; diet consists mainly of milk, with occasional doses of whisky; bowels are regulated by calomel powders followed by sulphate of soda. Quinine also was freely administered. From now on till 30th May patient became steadily weaker, lost flesh, and grew more lethargic. His temperature curve was extremely irregular; he sweated profusely; his bowels confined; stools generally clay-coloured, occasionally containing traces of normal pigment; urine scanty, high-coloured, containing bile pigments; jaundice was never intense, and varied in degree from day to day. The condition of his left lung was much the same, a few râles were heard at base. A slight cough was present, but no sputum. The heart showed signs of progressive dilatation. Treatment in the main was symptomatic and expectant.

31*st May.*—Loud coarse rhonchi to be heard over left lung. Patient is expectorating blood-stained sputum. Tongue dry, brown fur. A mixture containing quinine, digitalis, ammonia, and nux vomica was ordered.

1*st June.*—Patient is lying in a subconscious state, face cyanotic, breathing shallow; cardiac apex-beat very diffuse; sounds indistinct; the coarse rhonchi above referred to are absent, but there is still diminished resonance over left lung, with feeble breath sounds; no moist sounds detected. From now on till 6th June patient grew rapidly weaker, the profuse sweats and irregular pyrexia continuing. He coughed freely, sputum blood-stained. He became more and more comatose, his pulse more rapid, feeble, and irregular. He refused nourishment. Incontinence of urine and fæces prevailed towards the end.

5*th June.*—Temperature 105°·2, pulse 124. Breathing very laborious. Patient is evidently moribund, and, notwithstanding free stimulation, he rapidly sank, dying 9.5 P.M., 6th June, his temperature at 4 P.M. on the same evening registering 103°.

DIAGNOSIS.—The following are, we think, the crucial points of the case, and it is upon them that any possible diagnosis must be based:—

The extremely sudden invasion, the collapse, the irregular pyrexia, the jaundice, colourless stools, bile-pigmented urine, blood-stained sputum, and the rapidly induced typhoidal condition. The sudden invasion, irregular pyrexia, and drenching sweats seem to announce septic infection, with probable suppuration, while the character of the urine and stools, coupled with the icteric tinge of the body, point to the liver as the primary focus. It now devolves upon us to consider those diseases in which such a combination of symptoms may be found, and the following suggest themselves:—Acute yellow atrophy, ulceration and perforation of gall bladder, phlegmonous cholecystitis, infective and suppurative cholangitis, suppurative hepatitis, Weil's disease, and portal pyæmia.

Acute yellow atrophy.—This disease is excluded by the non-diminution in size of the liver.

Ulceration and perforation of gall bladder, phlegmonous cholecystitis and infective cholangitis.—The acuteness of the onset, severe collapse, and abdominal pain were compatible with ulceration and perforation of gall bladder, phlegmonous cholecystitis, or with the ague-like paroxysms which occur in the course of infective cholangitis; but the non-intervention of acute peritonitis excludes the two first-mentioned, while the absence of any remissions in the course of the disease negatives the latter.

Suppurative cholangitis, hepatitis, Weil's disease.—The non-enlargement of the liver is equally at variance with all these diseases, as far as is ascertainable. Jaundice may be slight in hepatic abscess, but is usually said to be persistent and intense in suppurative cholangitis, though subject to variations if secondary to gall stones. Weil's disease is excluded by the absence of hepatic and splenic enlargement, also by absence of nephritis, nervous symptoms, and hæmorrhages. Portal pyæmia is rendered improbable by the absence of any known focus of infection, the absence of ascites and other results of portal obstruction, *i.e.* hæmatemesis, splenic enlargement, etc.

The evidence, such as it is, is presumably in favour of suppurative cholangitis. As to the cause of the cholangitis, it seems to lie between (1) gall stones, (2) malignant disease of bile ducts, possibly secondary to gall stones. The points not in favour of malignant disease are the paroxysmal nature of the pain, which in malignant disease is either absent, or, if present, persistently severe; with the proviso that if the malignant disease be secondary to gall stones, it may have all the features of biliary colic. Other points against malignant disease are the absence of tumour and the usual distension of gall bladder, as well as the absence of any malignant cachexia, prior to onset of acuter symptoms. The personal history gives no help; there is no account of any previous illness, the patient having worked continuously up to time of seizure. It seems, therefore, very difficult to differentiate the possible causes of the cholangitis.

Re the lung condition, the diagnosis seems to lie between pneumonia, pulmonary infarction, atelectasis, localised pleural effusion. The bloody sputum suggested pneumonia, but its bright red colour was more in keeping with the sputum of tubercular hæmoptysis; other points against it were the absence of tubular breathing and the pulse respiration ratio.

To support the hypothesis of infarct, the possibility of infective organisms, eluding the vigilance of the liver, infecting the endocardium, with sequential pulmonary infarction, was hazarded.

In reinforcement of the suggestion, we have the progressive dilatation of the heart, coupled with the fact that cases of infective endocarditis, secondary to suppurative cholangitis, have been recorded by Jaccoud and Aubert; but the other features of endocarditis were wanting.

The possibility of lung collapse receives support from the fact that Leube refers to its incidence in cases of abdominal distension, *i.e.* ascites, and also in painful affections of the liver and spleen, citing as causes restricted diaphragmatic excursion, combined with fixity of posture; and in this connection he cautions us to be wary of diagnosing cardiac dilatation, as the lung retraction consequent upon the collapse may cause undue exposure of the heart.

As far as ascertainable from text-books, hæmorrhagic sputum does not occur in atelectasis, though the grave general toxæmia, with its attendant depravation of blood, supported by the well-ascertained tendency to hæmorrhages, petechiæ, etc., in hepato-toxæmic states, suggests its possibility. The physical signs were compatible with collapse. Futile aspiration negatived effusion.

In assigning as the diagnosis suppurative cholangitis, we are confronted with the difficulty of the non-enlargement of the liver; and in the presence of these hitherto assumed antagonistic conditions we have to invoke the existence of some condition in the liver which might curb its power of expansion; and the disease that most readily suggests itself is cirrhosis, and the supposition gathers probability from the fact that cirrhosis sometimes follows gall stones (Senator).

The difficulties of diagnosing the case were much enhanced by the mental condition of the patient, who lay wrapped in all the stoical indifference of dementia. To his sufferings he gave no voice, and the existence of pain was inferred from the fitful spasms that swept o'er his limbs from time to time, and disturbed his otherwise inanimate features.

Realising the lethal character of suppurative cholangitis, especially in the light of its sequential relation to so common a condition as cholelithiasis, one cannot but feel that too much pains cannot be taken to correct all tendency to gall stones by all solvent means in our power, and failing this, to avail ourselves of timely surgical intervention in suitable cases.

Inter nos we have often commented on the sensitive relationship existing between states of mind and even functional hepatic disturbance, and, granting the insane diathesis, it seems conceivable that the chronic invalidism and mental worry attendant upon gall stones at any rate might pave the way for, if not actually usher in, the graver degrees of mental alienation.

> "Sweet recreation barred, what doth ensue
> But dull melancholy, kinsman to grim comfortless despair."

AT THE AUTOPSY.—The following notes are abridged as much as possible on account of the exigencies of space:—

Body emaciated.—No distinct icteric tint of skin or conjunctivæ. There is more congestion than usual in the brains of the chronic insane, but the usual evidences of dementia are present.

Chest.—Cartilages rigid but not calcified; heart, pericardium normal; right auricle contains a large mass of pre-mortem clot; tricuspid orifice is dilated; right ventricle is empty and contracted. There is commencing atheroma of aorta. Left lung, no adhesion to pleura; small amount of blood-stained fluid in cavity; upper lobe is slightly emphysematous. From the middle of the lower lobe extending to the lower border is an area of dark purple colour; the bronchioles are intensely blood-stained; pieces from it sink in water, *i.e.* collapse; right lung is very large, is œdematous and congested; pleura is thickened, there is considerable increase of interstitial fibrous tissue, and the bronchi are dilated.

Abdomen.—Liver is soft; capsule is irregularly thickened, especially in the neighbourhood of the gall bladder; this latter is adherent both to the liver and to the duodenum, and contains a calculus about the size of a pigeon's egg, of yellowish colour and finely granular exterior. The bile ducts are dilated, and in the liver substance their walls are thickened and bile-stained; on section, the liver tissue appears dotted over with small pus exuding points generally under $\frac{3}{16}$ in. in diameter. This pus is thick, tenacious, and lightly bile-stained. Congestion, enlargement, and softening of spleen.

Principal weights.—Brain, 52 oz.; heart, 9 oz.; right lung, 32 oz.; right kidney, 5 oz.; liver, 49 oz.; spleen, 5½ oz.; left lung, 17 oz.; left kidney, 4½ oz.

It will be observed that the liver still remains slightly smaller than the brain, in spite of the fact that this latter is considerably atrophied; hence we may conclude that the liver likewise is diminished in size.

Portions of the liver tissue were taken from various parts of the organ; they were hardened in graded alcohols; the principal stains used were hæmatoxylin and eosin, the Weigert-Gram method and methylene blue.

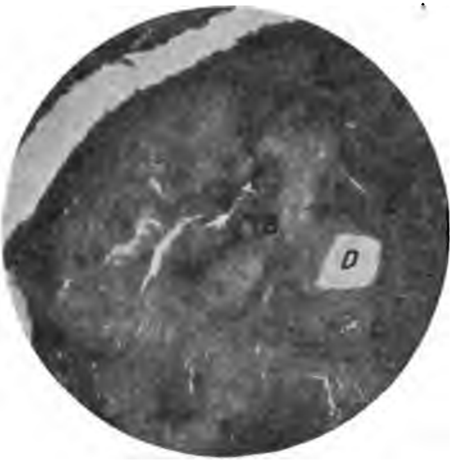


FIG. 1.—A necrotic area, showing—A, bile duct passing towards the centre; B, hyaline artery; C, transverse section of bile ducts, showing folding of mucosa; D, normal portal vein. (× 720.)

Under a low power, or even with the naked eye, sections show small spots, varying in size from ½ to 2 mm.; some of these, the majority, appear much more lightly stained than the rest of the tissue, while others show a peculiar concentric marking, which strongly differentiates them from those first mentioned. On examining the first of these small areas with progressively higher powers, we find that in their centre as a rule are to be found vessels in a state of hyaline degeneration, in the centre of which is usually amorphous débris with perhaps a few nuclei, the remains of the intima; beside these is generally an area consisting of granular débris, in which occasionally may be found a few degenerate-looking columnar cells, the remains of a bile duct which has undergone rapid necrosis. (As a proof that this is so, we may see a bile duct pass from the centre, where it is necrotic, to the periphery, where it is merely catarhal (Fig. 1).) Around their central areas are sometimes found a few engorged capillaries, but for the most part they are closely surrounded by the liver cells in a partially necrosed state.

The nuclei of these cells as a rule stain well; they may be shrunken or swollen, or they may stain a brownish tint or be apparently perfectly normal; the volume of the cell is always greatly increased, and the cell protoplasm is now apparently represented by a loose network, which generally stains a faint reddish purple, while the interstices are occupied by a clear substance of a delicate pink tint. We have observed in a few instances that this protoplasmic network may stain in the same vigorous manner as a nuclear network normally does (Fig. 2).

The degree of degeneration gradually diminishes from within outwards, and on the outer border of the necrotic area we find that the cells are becoming flattened in a concentric manner, showing that in necrosis an actual increase of volume occurs. Around this compressed area is a region of capillary engorgement, but without cellular infiltration. In a few instances the central part of these areas has refused all stain, apparently consisting of a granular network; more careful examination shows that this network is composed of the outlines of cells, and in the centre the nucleus still remains recognisable by its different refractive index, and attached to the cell wall by a few threads of the now almost totally destroyed cell protoplasm.

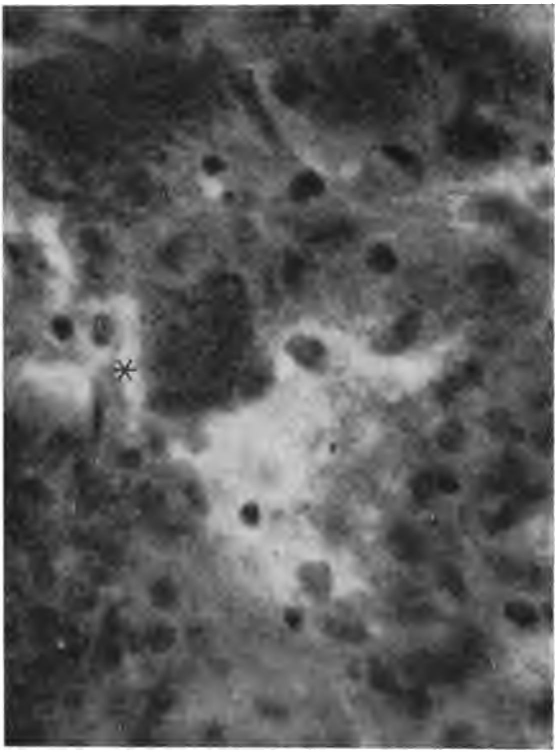


FIG. 2.—Section of edge of necrotic area, showing swollen vacuolated cells. Beside the asterisk are two cells, whose chromatin network has stained deeply like nuclei —an unusual occurrence. (×700.)

The other areas referred to can now be recognised as bile ducts of fairly large size, which are practically abscesses. They are filled with pus, and have lost the epithelial lining; surrounding them is a region of extreme inflammatory reaction, and external to this is a zone of intense vascular engorgement.

The portal spaces are much enlarged by growth of fairly firm fibrous tissue, in which are embedded an excessive number of bile ducts. The increase of fibrous tissue is as a rule confined to the portal space itself, the tissue beside it is gradually destroyed, islets of liver cells being occasionally to be found in the borders of the space. In a very few instances a lobule may be enclosed by fibrous tissue.

The cells of the mass of liver tissue show cloudy swelling, and there is evidence of a good deal of chronic congestion, in the shape of dilated capillaries, and of atrophied and pigmented cells; sometimes small round green globules are found in the cells. The old bile ducts, when retaining their epithelium, contain cast-off cells and débris; the mucosa is folded and villous, though never to the degree described by Rolleston and Pigg (1), and the cylindrical cells are frequently gone from some part of the wall (see Fig. 3). In the immediate neighbourhood of the duct, the fibrous tissue is probably œdematous (*i.e.* is clear and translucent, and does not stain vigorously), but beyond this the tissue of the portal space is commonly infiltrated with leucocytes for a moderate distance; similar infiltration frequently occurs apart from the neighbourhood of a bile duct. The endothelium of the hepatic arteries is frequently detached and separated from the subendothelial intima by red corpuscles, this change appearing possibly to be an early stage of the hyaline degeneration already mentioned. The veins never show any changes of importance.

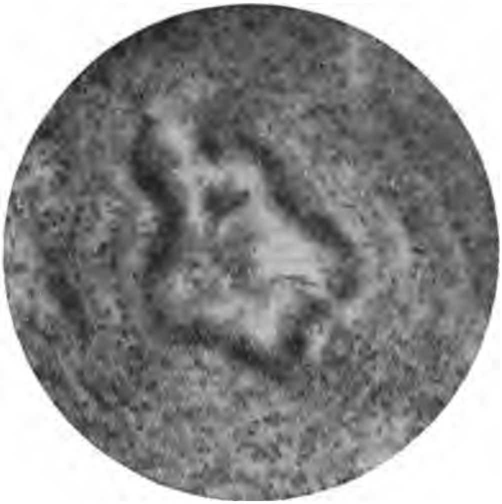


FIG. 3.—Transverse section of bile ducts, showing catarrhal contents and highly embryonic state of the surrounding connective tissue. (×150.)

The tissues were examined bacteriologically, but the results have not been sufficiently conclusive and satisfactory to merit publication.

The existence or not of a real biliary cirrhosis is one that is still undecided; the type usually described as biliary is that known as the monolobular form, the characteristics of which are too well known to need recapitulation here. The present case differs in a not unimportant manner from this type, while it is still further removed from any other. Microscopically, as a result of the repeated biliary colic which must have occurred repeatedly but unobserved from the mental state of the patient, we find adhesions binding the gall bladder to the liver, and to the duodenum ([2]). The perihepatitis in the neighbourhood gives corroborative evidence of the long duration of the disease. The liver is not increased in size, its surface is smooth—points at variance with monolobular form (though often in this form the surface is smooth). Microscopically, we find the increase of fibrous tissue is confined to the portal spaces, and only with extreme rarity are bands of fibrous tissue sent forth enclosing lobules or collections of them. There is, however, increase of the bile ducts, which, as in the classical form, run parallel with the edges of the portal spaces. As the immediate cause of this localised condition, we have the catarrhal condition of the bile ducts, which has probably existed many years. Apart from this, there is no other known cause, the residence of the patient so long in the asylum completely excluding alcohol. Ligature of the bile ducts has experimentally led to contradictory results with regard to the production of cirrhosis ([3]), but in the present case complete obstruction can have rarely if ever occurred, on account of the size and shape of the stone. Whether the biliary catarrh is antecedent to the calculous formation, or subsequent to it, cannot be definitely cleared up, as it may be accepted that secretion of albumin into the bile ducts is the usual cause of gall stones; if the calculus is not the cause of the catarrh which existed at death, it must have much aggravated its course.

As far as we have been able to ascertain, the association of cirrhosis with cholangitis is scarcely recognised; Mayo Robson, in a list of sequelæ to gall stones and to cholangitis, makes no reference to it. Yet it is of great importance, as the enlargement of the liver, which is so constant a sign of cholangitis, is masked by the cirrhosis, and the difficulties of diagnosis are greatly increased. We have seen that the areas of necrosis and inflammation are associated with swelling; and, scattered so thickly as these areas are throughout the organ, it can scarcely be denied, we think, that a real enlargement did occur, but was screened and masked by a previously unrecognised contraction.

The above are some of the principal points which are raised by this rare and interesting case; we have not attempted to discuss points of great interest, which this case has illustrated, of the relation of hepatic disturbance or of pain to mental disease, or of the relation of hæmorrhages to collapse of the lung, or to septic states.

In the bacteriological examination of the tissues, we have been able to obtain the valuable opinion of Dr. Wakelin Barratt, who agrees that the organisms are present, and that they resemble, both morphologically and in their staining reactions, the *Bacillus coli communis.*

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THE HYGIENICS OF MILK.

By W. LESLIE MACKENZIE, M.A., M.D., *Medical Officer of Health, Leith.*

1. *The problem.*—The problem of milk-supply is: To bring to the consumer clean, harmless, palatable cow's milk. By clean, I mean free from adventitious impurities, such as sand, dust, cobwebs, cow-dung, hairs, epithelial scales, and the like. By harmless, I mean not capable of producing any disease, infectious or other. By palatable, I mean not so altered from the natural flavour of wholesome milk as to disgust. Other qualities of milk are equally important, from other points of view. For example, as a matter of therapeutics, the percentage of butter-fat may be more important than the absolute freedom from dirt; or, again, fat may be of less importance than the readiness to decompose. But those qualities are only indirectly, not directly, questions for hygienics; because hygienics, which here practically means the scientific care of the human environment, concerns itself with the reduction in number of abnormal factors. Dirt, disease, and the consequent decompositions of milk may destroy it as a possible human food, so throwing it out of relation to the physiological needs. These three, therefore, it is the first duty of hygienics to eliminate. Practically, therefore, the problem is how to eliminate dirt and disease, how to prevent unintended decompositions, and how, thus, to preserve in its full physiological relations, a food of immense value.

2. *Assumptions preliminary.*—In thus placing the problem, I make certain assumptions. Among other things, I assume that milk is a highly important factor in the food environment of our present highly complicated society; that our present methods of providing the consumer with milk are full of defects; that the rectification of these defects is an entirely practical enterprise.

3. *Milk dirt, germinal and non-germinal.*—Under dirt, I include all the non-pathogenic germs; for these, though they do indirectly encourage specific diseases, are not individually

associated with any particular disease. They affect seriously the "keeping" qualities of milk. They have, therefore, pre-eminently a bearing on commercial, and, by consequence, on practical management. They are the pest of the small or the town dairy and the minor shopkeeper. They are the quintessence of uncleanness. They are, however, in part, useful in the production of milk products; but in "market" milk, as it goes forth for consumption *as* milk, they are nothing less than destructive ferments. The germs, or micro-organisms, I mean are mainly these:—The lactic ferments (including special bacilli, micrococci, and streptococci, in many varieties), *Bacillus coli communis* (in some of its "races"), the casein ferments, the blue milk bacillus, the red milk bacilli, yellow milk bacilli, bitter milk bacilli, the organisms that produce slimy milk, or stringy or soapy milk. Besides these, there are the yeasts and other moulds.[1]

To protect milk from most of these is not very difficult; but the care necessary is more than will ever be systematically taken by any but the most scrupulous dairyman. In the cleanest byres I have ever seen, where, too, the cows were well groomed and looked it, the chances of germ pollution were more than could be readily calculated. There are always at least the following germ-yielding conditions:—Dry hay or other fodder, dust from roofs, dust from byre floors, moulds, dried excreta, decomposing urine, the micro-organisms of the cow's hide, the innumerable germs of ordinary water, the repeated contacts with human hands and clothing. The cows lie down clean; they rise up dirty. To watch the milking of cows is to watch a process of unscientific inoculation of a pure (or almost pure) medium with unknown quantities of unspecified germs. Perhaps feeding is just over, or the cows are fresh in from the field; or, as in town byres, they were in some fields six months ago, and have never seen clear sky-light again. In comes the milker, man or woman, slaps the cow's buttock to make her rise. The milk-stool is placed—taken from some dirty corner of the byre. A few squirts of the teat, or of two teats, are given as a preliminary encouragement to the cow and a convenient lubrication for the fingers. Incidentally, the first squirts may help to clear out any micro-organisms that lodge in the ducts. The hands may be clean—or not. The clothes may be newly washed—or not. The nose, the mouth, the eyes, the ears, the face generally, the hair, may all have been specially cleaned just before—or not. Whoever knows the meaning of aseptic surgery must feel his blood run cold when he watches, even in imagination, the thousand chances of germ inoculation. From cow to cow the milker goes, taking with her (or him) the stale epithelium of the last cow, the particles of dirt caught from the floor, the hairs, the dust, and the germs that adhere to them.

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[1] Ed. von Freudenreich, "Dairy Bacteriology."

Meanwhile, what with switching of cows' tails, what with stamping of feet, the byre is in a state of persistent agitation. The cows are feeding. The imprisoned dust of the dry fodder is scattered to the air currents. Meanwhile, too, the other milkers are collecting the milk. They perspire. They transfer the milk from pail to can. They leave the total to gather more germs and dust. Perhaps, the moderately careful dairyman sieves the milk roughly from the pail; but the sieve is not such as to enmesh any but the major particles. Everywhere, throughout the whole process of milking, the perishable, superbly nutrient liquid receives its repeated sowings of germinal and non-germinal dirt. In an hour or two, its population of triumphant lives is a thing imagination boggles at. And this in good dairies. What must it be where the cows are never groomed, where udders are never washed, where teats are never rubbed, where the byres are never even approximately cleaned, where ventilators are never open, where the dung is a stale heap at the byre-door, where the pigs are a few feet away, where cobwebs are ancient and heavy, where the ammoniacal emanations of decomposing urine nip the eyes, where hands are only by accident all washed, where heads are only occasionally cleaned, where spittings (tobacco or other) are not infrequent, where the milker may be a chance-comer from some filthy slum,—where, in a word, the various dirts of the civilised human are, at every hand, reinforced by the inevitable dirts of the domesticated cow?

Are these exaggerations? They are not. I could name, for town and county, many admirable byres, where these conditions are, in greater or less degree, normal. But a general statement of germ-yielding conditions were incomplete without some quantitative confirmation. Here are a few figures from reliable sources.

Dr. Edward von Freudenreich says: "In Berne I have found on an average 160,000 to 320,000 bacteria per cubic inch in fresh milk; while Cnopf in Munich estimates the number at 960,000 to 1,600,000 per cubic inch, *i.e.* 33 to 56 millions per pint."[1] Again, he found that a "sample of milk containing 153,000 bacteria per cubic inch," on being kept at a temperature of 59° F., had, after an hour, 539,750 per cubic inch; after four hours, 680,000; after nine hours, 2,040,000; after twenty-five hours, 85,000,000. In other instances, the increase was even more striking, the temperature being higher. By the time milk usually reaches the consumer in this country it is certain to contain some millions of germs in each cubic centimetre. Later, I shall indicate the results of "Pasteurisation." Here another single instance will serve. Mr. H. L. Russell, of the University of Wisconsin Agricultural Experiment Station, found that "a gelatin plate exposure made in the stalls during the feeding showed that over 160,000 organisms were deposited in a minute on an area covered by an ordinary milk pail."[1]

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[1] "Dairy Bacteriology," p. 37.

So much for germinal dirt. Non-germinal dirt has less significance in itself; for its principal effects are essentially those of the adhering germs, and these effects have already been generally indicated. Yet the dirt, apart from the germs, is not unimportant either in amount or in kind. The consumer wishes to consume milk approximately as it comes from the cow. What I have already stated demonstrates how difficult it is, by current methods, to obtain such milk; but to realise how much the dirt, germinal and non-germinal, amounts to, one must examine actual specimens. Let one spend half a day or so at a creamery where, say, 6000 to 10,000 gallons of milk are dealt with between 7 A.M. and 4 P.M. Let him watch the milk as it is poured, apparently pure, into the mixing vat. Let him then watch the scraping of the separators at the end of the day. To strip off the tough, elastic layer from the metal, it is necessary to use a strong spatula. This layer, which is driven on to the wall of the separator by the centrifugal force due to about 6000 revolutions a minute, is made up of hairs, dust, cobwebs, pieces of straw, particles of cow-dung—scraps, in fact, of every sort possible in a byre. These varieties of dirt are bound in a matrix of mucus, epithelial scales, and such other slimy matter as may be separated from the milk. Experiment has shown that milk bacteria are perceptibly reduced in number, but not entirely eliminated, by the process of centrifugal separation, and probably the germ population of the inspissated débris described is proportionally greater than in corresponding volumes of milk. But even if the germ population of the milk is not very seriously reduced, the milk is made more limpid, and consequently more palatable. At the very least, the consumer does *not* want to consume either cow-dung or cobwebs; hairs *might* be, if they are not, filtered out by the ordinary sieves; epithelial scales and minute amounts of mucus are neither here nor there. But, for my own part, ever since I first saw and realised the amount of this lining deposit, which not infrequently is a hard ½ in. thick, I have never been able quite to *feel* that "unseparated" milk is as clean as "separated." Partly this is, no doubt, a prejudice, or rather a revulsion, due to seeing facts out of relation to the whole they belonged to. For, in a drinkable quantity of milk, the few epithelial scales, broken hairs, straw particles, or particles of organic dirt, would not, for the moment, appreciably alter its physical qualities. But, as I have shown, germinal dirt affects the "keeping" qualities of the milk, and the germinal is not in fact separable from the non-germinal. As we shall see, the milk is heated before, and cooled after, separation; and it is true that, with no further treatment, separated milk "keeps" longer than non-separated. Whether it be that the removal of the non-germinal dirt removes mechanically a large percentage of bacteria, or that the dirt removed reduces the nutrient quality of the medium, I am unable to say. Perhaps, independently of either alternative, the flavours of the milk are improved by the removal of foul organic substances that would normally form excellent material for putrefactive bacteriolysis. One fact the "separation" demonstrates; namely, that the ordinary byre or dairy sieve (or filter) does *not* remove any but the major varieties of dust particles. The question—who wants separated milk?—I discuss later. Personally, my answer is: "I do. I can mix it with separated cream if I choose. I *prefer* my milk clean, just as I prefer my meat, or my fish, or my vegetables. I do not consume my meat as it leaves the slaughter-house, nor my fish as it leaves the fishing-boat, nor my vegetables as they are dug from the soil. Neither do I *wish* to consume my milk as it comes from the byre. In every case I *prefer*—I cannot always ensure—that the dirt and the food shall be "separated." I have another alternative—not to drink milk at all, or to consume it only when cooked. Cooked dirt is always more tolerable than uncooked."

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[1] *Sanitary Journ.*, No. 39, N.S., p. 125.

In thus painting, with a broad pencil, the dirt conditions, I have omitted the aggravations due to ulcerated teats, inflamed udders, pustular conditions, and the like. To realise what these amount to, one must examine a few large dairies. For curiosity, I have sometimes gone round a byre of a hundred cows or more. The percentage of abraded teats would astonish any but a practised milker or inspector. This is easy to explain. To begin with, the conditions of the milk cow are largely pathological. The appropriation of her by the human milker compels certain modifications and adaptations. The ever-renewed dragging at the teats leads to hypertrophy, congestion, and increased vascularity. Normally, the teat is tender, easily abraded, easily inflamed. If the cow is on pasture, she may have the teats scratched or pricked by thorns, whins, brambles, or the like. If she is, as commonly in towns, entirely confined to the byre, coarse bedding, bad floors, or the innumerable accidents of movement, may irritate or injure. Obviously, want of protection exposes the teat to many injuries. Twice or three times a day, these injuries are aggravated by rough mechanical handling. The gentlest human hand hardly matches the "toothless gums" of the calf. And the hand of the milker is, as a rule, far from gentle. I speak from observation of a few scores of dairies in Galloway, where the science of dairy manufacture is, perhaps, further advanced than in any part of Scotland. At least half of the milkers, if not more, are men, who, in their ordinary labour, develop the normal "horny hand," and cannot divest themselves of it at the moment of milking. And the hands of the women-milkers are not softer and not often cleaner. In a Wigtownshire farm—and this is only a type—I have known ten cows out of forty, or thereby, affected with so-called "cowpox."

(I say "so-called," because the pathology in this disease is a *very* open question.)[1] In the affected cows, the teats—one or more—were ulcerated. When the scabs were detached, the ulcerated surface was seen to vary from a quarter of an inch square to an inch or more. At every time of milking, the teats were drawn until they bled; the scabs were in whole or in part torn off, and the raw surface was left to take care of itself. In one or two instances, minute vessels spouted for a considerable time after these rough attentions; you could tell the cow by the large spots of blood on the floor. By next milking, the teat had partly recovered. Perhaps, to rest the wound, the cow was milked but once a day. In any case, the pain of the retained milk would distress the cow more than the pain of the rawed ulcer. So the milking was continued; the ulcers became more or less chronic and indurated. Meanwhile, some eleven milkers were affected. Some were too ill to continue at work. Others tied up the affected hand and milked with the other. At least, that was their story to me. The history of the sores—vesicle, areola, induration, ulceration, cicatrisation, pain, swelling, fever—is not here important. What is important is this: the milkers went from cow to cow, probably thus spreading the disorder from unhealthy to healthy. They *said*—but what they said was not evidence—that the affected cows were milked last. This was the only concession to cleanliness. But, except from the two worst cows, the milk went to market. The milkers assured me that they had often seen ulcerated teats like these, and thought nothing of it. Yet, so far as I am aware, no order of any kind enables any officer or local authority to deal with the abominations I have sketched. Then, in calving time, the puerperal cows occupy the same byres as the non-puerperal. In such a time, the increase of organic putridity must be enormous. But the milk market is unaffected, except for the increased quantity of milk. One could add to these indefinitely. I mention only things visible in ordinary dairies. Then who shall enumerate the passing ailments of the milker? Coryza, sore throats, ranging from evanescent redness to complicated diphtheria, inflamed fingers, blepharitis, conjunctivitis, acne, ringworm, eczemas in their multitude, and the large range of minor diseases that are more or less septic in their effects. Bronchitis and the like, I may leave alone meanwhile. When these commonplace facts are clearly grasped, and set coherently in the imagination, they teach one to estimate how much the "lime-washing in April and October" has to accomplish; how ridiculously futile are the efforts at enforcing cleanliness, when every movement means more dirt; how miserable, on the outside of the disgusting facts, are the provisions for lighting, ventilation, and cleansing. These provisions, however, are not without value; for they are forcing into prominence the minimal conditions of wholesome byre management. These conditions I discuss later. Here I re-emphasise the present inevitable pollution of milk. Market milk is normally dirty; that is my first proposition, which anyone with eyes to see may verify any day. Germinal dirt, however, is of service in the production of the fermentations necessary for butter, curd, cheese, etc. Non-germinal dirt, so far as I have learned, is, except as a bearer of the other, of no service whatever. The day is fast coming—it is, indeed, come—when the germinal dirt can be controlled and regulated to a nicety. When the control of the germs goes far enough, the non-germinal dirt will be entirely extruded. Meanwhile, for the purposes of bringing to the consumer milk for consumption *as* milk, both germinal and non-germinal dirt must be controlled, or, when possible, extruded. Before proceeding to the methods of control or extrusion, I must detail some dangerous varieties of germinal dirt, namely, the specific infections. These are, in the common estimate, properly isolated as dangerous. They have therefore received the lion's share of attention; they form a good standard for fixing the safety of milk, and they demand a precise place here.

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[1] See Creighton, "Cowpox and Vaccinal Syphilis"; Jenner on "Vaccination."

4. *The dangerous milk infections.*—The infections of milk are as numerous as the specific germs that will grow in milk. But, for practical hygienics, the important infections in this country are the following five:—Tuberculosis, anthrax, diphtheria, typhoid fever, scarlet fever.

(*To be continued.*)

CLINICAL LECTURE ON A CASE OF TYPHUS, AND SOME LESSONS TO BE DRAWN FROM IT: BEING ESPECIALLY A PLEA FOR THE INCREASED APPLICATION OF BACTERIOLOGICAL SCIENCE TO PRACTICAL MEDICINE.

By W. S. GREENFIELD, M.D., F.R.C.P., *Professor of Pathology and of Clinical Medicine in the University of Edinburgh.*

GENTLEMEN,—I wish to bring before you to-day some incidents in relation to a case of typhus, and to try to draw from them some lessons which may be useful to you in practice.

On visiting my wards on Saturday, February 18th, I found a case of typhus which had been admitted to the small ward on the previous day, and had remained unsuspected in the ward with two other patients, a man of 54 and a boy of 10.

The history of the case, the general condition of the patient, half-delirious, with marked subsultus, dry brown tongue, and well-marked and widespread rash, indicated that it was a severe case, probably at about the sixth day; and I immediately ordered his removal to the City Hospital, the evacuation of the ward, thorough disinfection and cleansing of the ward and of all articles in it, including the clothes and effects of the other patients, as well as other measures to prevent possible risk of spread.

Before going further, let me say that there were some remarkable features about this case, which in the absence of rash might have made the diagnosis difficult—especially to anyone not practically familiar with the disease.

One was that the man actually walked to a dispensary, and thence to the Infirmary—an occurrence so rare at that period of the disease, that Sir W. Jenner said he had never known anyone do it except Sir George Buchanan, the late Medical Officer of the English Local Government Board, when a physician to the London Fever Hospital.

In a man half-dazed, as if with drink, covered with dirt and vermin, as the people who especially suffer from typhus often are, it may be impossible to decide, until after careful observation, if the case is typhus or no—especially at night.

As in most other diseases, there is a peculiar grouping of conditions which is easy to recognise, or which will at least put you on your guard in well-marked cases; especially if you have sat all night (as I once did when Dr. Wilson Fox's resident) by the bedside of a supposed case of pneumonia, taking frequent pulse-tracings to watch the effect of repeated doses of alcohol and other drugs, and when morning dawned saw a lovely typhus rash on the exposed arm. You would be likely to remember the condition and the odour for many a day.

I do not propose to discuss this case in particular, or to consider the differential diagnosis of typhus from typhoid, pneumonia, alcoholic delirium, or other diseases which may be mistaken for it.

I will only emphasise the importance of familiarising yourselves with the characters of infectious diseases, and, what is perhaps of even greater importance, being constantly alive to the fact that at any time you may meet with a case—perhaps the first case—of some infectious disease, and that upon your prompt diagnosis and action the arrest of an epidemic may depend.

All experience of epidemics—of cholera, plague, diphtheria, typhoid, scarlet fever, etc.—shows that it is through failure to suspect the possible nature of one or two early isolated cases, especially in districts where no known danger exists, that epidemics arise. You will find that the early cases, *e.g.* of diphtheria, may be so mild as to pass unsuspected, and the disease as it spreads acquires greater virulence, until it gains a firm hold.[1] In every case in which there is doubt, you are bound to take all measures to make an exact diagnosis where possible; and, until you have made sure, to take all the precautionary measures which you would take if the case were a definite one of the disease. This is not only safest for others, but for the patient. In three cases, widely separated in place and time, in which the patients were being treated as cases of tuberculosis, and in each of which the diagnosis of typhoid was absolutely refused, and the patients were being dosed with cod-liver oil and "fed up," I succeeded in getting a strict typhoid regimen enforced for a fortnight, with the result in each case of speedy improvement and ultimate cure.

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[1] See, *e.g.*, "Diphtheria, its Natural History and Prevention," by Sir R. Thorne, K.C.B., F.R.S., Milroy Lectures, 1891.

Fortunately, in some diseases, *e.g.* typhoid and diphtheria, and cholera and plague, bacteriological science has furnished means of rapid and almost certain diagnosis. And in typhus we may hope that the recent researches of two of our graduates will give a like result.

You will find it necessary in many cases to avoid expressing your suspicions to the friends, until you have made a definite diagnosis. You can usually ensure the necessary precautions without exciting alarm, and it is only where you fail to do this that you need take stronger measures. I grant that there is often difficulty, for nothing excites greater anxiety than the suspicion of an infectious disease, and sometimes strong resentment is felt or expressed if you even hint at the possibility of some serious infectious disease in a member of the family.

What measures, then, should you take to ensure the safety of your patient and of his friends? In this age of bacteriology and of sanitation, with epidemic and infectious diseases of all sorts constantly with us, with the enormous attention which has been paid to the life history and modes of diffusion of contagia, and to the agencies which counteract them, it is astonishing that there should be any need to ask this question. And it is because I find that there is widespread absence of information on these points even amongst able and experienced medical men, and far more amongst the laity, and because on all hands there has been forced upon one the fact that in institutions in which every other modern advance of medical science is applied, there is a disregard or ignorance of common elementary facts, that I cannot assume that you will be ready to deal with such matters when launched in practice, as so many of you will soon be.

I am frequently asked what to do in dealing with cases of infection in a house, even by some of my most experienced and able medical brethren. And if I were to tell you of facts which have come under my observation, illustrating the actual conditions in some public institutions, I should certainly surprise you.

In clinical lectures we usually deal with the diagnosis and medicinal treatment of diseases. And we assume that all the conditions of sanitation, nursing and feeding, to which we are accustomed here in the Infirmary, will be carried out generally. We are apt to forget that the conditions under which we treat our patients here, and to which a very large part of our success is due, are the result of a combination of conditions provided for us, which you will find it almost impossible to obtain so fully in private practice. The construction and general sanitation, the regulation of diet and of cleanliness, the careful and orderly nursing, are the outcome of long experience, elaborate planning, and constant careful control by the Managers and Superintendents. No expense or pains are spared to ensure the highest efficiency in every respect. And if, in spite of all this, there should be any flaws or deficiencies, we have ourselves to blame for neglecting to point them out and ask for a remedy.

You will find that, however wealthy and intelligent your patients may be, however solicitous their friends that nothing should be omitted which can promote recovery, it will be almost impossible to get such good conditions as are provided here for the poorest beggar.

It will be for you to direct and control all these matters in private practice, to be able to speak with precision and authority on every matter relating to the care of your patients and their families. And I blame myself much that I have too often neglected to give sufficient practical instruction in this essential branch of treatment in my clinical teaching.

In relation to infection, there is a too prevalent error that all matters relating to infectious diseases are especially the concern of the public health authorities; and may be left to them. Now, I wish to point out very emphatically that this is a profound misconception, and one productive of most injurious effects. There are many conditions of infection which sanitary leglislation does not touch. And even in the latest and most advanced legislation in this country, relating to epidemic and infectious diseases, every legally qualified medical practitioner is, in regard to some points, placed in a position in which he is, for his own patients and their friends, not only of equal authority with the public health authorities, but he can by his decision entirely frustrate their action.

Take the matter of disinfection of a house or room in which a case of infectious disease occurs. If you will look at the Public Health (Scotland) Act, 1897, 60 & 61 Vict. sec. 47, you will find that the Public Health Authority can only serve notice on the occupier that disinfection must be carried out within a given time; and that if the person so notified informs the Local Authority that he will cleanse and disinfect, etc., and if this is done to the satisfaction of *any legally qualified medical practitioner*, as testified by certificate from him, the Public Health Authority has no power to do anything.[1]

I need not discuss this matter further, only I may say that the difficulties arising from expense on the rates, from possible claims for damage, and from the enormous difficulty of applying in detail the necessary measures to the varied conditions in every case of infectious disease—not to mention the flaws in the Act, and the cumbrous machinery of the law[1]—all render it certain that no Public Health Authority can undertake anything beyond a general supervision, and special efforts in the more dangerous diseases.

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[1] For the sole exception, see sec. 79 of the Act, in Skelton's "Handbook of Public Health," Pt. i., by Patton MacDougall and A. Murray, 1898, pp. 132–133.

What you have to do with is not merely the responsibility imposed on you by the law, but the protection of those under your care. It is to you, as their confidential adviser, that they will look for detailed guidance and instruction; and if you do not undertake the responsibility, it is upon you and not upon the Public Health Authority that the blame rests with regard to them. I repeat that the Public Health Authority has to do with *public* health; it does not and cannot undertake to fulfil your duties to your private patients, nor can it divest you of the moral responsibility for their welfare. Please understand, then, that in what I say I refer entirely to your action in practical treatment, and not to any question of *public* health or sanitation.

In relation to the whole subject of infection of every kind, I wish especially to enter a plea for the application of the elementary facts of bacteriological science to medicine, not merely for diagnosis, but for prevention and treatment.

Surgery, and to a large extent midwifery, have been revolutionised by the acceptance of these elementary principles. In medicine we lag far behind. There are large branches of medical practice in which we physicians are much in the condition that surgeons were in thirty years ago when Lister was beginning his campaign. I have lived through the days when we regarded pyæmia as a necessary result of some injuries and operations. When in hospitals in Paris it was common to ask of a surgical case, "Has he had his erysipelas yet?" And I remember a time when in the Hôpital des Enfants Malades in Paris, it was assumed that any child would have, in succession, measles, whooping cough, etc., as a necessity. And I happen to have watched the first introduction, and the gradual adoption, of Lister's system in London; and to know how many surgeons, whilst professing to adopt it, neglected the most essential precautions, because they did not understand, or did not believe in, his scientific principles. Nor was I surprised when some gave it up as a failure—and, indeed, their failures were ghastly—and went back to cleanliness and water-dressings.

Now it seems to me that we are in some respects, in practical medicine, far behind the surgeons, and that we need to pass through a period of enlightenment of the same kind, and on the same lines, as those which have made modern surgery so brilliant a success. Certainly you will agree with me that we ought to try to banish septic and suppurative complications in all diseases, and that we need special care in those whose susceptibility is increased by illness, constitution, or age, or by the necessary results of confinement. And I think you will agree that we should especially try to diminish all risks in those suffering from common infectious diseases. For you know that in these—*e.g.* in diphtheria, scarlet fever, and measles, the secondary suppurative catarrhs, glandular affections, and so on—are the result either of so-called "mixed" infection, or of secondary infection with pyogenic organisms or with tuberculosis.

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[1] *Idem, passim.*

I will go further, and say that I cannot share the view that these common scourges of childhood should be regarded as necessary, or made light of. I believe that they might be banished, and ought to be banished, instead of being constantly present and even increasing, a source of terrible suffering and anxiety, of vast expenditure, of great mortality, and life-long weakness.

Let me now return to our case. I gave, as I have said, stringent directions for disinfection, such as might be applied in a case of the most serious infection, and where the case had been long in the ward.

I need hardly say that these directions were far in excess of what was necessary. For, in the case of typhus, we know that the contagium is not nearly so difficult to deal with as that of some other diseases. The infection may probably be carried to a distance by clothes and bedding, but far more commonly comes directly from a patient or corpse. Under ordinary conditions, typhus rarely spreads in a general hospital which is well ventilated and properly nursed. For many years typhus was treated in general hospitals in London in the ordinary medical wards, and for long periods it was common to have at least one case in a medical ward. We took the precaution of putting the patient at the end of the ward, and having one bed vacant alongside. And, in my own experience, I never saw the disease spread to other patients or to any nurse. So strongly was it believed that isolation was not necessary, that early in 1874, in St. Thomas's Hospital, where we had an entire pavilion of the building for infectious diseases, I incurred grave censure from some of the physicians for isolating a very severe case of typhus. Dr. Murchison alone supported me, and predicted a disaster if the usual practice were continued.

But you must remember that some persons are highly susceptible to infection. One of the worst cases I ever saw was in a first year's medical student, whose infection was almost certainly derived from being for a short time in a general medical ward in which there were two cases, though he did not come in contact with them. That case was investigated by Murchison himself, a sufficient proof that the evidence to this effect was strong. And if middle age is a more dangerous age for typhus, from 10 to 20 is a much more susceptible one. Fortunately none of the other patients or nurses took the disease.

But although such thorough measures may not be necessary as in some other diseases, I gave, as I said, the most stringent directions for thorough disinfection. And although I said nothing of my intentions, I did so chiefly because I was anxious to test the completeness of the methods commonly employed, on the same principle as fire-drill is carried out. And, this especially, because some occurrences had aroused my suspicion that there was a flaw somewhere. In particular, some time ago, there were cases of pneumonia occurring in succession in patients in the ward, and which seemed due to infection, though how derived I could not trace; and also that, in spite of all precautions, cases of acute tuberculosis, arising in patients who have been in hospital for several weeks, and in whom the evidence is strong that they were not previously tubercular, still occur.

Several years ago I observed that in a considerable proportion of cases which had been for more than six weeks in my wards, especially if suffering from some chronic disease which increases susceptibility, if discharged and subsequently readmitted, would most probably have become tubercular. This happened in cases of aneurysm, cancer, anæmia, myxœdema, some cardiac diseases, and so on. For some years, especially since the number of cases of phthisis has been restricted, in consequence of an appeal to the Managers, and greater precautions taken to prevent infection, this has been less frequent. It has persisted more in cases long in the small ward. But it still occurs. One very striking case occurred last summer, in a man 60 years of age, with pernicious anæmia, in whom the onset of acute tuberculosis was watched after he had been long in. In that case there was no evidence of previous tuberculosis, and after death we found no old tubercular lesion, but widespread acute tubercular lesions, exactly like those seen after inoculation of a rabbit.

Such occurrences necessarily give one a feeling of grave responsibility, and make it a duty to try to eliminate all possible causes of infection. For, as I have said, in many diseases the power of resistance to infection is greatly lowered, and either the contagion of specific infectious disease is easily acquired, or the pyogenic organisms gain a ready hold.

Now, I have seen in various instances, where I have had to investigate the causes of septic and infectious diseases, that even in the best managed hospitals, and with the utmost care and attention to sanitation, there may be fatal flaws due to carelessness of servants or workmen. In Government and other inquiries in which I have taken part, and in many other instances well known to me, there have been most astonishing discoveries. Even the precautions taken to ensure thorough sanitation may, by the ingenuity of plumbers and others, be turned into death-traps. Everyone who has had to do with such matters is familiar with instances, and there are few more romantic or fascinating subjects. And this seemed a good opportunity for testing the matter fully here.

The measures taken were therefore most complete, and were carried out by the regular officials, under the direction of my able resident and experienced nurses, one of whom had had some training in a fever hospital. I have therefore every confidence that they were up to the highest standard, and were such as would have been taken in the most infectious disease possible. All bedding, clothing, etc., was directed to be removed and disinfected in the special apparatus for the purpose. And I assumed that they were disinfected by the only means known to be efficient for the purpose, such as is invariably adopted in public institutions, namely, by steam of regulated temperature and for a sufficient time. As to this part I had no doubt or anxiety.

You may think me unreasonable, but I was not satisfied with what I found on my visit next day. One is so accustomed to the application of common bacteriological principles in the laboratory, that what I saw startled me. It had never occurred to me that there could be two fundamentally different sciences of bacteriology in my laboratory and in practice. For I saw on the floor pieces of carpet unmoved, pictures hanging on the walls which had not been touched. I opened drawers and found within old papers and other rubbish, rags, etc., covered with dust. Books, too, on the bedside apparatus. The walls and ceiling had not been touched. On inquiry, I found that they had all remained and had been "fumigated."

Now, I must confess that, in my ignorance of the actual conditions, I had failed to give full and proper directions; and if any disaster had ensued in consequence, I and I alone should have been to blame. For not only had the conditions usually regarded as necessary been observed, but I believe that the measures taken were far more thorough and efficient than those commonly taken in private houses, or even in connection with some fever hospitals. And I have not the slightest doubt that if I had applied for some special, even costly, measures to be taken, and stated that I believed them to be necessary, they would have been granted. In proof of this, I need only point to the costly apparatus provided by the Managers for the surgeons, for sterilisation, and the elaborate precautions for periodic cleaning, for isolation, and other means of preventing infection.

What had been done? Sulphur to the amount, it is thought, of 2½ lb. had been burned, and the ward had been closed for twelve hours, and then thoroughly ventilated, and some "cleaning" done. Ought I to have been satisfied? Yes, if I had not known that the measures taken were totally inefficient to destroy any of the more resistant contagia, and that I could not have certified that any real "*disinfection*," in the true sense, had been carried out. For I knew that if I had left on the floor a piece of rag, on which I had dried sputum containing tubercle bacilli, I could produce tuberculosis by inoculating a rabbit from it; and so of many other contagia. And although I had no anxiety as to this particular case, it revealed a condition full of danger as to other diseases.

Two points especially struck me, namely the reliance on sulphur fumigation alone to destroy contagia, and the absence of thorough cleansing and removal of dust.

Let me first say a few words on the method of disinfection employed, namely, SO_2 fumigation. Sulphurous acid is well known to be an active bactericide, under certain conditions when in fluids. But, though long used officially, it has now given way almost entirely to other chemical disinfectants in countries like Germany, where the subject is dealt with scientifically.

What are the main facts as to disinfection by burning sulphur? You will find them pretty fully stated in two of the leading works of authority on the subjects of Hygiene and Disinfection.[1] Sulphur when burned produces SO_2, sulphurous anhydride. "In the anhydrous state it is not disinfectant."[2]

"Whenever the anhydrous gas is burned, the rooms must be sprayed or steamed to provide the necessary moisture to form the acid, H_2SO_3."[3]

If you will read the evidence afforded by various experiments which have been carried out on a large scale, you will at least arrive at the conclusion that to ensure disinfection by SO_2, great care is needed that the concentration of it in the air must be sufficient and of long enough duration, and that it is useless unless accompanied by moisture.

"It has been proved over and over again that the best results with this agent can only be attained under very strict experimental conditions, such as are quite unattainable in ordinary circumstances."[4]

Notter and Firth say, further, that Koch's experiments show that even when 10 per cent. SO_2 is present, and the air saturated with moisture, micro-organisms grow vigorously after twenty-four hours' exposure. To obtain this percentage would require 10 lb. of sulphur for each 1000 cub. ft. of air space, if combustion were perfect.

"As the disinfection of any given place is usually a complex operation, involving afterwards mechanical processes of scrubbing and cleansing, it is possible that a less quantity may suffice, but in many cases this should not be placed at a lower limit than 3 lb. of sulphur for each 1000 cub. ft. of space. Too great reliance, however, must not be placed upon disinfection by sulphurous acid; at best it is an uncertain agent. The slightest covering will protect micro-organisms from its action."[1]

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[1] Notter and Firth, "Theory and Practice of Hygiene," 1896, pp. 690–91, etc.

[2] Rideal, "Disinfection and Disinfectants," London, 1895, pp. 91–96. For the regulations for disinfection in various countries, see p. 94, where is also a reference to a Memorandum from the Local Government Board. 1½ lb. sulphur burnt over a small fire in a carefully sealed room for six hours or more. Wall paper then stripped off and burnt, and ceiling and floor thoroughly washed.

[3] Rideal, *ibid.*, p. 92.

[4] Notter and Firth, p. 691.

This amount, 3 lb. per 1000 cub. ft., is that usually accepted as sufficient, *if followed by subsequent thorough cleansing*, etc. But I know that some eminent sanitarians distrust it altogether. One of the leading sanitary authorities in Great Britain frankly avowed to me that he considered that its chief value was, that it made an unpleasant smell and ensured thorough ventilation.

For the disinfection of clothing and bedding, I know no authority of any importance who even suggests sulphur, although I find it is still relied upon here. Moist heat, by boiling or by steam, or solutions of active disinfectants, must be employed. It is said that dry heat, if used, must be over 220° F., for six to eight hours, but the action of this is very uncertain.[2]

Now, you will see one reason why I was not satisfied with what had been done. The quantity of sulphur burned is said to have been 2½ lb., though it was not exactly estimated, or less than one-third of the modified amount allowed to be needed for the air space. The room was not hermetically sealed, and no special provision was made for the necessary moisture. Further, so much reliance was placed upon this, that no sufficient subsequent cleaning had been attempted; carpets were left lying on the floor, pictures hanging untouched on the walls, accumulation of rubbish in drawers, etc. Yet I have reason to believe that this reached or exceeded the high-water mark of practical disinfection, as frequently practised—even in some institutions where it is especially needed,—and that it far exceeded what is usually thought necessary.

In my inquiries as to the methods of sulphur disinfection commonly employed, I chanced upon one stated to be frequently used. I show you some "sulphur cones" which, from the label on the box, you would believe to be largely used on official authority. But as one cannot take for granted all the statements in advertisements, even by eminent firms, I asked for further information, and received the following replies:—

"Each 4 oz. cone contains 3½ oz. of sulphur. There is no other disinfectant in them than sulphur. The cones are identical with those supplied by us to the Public Health Department of Edinburgh, and a large number of County Councils. The regulation is 1 lb. sulphur to every 1000 cub. ft. This we think somewhat in excess of actual requirements, two or three cones being in our opinion quite enough for the space named, *i.e.* 1000 cub. ft."

This is, of course, only the expression of opinion of a firm of chemists, but it shows the kind of idea and practice prevalent.

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[1] Notter and Frith, p. 691.

[2] See especially report on "Disinfection by Heat," by Dr. Parsons, in "Report of Medical Officer of Local Government Board for 1884."

There is no question that, even in "disinfection" by a Public Health Authority, three or four of these cones may be left to burn in a room of 1500 cub. ft. or more, not hermetically sealed, and with no appliance for ensuring moisture. You will see that there is a mistake somewhere. It may be that the word "disinfection" is used in some popular sense, denoting the expulsion of animal occupants. More probably, if used by any Public Authority, it would imply that no reliance was placed on any attempt to disinfect a room, and that it was assumed that all articles capable of conveying contagion were removed and otherwise disinfected, and that ordinary house-cleaning would do the rest. But if this is the case, some other unpleasant substance should be used about which there could be no misunderstanding.

Let me conclude with some axioms on disinfection—

1. Any method of so-called disinfection which does not really remove, so far as possible, the risk of infection, is not only useless but injurious.

For it generates a false confidence, leading to the neglect of common precautions; *e.g.* preliminary measures to *avoid* infection of articles of furniture and clothing will be incomplete if it is thought that they can easily, simply, cheaply, and without damage, be disinfected after.

In other words, the supposed disinfection directly hinders those ordinary measures of cleansing which would otherwise be carried out, but which are supposed to be unnecessary after disinfection.

2. No method of disinfection is satisfactory which is not in accordance with the ascertained facts of bacteriological science.

There cannot be two different sets of biological and chemical facts on the same subject entirely at variance with each other. Hence, in so far as the common basal principles and discoveries of bacteriology can be readily applied, they should be, and not be superseded by methods which, as tested by scientific experiment, are uncertain or worthless.

3. Disinfection should be graduated, and regulated—(1) by the known facts as to the life history of the contagium, and its mode of spread; (2) the period of the disease and the extent to which the contagium may have accumulated.

It is absurd to treat in the same way the contagia of whooping-cough and scarlet fever, or of diphtheria and typhoid. Nor is it reasonable to take the same measures in a room from which a case of scarlet fever has been removed on the second day, and one in which a case has been for five or six weeks.

But if any *one common method* must be employed in all cases, it must be that capable of dealing with the most virulent and persistent of contagia.

(*To be continued.*)

ON THE VALUE OF MILK WHEY IN INFANT FEEDING.

By HENRY ASHBY, M.D., F.R.C.P., *Physician, Manchester Children's Hospital.*

WHEY has been used—time out of mind—as a substitute for whole milk for the infant, when digestion was at a low ebb. It must early have been recognised that cow's milk curdled in the stomach into masses of sour-smelling curd, upon which the digestive juices were apt to spend themselves in vain, and these were either rejected by vomiting or passed in the fæces. Under these circumstances whey was a useful temporary resort, the vomiting or diarrhœa ceased, and the digestive powers might be coaxed back into their normal condition.

I am inclined to think that, at the present time, the value of whey for infant feeding is often overlooked or forgotten, a result no doubt due to the fact that so many tinned or bottled infants' foods—of the "perfect substitute for mother's milk" sort—are extensively advertised by their makers, and our more homely milk modifications have been thrown into the shade. But surely no sane individual can ever claim that milk which has been dried, "tinned," and stocked, can ever again be made to yield a fluid that can compare with fresh milk or milk whey as a food.

Some years ago Frankland suggested whey as a diluent for milk, and gave directions for the preparation of "humanised milk"—*i.e.* a mixture which resembles woman's milk in the percentage of its constituents—by mixing together certain proportions of cream and whey. Since then "humanised milk" has been manufactured on a large scale by mixing cream, milk, and sugar water in various proportions, sterilising and sending out in bottles. I doubt very much if the manufactured article, which is mostly overheated to make it keep good for months, is as good a food as can be made at home by mixing cream, or milk and whey, and using it fresh. There are comparatively few of us within easy reach of a Walker-Gordon laboratory, where we can have any formula for a milk food made up and delivered daily with an exactness we cannot imitate in the household,—but we have to depend upon home-made modifications. The temptation, no doubt, is great to fall back upon a manufactured food, it is so much easier to take a spoonful of powder out of a bottle or tin and stir it up with warm water, when the infant has to be fed, than to set aside milk to cream and to prepare the whey. But it is worth all the trouble and time expended on its preparation.

When "rennin" or essence of rennet is added to warm milk, the proteid in the milk—namely, caseinogen—is split up into an insoluble proteid casein and "whey proteid," the latter being unaltered either by "rennin" or heat. The casein is precipitated in company with the fat, leaving the whey proteid, lactalbumin, lactose, and salts in solution. When whey is made, as in cheese-making, it is desirable that it should contain as little fat as possible, as the fat is wanted to enrich the cheese. The following is the percentage composition of whey as given by several authorities:—

Whey.

	Proteid.	Fat.	Lactose.	Salts.	Water.
Koenig .	·85	·23	4·71	·65	93·24
Bauer .	·82	·24	4·65	·65	93·3
Benger .	·8	·16	...	·65	...

But when whey is required as a food, it is clearly desirable that at least some of the fat should be retained in the whey, and not be rejected with the curd. This may be effected by a thorough agitation of the curd before straining. Perhaps the most ready way of preparing whey for infant feeding is to use Hawksley's sterilising apparatus, as it is provided with a thermometer, and consequently the temperature can be regulated. Thirty oz. of good fresh milk are placed in the bottle provided, and the heat raised to 104° F.; add two teaspoonfuls of Benger's essence of rennet, and set aside for a few minutes. When curdling has taken place, thoroughly break up the curd by stirring and shaking the bottle; then strain through fine muslin or a colander. In this way 22 or 23 oz. of an opalescent fluid is obtained, which should be heated to 160° F. for twenty minutes, in order to destroy the rennin. It may require a further straining, as more curd is apt to form on heating. Mr. F. Baden Benger, F.C.S., was good enough to make some analyses for me of whey prepared in this way. He found on an average that the whey contained—proteids, ·97; fat, 2; salts, 6·1 per cent.; the amount of fat being much larger than in whey prepared by simply draining the whey off the curd without agitation. Centrifuging, in Gerber's acid butyrometer, the whey prepared in the wards of the Children's Hospital, I find an average of 1·75 to 2 per cent. of fat.

Whey prepared in this way, with or without an addition of 2 or 3 drms. of milk sugar to the pint, makes a useful food for newly-born infants who have to be artificially fed, or for infants who suffer from chronic vomiting, or have liquid, green, and curdy stools. They will gain weight and be more comfortable than when taking diluted milk. A weak "humanised" milk may be made by adding 10 oz. of fresh milk to 20 oz. of sterilised whey, and adding ½ oz. of milk sugar. To make a "humanised" milk more rich in fat, use "top milk" in the same proportion. Let a quart of fresh milk stand in a covered glass jar in a cold place for *four* or *five* hours; remove the upper 10 oz. by skimming, add this to 20 oz. of sterilised whey with ½ oz. of milk sugar.

If these mixtures are carefully made according to directions, perfectly fresh milk of a good average quality being used, the analysis will work out something like the following, but exactness cannot be expected, inasmuch as milk varies in the amount of fat it contains and the time it takes to cream:—

	Proteids.	Fat.	Lactose.	Salts.
Human milk, average .	1·75	3·5	6	·3
Whey	·8	2	4·5	·6
Milk, 10 oz. Whey, 20 oz. Lactose, ½ oz.	1·75	2·5	6	·6
Cream, 8 per cent., 10 oz. Whey, 20 oz. Lactose, ½ oz.	1·75	4	6	·6

It is always well to add a grain or two of bicarbonate of soda, to render the mixtures neutral or slightly alkaline.

It is often convenient to give dyspeptic infants whey at first, or even dilute the whey with a solution of maltose or barley-water, as such infants cannot always digest as much as 2 per cent. of fat in their food. As they improve, add milk to the whey, or "top-milk," as their digestive powers gain strength.

It may be asked, what advantage has whey over a solution of lactose as a diluent? It certainly contains "antiscorbutic" properties which a solution of milk-sugar does not, and, moreover, it contains soluble proteids and a certain amount of fat. I have frequently found infants improve and gain in weight after whey has been substituted for sugar water or barley water as a diluent for milk, and my experience is strongly in its favour. But more than the average intelligence is wanted in the nursery to carry out details.

REVIEWS OF
BRITISH AND FOREIGN LITERATURE.

The Genesis and Dissolution of the Faculty of Speech : A Clinical and Psychological Study of Aphasia. By JOSEPH COLLINS, M.D. New York: The Macmillan Company; London: Macmillan & Co. Ltd. 1898.

THOSE of us "on this side" who have the pleasure and advantage of being personally acquainted with Dr. Joseph Collins, regard him as one of the most able and brilliant of the younger exponents of neurology in America, and expect that anything which he writes will reach a high level of excellence. This important treatise, to which the Alvarenga Prize of the College of Physicians of Philadelphia was awarded in the year 1897, fully comes up to one's expectations in this respect. It consists of some 420 pages, and is divided into twelve chapters and two appendices.

In the first chapter, the meaning of the term Aphasia, the conditions which should be included under it, and the classification of Aphasia, are discussed and considered.

After suggesting a classification of his own (with which the present writer cannot altogether agree), he adopts a working classification, and divides aphasia, in the first place, into motor aphasia, sensory aphasia, and total or compound aphasia; and then subdivides each of these main forms into several separate varieties. In connection with this classification, he states that "the word motor aphasia is never used synonymously in his own mind with the word ataxic; moreover, that the word motor is retained to apply to that form of aphasia anatomically characterised by lesion of Broca's convolution, solely because such usage has been consecrated by time." He agrees with Charlton Bastian in thinking that the images stored up in Broca's convolution are "genetically sensory, the result of motion."

Under the term subcortical auditory aphasia, he not only includes lesions of the subcortical sensory tract, but thinks that under this head "may possibly also be included diseases of the labyrinth, as suggested by Freund, Bleuler, and Wernicke, and of diseases of the organ of Corti and of the nerve fibres going from it." This position seems to us an extremely doubtful one, as also the corresponding statement that the term subcortical visual aphasia "may possibly be extended to include disturbance of the peripheral visual mechanism (retina, muscles, and optic nerve, up to the external geniculate body and the pulvinar of the thalamus)." This point is again referred to in more detail in chapter vi.

In the second chapter, a very complete review of the literature of aphasia, up to the date of the publication of the work, is given. In this chapter, which shows the wide erudition of the author, full justice is done to the contributions of British writers.

The third chapter, which is devoted to "an analysis of the genesis and function of speech," is perhaps the most important in the whole work, and is well worthy of careful study.

In the fourth chapter "the conception of aphasia" is treated of. The more important facts in the anatomy of the brain which have a direct bearing on the subject of aphasia (the position of the speech centres, etc.) are lucidly described.

In this chapter the author emphasises the fact that the speech faculty is maintained by the left hemisphere of the brain in those who are right-handed, and by the right hemisphere of the brain in those who are left-handed, and, if we understand him rightly, refuses to allow of any exceptions to this statement, though there is every reason to believe (as a case published by the writer of this review in a recent number of *Brain* shows) that in rare instances exceptions do occasionally occur.

The associations and connections between the different speech centres are next considered, the observations of Flechsig and Van Gehuchten, with regard to the course of the projection tracts, being referred to in considerable detail.

"The zone of language and the psychical revival of words in silent thought" are then treated of. Dr. Collins entirely agrees with Dejerine and his pupils—Mirallié, Thomas, and Roux—that a lesion in any one part of the "speech area" or "zone of language," or of any of the centres (the motor speech centre, or, as he terms it, the articulatory-kinæsthetic speech centre, the auditory speech centre, or the visual speech centre) of which the "zone of language" is composed, necessarily entails a perversion of function in other parts of the zone of language.

In the latter part of this chapter the debatable question as to the existence of a graphic speech centre is fully considered. Dr. Collins concludes: "After taking all the evidence into consideration, of whatever nature, that can be cited in favour of the existence of a specialised graphic motor centre, and carefully weighing it, the conclusion is forced upon us that, so far, the advocates of the existence of such a centre have furnished us with no absolute proof. It seems to me," he says, "entirely contrary to all that we know of speech, from a study of its genesis and its dissociation, to look for the existence of such a centre."

Chapter v. is devoted to the consideration of motor aphasia, the two varieties, cortical motor aphasia—or, as Dr. Collins terms it, articulatory-kinæsthetic aphasia—and the subcortical variety being considered in detail. In referring to the difficulty of differentiating these two varieties, and to the value of the test which is known as the Proust-Lichtheim test, Dr. Collins with great justice states: "The truth is that the Proust-Lichtheim test is not very readily applied, because many of the patients with whom we have to deal are quite incapable of comprehending what we are striving for when we endeavour to utilise it, and what we wish them to do; but when it can be applied, it is a test of great value."

Dr. Collins does not favour the view that the cortical centres in the right hemisphere, corresponding to the speech centres in the left hemisphere, are endowed with any active (*i.e.* independent or spontaneous) speech function; though he does "not deny that the corresponding centre of the other side, although not ontogenetically intended for speech, may and does take up, in a crude compensatory way, the function of the speech area. Nevertheless, I believe," he says, "that it never does so, except by process of education; it does not seem to me that there is any spontaneous assumption of function." A little later, however, he seems to qualify this opinion in some degree, for he states: "It is more than likely that attention has been concentrated too exclusively on the unilateral representation of the speech faculty," etc.

With reference to the debatable question as to the necessary occurrence of agraphia in cases of cortical motor aphasia, due to a lesion of Broca's centre, Dr. Collins takes up a very strong (we think far too strong) position. He says: "But a patient with cortical motor aphasia is completely agraphic, never learns to write with his left hand, no matter how long he may live after the aphasia has developed."

In chapters vi. and vii., the important subject of sensory aphasia—word-deafness, word-blindness, and the subcortical varieties of these conditions—is ably treated.

In speaking of subcortical visual aphasia, Dr. Collins states: "Generally speaking, they" (the symptoms) "are verbal blindness, always associated with right lateral homonymous hemianopsia, as the lesion is either of the primary visual area in the cortex of the occipital lobe bordering on the calcarine fissure, or of the optic radiations connecting the occipital lobes with the left angular gyrus, the higher visual centre." This statement seems to imply that a lesion in *one* half-vision centre, or of the fibres passing from it to the left angular gyrus, may produce word-blindness; whereas, as is well known, word-blindness only results when the lesion cuts off the connections between *both* half-vision centres and the visual speech centre. A lesion of one half-vision centre does not produce word-blindness, but merely hemianopsia; while complete destruction of both half-vision centres probably produces complete blindness.

In this chapter, Dr. Collins incidentally considers Sir William Broadbent's view as to the existence of a naming and propositioning centre. He considers that "the existence of such a centre is contrary to the conception of the psychogenesis of speech," and that "the cases that have been cited to establish its autonomy are made to support it only by putting unwarrantable interpretations upon accompanying symptoms."

In chapter vii. total aphasia is considered. In chapter viii. the diagnosis of aphasia, in chapter ix. the etiology, in chapter x. the morbid anatomy, and in chapter xi. the treatment of aphasia, are admirably treated of.

In chapter xii. the important subject of the medico-legal aspects of aphasia is shortly considered. This chapter seems to us to be the least satisfactory in the book. Dr. Collins' views on the medico-legal aspects of aphasia seem to us theoretical rather than practical; they appear to be too exclusively based upon, and we think warped by, the theoretical consideration that "the integrity of the primary speech centres constituting the zone of language is necessary for the full and legitimate genesis and notion of the word, and that disease of any of them must be attended by disorder in the conception and in the idea of the word." He consequently concludes—but surely such a conclusion is altogether unwarranted—that no one who is suffering from cortical aphasia, whether motor aphasia, word-deafness, or word-blindness, is capable of making a will. He states: "If it were necessary to define the status of the aphasic patient's testamentary capacity dogmatically and in a few words, I should say that, although every case is a law unto itself, no one is of sound disposing mind who has true aphasia—aphasia due to lesion of the zone of language; while a person who has any form of subcortical aphasia, be it motor or sensory, may be, and usually is, capable of indulging in civil transactions, although there are exceptions to the rule." It is only fair, however, to state that in the concluding paragraph of the chapter, this opinion is qualified by the statement that "in cases of this sort the eventual determination must be reached from personal study of the case, and not from its conforming to any hypothetical conditions."

In support of the opinions advanced in the work, Dr. Collins details numerous interesting cases of aphasia which have come under his own notice. Opinions will doubtless differ as to the conclusions to be based on the facts of these cases; and in this connection we think it is only right to point out that we cannot agree with the author's opinion as to the nature of the case which is considered sufficiently important to be separately detailed in Appendix ii. Dr. Collins cites it as a typical case of articulatory-kinæsthetic or motor aphasia, due to destruction of Broca's convolution. Personally, we are disposed to think that the lesion was probably situated farther back, and did not directly involve, or in all probability was not limited to, Broca's convolution. Dr. Collins further concludes that "the case shows with uncommon clearness that a striking degree of verbal blindness occurs with cortical motor aphasia"; and that it goes "to demonstrate that Broca's convolution has no direct representation in the capsular irradiation, and finally that the products of its activity are not sent directly to be externalised, but are sent to the Rolandic representation of the speech musculature and to the area of representation for other modes of language communication." Surely, in the absence of post-mortem verification as to the exact position and extent of the lesion, such conclusions are altogether premature and unwarranted.

The work is one which should be carefully and closely studied by every one who is interested in aphasia, and in the difficult and complicated physiological and psychological problems connected with the cerebral mechanisms concerned in speech, and with the derangements of those mechanisms which are produced by disease; for, whether one agrees with the opinions and conclusions which Dr. Collins advances or not (and different thinkers will necessarily differ on many of the difficult, obscure, and unsettled questions which he so ably discusses), one cannot but feel, after a careful study of the work, that one's ideas are enlarged and one's conception of the whole subject of aphasia and the faculty of speech is materially elucidated.

The book is clearly and admirably written, though many of the words and terms which Dr. Collins delights to use will no doubt be new and strange to English and Scottish readers. BYROM BRAMWELL.

Diseases of the Nervous System. By Sir W. R. Gowers. Vol. I. Third Edition. By Gowers and Taylor. London: J. & A. Churchill. 1899.

This is the first volume of the third edition of the monumental work of Sir William R. Gowers, who has, however, on this occasion taken Dr. James Taylor into collaboration. As in the case of the preceding editions, the first volume is devoted to the diseases of the spinal cord and nerves. We note, however, certain marked features in the volume before us which distinguish it from its predecessors. In the first place, it is increased in size by over two hundred pages. By this means a considerable amount of fresh matter has been added, embodying the results of the personal experience of the authors and the more important facts recently ascertained by other investigators. A new chapter has been interpolated upon the "General Constitution of the Nervous System," wherein are detailed the results of recent histological investigation, a general criticism of the neuronic theory, and its application to the pathological changes commonly met with in the central nervous system. A vein of scepticism may be detected running through this chapter, and it would be interesting to speculate how far the facts on which some of the modern views are founded will require modification in the next edition.

In the description of several of the diseases of the spinal cord—acute myelitis, atrophic poliomyelitis, etc., a considerable amount of new matter, bearing upon the question of "infection" as a causative factor, is included, and reference given to recent work on this important matter in the pathogeny of nervous affections.

An entirely new chapter has been added on sclerosis of the spinal cord, resulting from toxic blood conditions. We welcome this addition to the book, as it brings before the student in summary the results of much valuable recent investigation, scattered throughout English and continental journals, and also as it clears up certain clinical difficulties, which have been long recognised, but which could not be classed under any known pathological entity. Such scleroses are found in pellagra, leukæmia, pernicious anæmia, diabetes, and in some enfeebled and debilitated persons. Its anatomical resemblance to the so-called postero-lateral sclerosis is evident from the figures which are given of both conditions.

Considering the marked additions to the letterpress in this volume, there has not been a corresponding increase in the number of new figures. Those, however, that have been added are mainly original and of a high class. We regret to find, however, that the number of impressions which have been taken off the blocks prepared for the first edition of the work in 1886 has materially impaired their clearness and character.

Without in any way wishing to detract from the elaborate and valuable treatises upon the diseases of the nervous system which have appeared in the English language during the past few years, we still hold that the text-book of Gowers is the book which should be in the hands of all students and practitioners.

W. Aldren Turner.

Operative Gynecology. By H. A. Kelly, M.D., Baltimore. 2 vols. London: Henry Kimpton. 1898.

This important work is to be welcomed most heartily, and may at once be said to be one of the most thorough and able original works on the subject in the English language. It is refreshing amidst the flood of gynecological and obstetric treatises with which we have lately been inundated, to have in our hands a treatise such as this, which embodies the practice of one at whose disposal there is such an amount of clinical material, and whose experience has been so varied and matured. Indeed, in it we have not only the views of the author himself, but also an introduction to, and review of the methods adopted in that most perfectly equipped and conducted hospital, the Johns Hopkins.

It is a "one man" book, and as such will remain as a lasting tribute to its author, though it is probable that much which it now contains as up-to-date, will, from the rapid advance of gynecological surgery, be in a short time superseded by other methods.

What strikes one throughout, is the lucid and practical manner in which operative details are handled, which enables the reader to acquire a thorough grasp of any given procedure described, while the avoidance of mistakes, which the author himself has committed and unselfishly confesses, forms by no means the least valuable feature of the information given.

To the work we are introduced by an exposition on antisepsis and asepsis, which for completeness seems to leave little to be desired either in the methods adopted in hospital or private practice. Many valuable hints may here be gleaned, such as the sterilisation of sutures, etc., and particularly catgut, with which the author, like most others, has had much trouble, and has made a special study.

The method finally adopted is Krönig's cumol method, which, though somewhat intricate, has given reliable results, and enabled the author to use with much more freedom this dangerous but ideal ligature in abdominal operations.

The general methods of antisepsis and asepsis described certainly give a high ideal to aim at, but it is almost impossible for the average surgeon on this side of the Atlantic to literally follow; and indeed it is questionable if such rigid methods can be shown to be altogether necessary; statistical proof is still wanting.

The chapter on pathogenic organisms might have been dispensed with, as it is neither a complete exposition of the subject, nor is it sufficiently practically dealt with to be of much value. One is inclined to regret that from the thorough bacteriological examinations which are made in the Johns Hopkins Hospital, the author has not given any statistical records in this department.

The topographical anatomy forms quite a feature, and special mention might be made of the original illustrations here introduced, which are at once instructive and beautiful, particularly those on the disposition of the intestines.

Insufficiency of the pelvic floor is thoroughly considered, and for the repair of relaxed or torn conditions of the perineum an original and seemingly most effective method is described.

As is to be expected, in a department which the author has made so peculiarly his own, the urinary organs are minutely detailed, and the manipulations required for the diagnosis and treatment of their diseases is most efficient and complete. The topography of the bladder, its direct cystoscopic examination, and the catheterisation of the ureters, are all described in an explicit manner, and will well repay close study and reflection. Pathological conditions of these important structures, at one time so difficult to detect, are now by the methods described in many instances made easy of diagnosis. To this subject the author has earnestly devoted himself, and to such purpose that he deserves the thanks and congratulations of gynecologists generally.

Curettage of the uterus is fully described; the author here specially states that he never packs the uterus after the operation, and for the removal of remnants of abortion he discounts the use of the finger-nail, in case of conveying sepsis, which seems carrying aseptic methods somewhat beyond rational limits.

Volume ii. is given over entirely to intra-abdominal operations. The general principles laid down are lucid and practical, and many minor details of great value are described. In the preparation of the patient it is surprising no mention is made of disinfecting the skin of the abdomen by an antiseptic compress for forty-eight hours previous to the operation; but, in other respects, for completeness, it leaves nothing to be desired. The rules laid down for the arrest of hæmorrhage are most practical, while the question of abdominal drainage has been made a special study, and will repay close attention. In substance, it may be said that the author considers that abdominal drainage should, if possible, be dispensed with, as it is an imminent source of danger, and should only be undertaken (1) in abscesses walled off from the peritoneal cavity, (2) in widespread peritoneal suppuration, and (3) when intestine has been sutured and there is doubt as to the accuracy of the suturing. Postural drainage and drainage per vaginam are more frequently recommended.

Closure of the incision is recommended to be done in successive layers with catgut, silver wire, and silkworm. The subcuticular layer savours strongly of sentiment, and seems a somewhat prolonged procedure where rapidity of operation is so important.

No mention is made of counting the instruments, compresses, and sponges used: this, we consider, ought to be essential before every abdominal operation, as records of fatal results from carelessness, from want of counting, are but too numerous.

The Trendelenburg posture is strongly favoured.

The after-treatment of normal abdominal cases is laid down most fully, while the methods of recognising and treating the numerous post-operative complications are instructive and valuable.

The varieties of pelvo-abdominal operations are fully discussed in detail, and many hints may be gleaned even by the experienced surgeon.

Particularly full is the description of myomectomy and hysterectomy, and it is refreshing indeed to have such conservative views on the treatment of fibro-myomata generally. The method of performing abdominal hystero-myomectomy bears the stamp of originality; the ligation of the uterine vessels on one side only, before removal of the tumour, is novel, but has advantages, as experience has taught me. The description of complicated hystero-myomectomy is very lucid, and forms one of the most practically important parts of the book.

A specially interesting and instructive chapter is devoted to the conservative operations on the uterine appendages, and ought to do much to restrain the radical measures so freely adopted. The removal of a healthy ovary or tube because its opposite fellow is diseased, is rightly and strongly condemned. The retention of healthy ovaries in whole or in part is advocated in all cases possible, as it is probable that these organs play a most important part in maintaining a healthy state of the animal economy. The question is a difficult one, and may be said to be entirely in its infancy. The difficulty of recognising healthy from diseased parts of organs must in many instances be more or less haphazard and empirical, and one may find, after performing a so-called conservative operation of considerable magnitude, that the symptoms remain *in statu quo ante.* The object is commendable as being the general attitude of all true surgery, and as such deserves much careful thought, which cannot fail to be initiated on perusal of the article.

Suspensio uteri is fully and originally dealt with. The author here recommends the suturing of the posterior surface of the fundus uteri to the abdominal wall by a couple of buried silk sutures. He claims for this operation that the uterus is suspended, not fixed, and shows satisfactory statistical proof that the statement "that it forms a complication to succeeding pregnancies" is almost entirely unfounded. The subjects of extra-uterine pregnancy, Cæsarean section, and conservative operations on the pregnant uterus, as well as vaginal hysterectomy and intestinal operations, are fully discussed. It is, however, impossible to review in detail each individually. As the author in the preface states, the work is intended simply as a summary of the various gynecological operations he has found best in his practice. It is no digest of the literature. As such, then, it is particularly valuable, and will well repay careful reading. Dr. Kelly is to be heartily congratulated on its production, and we feel sure it will add lustre to his already illustrious name. It maintains throughout a high standard. The illustrations are extremely numerous and instructive, and are so far the finest in any similar work.

F. W. N. HAULTAIN.

The Science and Practice of Midwifery. By W. S. PLAYFAIR, M.D. Ninth Edition. Two volumes. London: Smith, Elder, & Co. 1898.

TEXT-BOOKS in obstetrics may come and go, but "Playfair" we are glad to have always with us. This is readily accounted for, not only by the sound practical and useful manner in which the subject is treated, but by the charming style in which it is written, giving, as it does, in a clear, consecutive, and interesting way, a complete exposition of the art. In its very simplicity of description, perhaps, lies its chief drawback, if that be possible. The hard-worked student is but too apt to interestingly read in a novel-like manner chapter after chapter, pleasing himself with the absorption of the main facts they contain, but failing to assimilate the important details with which they bristle.

The present edition (the ninth) has been thoroughly brought up to date, and must be welcomed by teacher and student alike. Dr. Eden is to be congratulated on the able manner he has treated the chapter on conception and generation, which forms the most radical change in the work. His description of the early association of the ovum and deciduæ, the formation of the placenta, and the changes occurring in that organ towards the end of pregnancy, are instructive and valuable, embodying, as they do, the latest work on this intricate subject.

Extra-uterine gestation is much more fully dealt with in this than in previous editions. The author appears to have omitted, however, the frequency with which the symptoms of so-called rupture are the result of free bleeding through the abdominal extremity of the unruptured tube; while a more clear differential diagnosis between extraperitoneal and intraperitoneal hæmorrhage after rupture, would have been valuable as a guide to the very different method of treatment required in these two conditions. It is debatable, also, whether the author is right in stating "that the vaginal route is unsuitable for operating upon unruptured tubal pregnancies."

We cannot agree that pulsation in the vaginal fornix is a sign of secondary importance in diagnosing pregnancy. In conjunction with other signs and symptoms, it must be looked upon as of the very greatest value in the difficult diagnosis of very early gestations.

The question of the superiority of version and forceps in flattened pelvis is too strongly urged, the proofs afforded by Braxton Hicks and J. Y. Simpson being of little value in these days of axis-traction, which has almost entirely revolutionised the relative benefits of forceps operations.

As a whole, the work maintains its previous high standard of excellence as a trustworthy and highly efficient text-book. As a classic among obstetric treatises it must still remain, but at the same time, from the thorough revision it has undergone in the present edition, it will, as it heartily deserves to do, maintain its position as a prime favourite of works of its kind.

F. W. N. Haultain.

Clinical Lectures on Cleft Palate, Simple Fractures, Traumatic Arthritis, etc. By W. Arbuthnot Lane, F.R.C.S. London: The Medical Publishing Co. Ltd. 1898.

The reproduction of these clinical lectures in book-form is a welcome addition to surgical literature. Mr. Lane's style is most attractive when pleading for the acceptance of heterodox opinions, especially in the domain of etiology and treatment. The father of a large family will find comfort in his teaching, that the operative treatment of post-nasal adenoids is quite unnecessary. The author's bias in favour of the flap-method for closing a cleft in the palate is quite in harmony with his progressive tendencies in other departments of surgery.

The pathology of the different forms of chronic arthritis is much simplified, by ascribing the well-known changes within and around the affected joints to mechanical and traumatic influences, irrespective of the fact that of the thousands of individuals exposed to these, only a very limited number become the victims of chronic joint disease. Mr. Lane's views on the treatment of simple fractures by operation may be safely described as better known than accepted. Whether the reader agrees with the author or not, he will readily admit that the perusal of these lectures has been both interesting and instructive.

ALEXIS THOMSON.

Leçons de Clinique Chirurgicale faites à l'Hôtel-Dieu. By Professor PIERRE DELBET. Paris: G. Steinheil. 1899.

THESE clinical lectures were delivered during the months of August and September 1897; in their present published form, they are dedicated to the distinguished occupant of the chair of Clinical Surgery, Professor Duplay. They deal with a number of subjects illustrated by cases in the wards, including cleft palate, middle ear disease and its complications, thyro-hyoid cysts, cicatricial stricture of the œsophagus, Pott's disease, fracture of the spine, cirsoid aneurysm, carpus curvus (a deformity resulting from rickety curvature of the lower end of the radius), contraction of the palmar fascia, varix of the internal saphena, Dupuytren's fracture, intestinal obstruction, hydatid cysts of the abdomen, and appendicitis. The lectures are written in the clear and finished style which is characteristic of the French schools.

ALEXIS THOMSON.

Practical Uranalysis and Urinary Diagnosis. By CHARLES W. PURDY, M.D., LL.D. Fourth edition. London: Sampson Low, Marston, & Co. Ltd.

DR. PURDY'S manual is one of the best, because one of the most practical, works extant upon uranalysis. Every page bears witness that the knowledge of the author is never second-hand, that all the methods and tests described have been personally made use of, and that the instructions given for their correct application are founded upon actual experience. Too many works of this kind contain details of methods theoretically correct, practically fraught with errors and difficulties. No one should attempt to lay down absolute rules for the guidance of his readers, as to the performance of any test or method of analysis, until he has attained full personal knowledge of its characters. This volume, the fourth edition, bears the impress of the personal equation—a personal equation of high value.

Considerable space is devoted to examination of the urine of proposers for life assurance. No doubt there is much laxity in this matter in many instances, but the elaborate rules laid down by Dr. Purdy could not well be carried out at all fully by the majority of examiners, nor would the fee received recompense for the time spent upon them.

A. LOCKHART GILLESPIE.

RECENT ADVANCES IN MEDICAL SCIENCE.

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MEDICINE.

UNDER THE CHARGE OF

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Lesions associated with Acromegaly.

Labadie-Lagrave and Degny (*Arch. gén. de méd.*, Paris, February 1899) contribute an interesting article upon the association of cardiac and renal disease, and of diabetes and polyuria, with acromegaly. The text of their remarks is provided by the details of two cases of acromegaly of dissimilar character. The first, a painter, who had suffered from the symptoms of plumbism common to his trade, lead colic, and a transient neuritis, was found on examination to present all the characteristic features of acromegaly, along with an increased area of cardiac dulness, the sounds of slight pericarditic friction, albuminuria, polyuria, a lead line upon the gums, indistinct vision, and severe headache. The diagnosis made was interstitial nephritis from an arterio-sclerosis caused by saturnism, imminence of uræmia, pericarditis of renal origin, hypertrophied heart, acromegaly. The amount of urine passed varied from 87½ to 177½ oz. daily, with 7·7 to 23·1 grs. of albumin. Sudden death occurred about a month after admission to hospital, and an autopsy showed the presence of saturnine nephritis, enlarged heart with pericarditis adhesiva, cerebral hæmorrhage, commencing peritonitis, and acromegaly. There was no pituitary lesion, the liver was large with signs of commencing cirrhotic changes. (Clinically it was made out to be small.)

The large size of the heart—its weight reached nearly 2¾ lb., without any valvular lesion—was not astonishing when the renal condition and the "acromegalique" diathesis of the patient were considered. The second case was said by Charcot in 1882 to be one of acromegaly. Gradually for more than fourteen years the symptoms increased, and he latterly exhibited cardiac arythmia, with hypertrophy, arterial sclerosis, and traces of albumin in the urine. Careful analysis of the urine showed that, though in normal quantity, it contains rather less urea and nitrogen, more uric acid, an average amount of phosphoric acid, *but a marked decrease in lime salts.* Before death no examination was permitted. Discussing the facts presented by these cases and the extant literature on the subject, the authors are led to formulate the following propositions:—(1) Pressure, even when light, upon the pituitary body at once alters the blood pressure, and slows the heart-beat, but at the same time strengthens it. (2) Electricity, especially in weak current, acts similarly but more intensely. (3) Pituitary extract, when injected into the veins of animals, does the same. They regard the pituitary body as extremely sensitive to varied pressures of the cerebro-spinal fluid and the blood, the effects of which initiate variations in symptoms, such as are dependent upon the action of this gland and upon the nature of its secretion.

Summing up their remarks, they place the pituitary body, in the sense of its function, alongside the thyroid gland, elaborating a substance which acts on the cardiac nervous mechanism and on the blood vessels, in the same way as iodothyrin. It has been proved that rabbits, when they are deprived of their thyroid glands, show a marked increase of the pituitary body. The cardio-vascular symptoms, so common in cases of acromegaly, can therefore be easily accounted for, the causal lesion affecting the circulatory organs as well as the general osseous and muscular frame wall of the body.

Chadbourne (*N. Y. Med. Journ.*, April 2, 1898) gives a short account of a case of acromegaly, in which symptoms of diabetes mellitus were also present; while Woods Hutchinson has an article upon acromegaly and giantism as related to the pituitary gland in the same journal (March 26, April 9, 1898). Buday and Jancsó (*Deutsches Arch. f. klin. Med.*, Leipzig, Bd. lx. Hefte 4 and 5) contribute to the literature of the subject by detailing a case of "pathological giantism," in which an adeno-sarcoma of the pituitary body, as large as a hen's egg, enlargement of the bones, glycosuria, etc., were found. The patient grew rapildy between his twentieth and thirtieth year, finally reaching 6 ft. 7½ in.; although this extra growth only began after his twentieth year, it had been noticed earlier that the epiphyseal junctions of his bones were enlarged. The writers regard the case as differing from ordinary acromegaly, because of the absence of marked thickening of the bones of the extremities, the freedom of the soft parts from any abnormal growth, and the general appearance of the patient.

In *Policlin.*, Roma, September 15, 1897, a paper by Massalongo can be referred to, in which he argues strongly against the description of pulmonary hypertrophic osteo-arthropathy as a distinct clinical type; regarding the pulmonary affection simply as a coincidence, not a cause. Thayer (*Journ. Nerv. and Ment. Dis.*, N. Y., No. 5, 1898) illustrates the difference which the enlargement of the bones in acromegaly bears to the epiphyseal increase in osteo-arthropathy by means of skiagrams.

Loeb (*Centralbl. f. innere Med.*, 3rd September 1898) contributes a valuable addition to our knowledge regarding the relations between the state of the pituitary body and the presence of diabetes mellitus. As long ago as 1884, Loeb stated (*Deutsches Arch. f. klin. Med.*, Leipzig, Bd. xxxiv. S. 449) that tumours of the hypophysis cerebri were able to cause variations in the body temperature, either raising or lowering it, and in many cases to bring about glycosuria. His recent article should be consulted in the original. He ends by admitting that the causation of glycosuria or diabetes by pressure of an enlarged pituitary gland on the neighbouring parts of the brain can only be regarded as being a probable hypothesis.

Details of five cases of acromegaly are to be found in the *Trans. Path. Soc. London*, for 1898. In two cases the pituitary gland was the seat of an adenoma, in one of a sarcoma, in one of a gumma, and in one of hyperplasia. In three of them the thyroid gland was markedly enlarged. Bailey (*Phila. Med. Journ.*, 1898) found a hypertrophy of the thyroid and an adenoma of the pituitary gland in one case, and an adenoma of the latter gland in another patient who had suffered from paræsthesia in the limbs, weak sight, and headaches for years. Johnston and Monro (*Glasgow Med. Journ.*, August 1898) found a pituitary sarcoma in an acromegaly case, without any thyroid enlargement.

ASTHENIC PARALYSIS, ALBUMOSURIA, AND MULTIPLE MYELOMATA.

A PECULIAR and remarkable case of the above is detailed by Senator (*Berl. klin. Wchnschr.*, 20th February 1899). In February 1897, albumin and casts were detected in the urine of a female, æt. 36, who came complaining of the usual symptoms of nephritis. All other organs were healthy. Treatment out of hospital was unsuccessful, and in March examination demonstrated the presence of albumose in the urine, while the patient had begun to suffer from severe pain in the head, back, and limbs, and complained of great lassitude. Admitted later into hospital, she gradually grew worse, her tongue was paralysed, so that feeding through a tube had to be resorted to. Over her chin the skin became insensitive, the surface of the body presented a marked white look.

Some paresis of the arytenoideus muscle was observed, and a diminution of the sense of touch over the areas supplied by the left peroneal and tibial nerves; no other signs of disturbance of the cerebral nerves could be detected, but increased knee-jerks were present. The tongue was not atrophied, but showed fibrillary twitchings. The pulse rate was increased—90 to 96 per minute. No definite diagnosis was arrived at. The presence of the albumosuria suggested osteomalacia, but there were no signs of that disease. Since the case was observed and the details first published (*ibid.*, No. 48, 1897), several instances of multiple myelomata accompanied with albumosuria have been recorded. Senator has collected eleven with seven autopsies. After discussing the various points presented by them, he sums up by surmising that in his case the primary affection was to be ascribed to the sarcomatous growths, leading to albumosæmia, albumosuria, nephritis, profound anæmia, and an insufficiently nourished condition of the nervous system from these lesions, disposing the patient to asthenic paralysis.

TETANY FOLLOWING PARTIAL EXCISION OF THE THYROID GLAND.

SCHILLING (*München. med. Wchnschr.*, 21st February 1899) records the case of a youth nearly 16 years of age, who from childhood had suffered from a simple goitre. He had had no other special diseases. The glandular enlargement had increased more rapidly for several months before he presented himself for examination, and as it caused severe inspirations, dyspnœa, and other symptoms of pressure upon the trachea and larynx, the left lobe of the thyroid, which was the larger, was excised. Fifteen months later the patient noticed an increase of the remaining right lobe, and a fortnight after this was suddenly attacked with spasmodic contractions of both arms. The first attack lasted two hours, and was benefited by the administration of morphine; while two similar attacks followed during the same day. Next day he had two such, but not so severe, though the legs were also affected by them. After a period of quiescence, lasting for five days, two mild attacks were experienced. After this no further spontaneous movements occurred, but the motor nerves of the patient were in a highly responsive state. Trousseau's phenomenon was well marked; the reflexes increased. A month after, the right lobe of the thyroid gland began to enlarge, all the symptoms had disappeared, and the lobe had decreased in bulk to its original dimensions.

Discussing the facts, Schilling points out that the right lobe of the gland was much smaller than the left before operation, that after removal of the left lobe no symptoms of any disturbance appeared until the half gland left became enlarged and tense, and had wakened to the fact that it was called upon to supply the whole body with thyroid secretion. The consequent excitation and increased production of juice induced a state of the nervous system calculated to give rise to the symptoms of tetany. These symptoms subsided whenever the balance between thyroid secretion and bodily needs were equalised.

A consideration of the references extant as to this condition follows. Szumann (*Centralbl. f. Chir.*, Leipzig, 1898), Turetta (*ibid.*, 1892, Bd. iv.), and Meinert (*Arch. f. Gynäk.*, Berlin, 1898, Bd. lv.) have each recorded similar cases. Where the total removal of the thyroid had been practised, Billroth observed no onset of tetany in 109 cases. Weiss found 23 per cent. among fifty-three cases, Roux none in 118. Eiselsberg, Schiff, Wagner, and Horsley saw symptoms of tetany and of chronic convulsions in animals deprived of a more or less large portion of the thyroid gland.

Finally, Schilling remarks that tetanus commences with trismus, and thereafter affects the more peripheral muscles, and is remittent, not intermittent; while tetany first affects the muscles of the extremities, never beginning in the masseters, and is always of an intermittent type.

SURGERY.

UNDER THE CHARGE OF

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RESECTION OF THE CERVICAL SYMPATHETIC IN EXOPHTHALMIC GOITRE, IN EPILEPSY, AND IN GLAUCOMA.

JONNESCO of Bucharest, who has written already on this subject, is now able to furnish the results of this operation, which he may be said to have introduced, and which he has now carried out in sixty-one cases, as follows:—

43 of idiopathic epilepsy.
1 ,, ,, ,, and chorea.
1 ,, ,, ,, and Graves's disease.
8 ,, Graves's disease (five primary and three secondary).
1 ,, ,, ,, and glaucoma.
7 ,, glaucoma.

In none of the above cases was the operation fatal; six epileptics died sooner or later after the operation, of intercurrent acute disease.

Jonnesco maintains that simple section of the cervical sympathetic (first recommended by Jaboulay), is not only insufficient but irrational, as it does not prevent the individual ganglia from continuing to exercise the injurious influence which it is the object of the operation to abolish. The removal (resection) may concern one, two, or all the ganglia, with the intervening cord, on one or on both sides, according to the disease for which the operation is performed. The details of the operation have already been published.[1] When it is proposed to resect the entire sympathetic on both sides, it is safer to allow an interval of eight days to elapse between the two operations.

In Graves's disease it is necessary to remove the whole sympathetic on both sides, in order to eliminate the three sets of fibres, *i.e.* those to the eye, to the thyroid, and to the heart. Of ten cases subjected to this procedure, six have entirely recovered, and the remaining four have greatly improved. The cures date from twenty-six, twenty-five, fifteen, fifteen and a half, seven and a half, and four months. Soon after the operation, there is disappearance of the exophthalmos, and of the general nervousness. The tremors, tachycardia, pulsation of the carotids and of the goitre, disappear later. The pulse may slow down from 140 to 70 or 80. The goitre itself changes more gradually, losing its expansile pulsation and murmur, and diminishing in size; several months or a full year may elapse before it returns to the normal. The gland undergoes a sclerotic shrinking.

In cases of secondary Graves's disease, *i.e.* those in which the thyroid is enlarged long before the other symptoms appear, the influence of the operation on the goitre may be nil, but the nervousness and the tachycardia improve very considerably. The persistent goitre may then be excised, or this may be done in the first instance, for statistics show that thyroidectomy, in secondary Graves's disease, is much more likely to be curative than in the primary form, in which the orbital and cardiac phenomena are present from the outset.

In epilepsy, the object of the operation is to bring about an alteration in the circulation of the brain, converting the cerebral anæmia into a permanent congestion, thus improving the nutrition of the brain cells, or ridding them of toxic products. In reflex epilepsy it is desired to interrupt the paths along which impulses travel from the viscera to the brain. To fulfil these objects, it is necessary to extirpate the whole cervical sympathetic. Jonnesco has carried this out in forty-five cases of epilepsy; in forty-two of these the entire cord was removed on both sides. Six died, at a varying period after the operation, of epilepsy or intercurrent disease. Of the remaining thirty-nine, only nineteen are of sufficiently long standing to be reported upon at present. No less than ten of these are cured; five have had no fits for two years, one for nineteen months, three for fifteen to eighteen months, and one for six months. Six cases have decidedly improved. In two the result has been negative.

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[1] *Centralbl. f. Chir.*, Leipzig, 1897, No. 2.

In epileptics of long standing, suffering from mania and epileptic dementia, no improvement can be expected from the operation, because of the degenerative changes in the brain cells.

In glaucoma, the extirpation of the superior cervical ganglion destroys —(1) The vaso-constrictor fibres of the eye—this results in dilatation of the arteries, lowering of the blood pressure, and diminution of exudation; (2) the excito-secretory fibres of the eye—this results in diminution in the amount of the aqueous humour; (3) the fibres supplying the dilator pupillæ—this results in the contraction of the pupil, and in the easier escape of the aqueous humour from the eye; (4) the destruction of the fibres distributed to the non-striped muscle of the orbit—this results in the relaxation of these muscles, and the recovery of the venous circulation of the eye. All these fibres pass through the superior cervical ganglion on their way to the orbit, and are therefore interrupted when the ganglion is removed. The operation has yielded remarkable results. In a series of eight patients, the superior ganglion was removed on both sides in four, on one side only in three; in the remaining one, the entire sympathetic was removed on both sides, because there existed a combination of glaucoma with Graves's disease. The results are stated to have been as follows:—(1) Diminution of the ocular tension; (2) marked contraction of the pupil; (3) disappearance of the supra-orbital pain and headache; (4) cessation of the attacks in glaucoma irritativum; (5) decided and persistent improvement in sight, along with enlargement of the field of vision in those cases in which the papillary atrophy was not absolute. There was no improvement in two cases of glaucoma chronicum irritativum, in one case of simple chronic glaucoma, and in one case of acute glaucoma. The benefits from the operation were so remarkable in the four remaining cases (in three of whom a double iridectomy had been previously performed without any result), that in such a painful, distressing, and often hopeless affection, the procedure suggested is well worthy of further trial.—*Centralbl. f. Chir.*, Leipzig, 11th Feb. 1899.

THE FORCIBLE CORRECTION OF ANGULAR CURVATURE OF THE SPINE.

In concluding a series of articles on the above subject, M. Pierre Wiert, after carefully weighing all the evidence obtained from the reported cases, is not disposed to be enthusiastic in its praises. Suitable cases are the exception. It is not to be attempted, unless the child is in good general health, and is free from pulmonary disease. Curvatures of old standing are to be left alone. Cure can only be expected in recent, slight projections. Post-mortem examinations have shown the impossibity of sound ankylosis after forcible correction of the curvature, and suggest the probability that the reported successes are only temporary. The author agrees with Murray of Liverpool, in preferring the orthodox treatment of Pott's disease to any operative interference.—*Rev. de chir.*, Paris, February 1899.

TUMOURS OF THE TESTICLE.

FROM an analysis of the recorded cases and the individual examination of fourteen specimens, Krompecher comes to the following conclusions: —Tumours of epithelial origin, *e.g.* adenoma and carcinoma, are rare, compared with tumours arising from lymphatic endothelium (endothelioma) and ordinary connective tissue (sarcoma). The origin of the endothelioma, from the lining endothelium of the larger lymphatic spaces, has been directly observed. The endothelioma, with alveolar arrangement of the cells (alveolar sarcoma), is the most common malignant tumour of the testicle. Clinically, sarcomata and endotheliomata occur for the most part in children and adults under 40; they grow rapidly, and present, as a rule, a smooth surface; the tunica albuginea and skin resist perforation by the tumour tissue, and the cord usually remains free; the primary seat is frequently the epididymis (*Virchow's Archiv*, Bd. cli., Suppl.). Most has investigated six cases of malignant tumour of the testicle—two large round-celled, one alveolar, one angio-sarcoma; one round-celled, in which the regular arrangement of the columns of cells suggested their origin from lymphatic endothelium; and one of endothelioma, growing from the blood vessels. With the exception of one patient, who could not be traced, all died, in spite of the removal of the tumour at a time when no secondary growths could be discovered. The cause of death was metastases in the retroperitoneal lymphatic glands. The author investigated the anatomical arrangement of the lymphatics of the testicle by experimental injections on the cadaver, and found that the numerous lymphatic vessels pass up the inguinal canal and behind the peritoneum, as far as the inferior pole of the kidney; they then turn inwards and open into the first group of glands, near the aorta, on the left side, and over the vena cava on the left. They do not give off any branches in their course. The outlet from the glands is into the receptacle of the chyle, from which the tumour infection may spread further by way of the thoracic duct.—*Virchow's Archiv*, Bd. cliv. Heft 138.

COXA VARA.

AT Kirmisson's instigation, Charpentier has made a critical study of this affection, based on the published reports of Müller, Lauenstein, Hoffa, and Kocher, on the specimens in the museums, and on the clinical material of the hospitals in Paris. His first contention is, that coxa vara is not a morbid entity but a group or collection of several pathological conditions. In the sense that coxa vara implies a curving of the neck of the femur, it may result in young subjects from rickets, from minor or attenuated forms of osteomyelitis, from hip-joint disease, from arthritis deformans, and from fracture of the neck of the femur. In those of rickety origin, the deformity is bilateral, is little marked, and does not interfere much with the functions of the limb. The neck is short, thick, and curved; the angle which it makes with the shaft is less than normal, but is never less than a right angle. The deformity is only recognised when one is on the look-out for it, and consists in upward displacement of the trochanter, limitation of abduction, eversion, and limitation of internal rotation. The limitation of internal rotation does not depend on the bending of the neck, but on the rickety curve of the shaft of the femur. The shortening of the extremity amounts to 1 to 1·5 cms. at the most. The author has observed the deformity in six out of thirty-two rickety children attending his out-patient clinique. In those cases resulting from attenuated forms of osteomyelitis there may be considerable shortening of the limb, stiffness of the joint, and other functional disturbances, including pain. The diagnosis from rickets is made with difficulty, unless the inflammatory changes are prominent and are attended with irregular rises in temperature. The author is quite convinced that a certain number of cases of coxa vara belong to the category of hip-joint disease and of arthritis deformans, the latter being especially present in cases with marked functional disturbance. He is also inclined to regard greenstick fractures of the neck of the femur as an etiological factor; occurring, as it does, in early life, it may give rise in later years to the clinical features of coxa vara. Lastly, the occurrence of late rickets, localised to the neck of the femur, must be accepted as a cause of the deformity, just as localised rickets is regarded as an important factor in the production of the genu valgum of adolescents. The author refers to the difficulty of distinguishing these various pathological conditions, which may result in coxa vara, from one another, and to the failure of the Röntgen rays, in helping us to make a differential diagnosis.—*Rev. d'orthop.*, Paris, 1898, Nos. 3 and 4.

THE TREATMENT OF THE INTESTINAL PARALYSIS OF PERITONITIS BY ENTEROSTOMY.

VAN ARSDALE of New York suggests that in the surgical treatment of septic peritonitis, in the stage in which the bowels are distended and paralysed, the evacuation of the intestinal contents should be regarded as an essential part of the operation. The suggestion has been made before, and has been put in practice upon isolated occasions. In any case of septic peritonitis in which the stage of what amounts to obstruction has been arrived at, while it is possible that recovery may follow on washing out and draining the peritoneal cavity, this result is unlikely, because the patient is apt to succumb to the continued absorption of poisons from the contents of the paralysed bowel. A temporary opening in the distended gut is the best method of putting an end to the poisoning. The best possible means of emptying the intestines is by free longitudinal incision (one or more), care being taken that the discharge is conveyed externally without soiling the abdominal wound or contents. The author records three cases in which the recovery of the patient appeared to be traceable to the procedure described; the parts of bowel opened were the ascending colon in two cases, and the lower ileum in a third. The openings either close themselves or are readily made to do so. The author also suggests that if the distension and paralysis of the bowel be not complete, it may be desirable to leave a loop of distended intestine in the operation wound and fix it there by suture, so that at any subsequent time, if the bowels do not move, or if the symptoms of absorption continue, artificial openings may be made without further trouble.—*Ann. Surg.*, Phila., January 1899.

THE PARAVAGINAL SPACE AND PARAVAGINAL HÆMATOMATA.

DR. VOITURIEZ of Lille describes the paravaginal space as the potential space between the parietal layer of the tunica vaginalis testis and the fibrous tunic of the latter, derived from the fascia transversalis. It corresponds to the space in the wall of the abdomen occupied by the extraperitoneal fat. The two layers which bound the space are easily separated from one another, and between them ramifies the funicular branch of the deep epigastric artery and its corresponding vein. The existence of the space is demonstrated in performing the radical cure of hydrocele, for, when the fibrous tunic is incised, it is quite easy to separate it from the tunica vaginalis and to shell out the hydrocele from the wound, except at the extreme lower end of the testicle, at the point which corresponds to the primitive attachment of the gubernaculum testis. The space may also be injected experimentally with any material which will coagulate (gelatin), and it may be recognised clinically when distended with blood; the latter condition may be described as paravaginal hæmatoma. It may occur independently, or it may coexist with intravaginal hæmatoma (the common hæmatocele). The former condition has been described by Reverdin and Kocher, and a personal observation is recorded by the author from the clinique of M. Duret. Paravaginal hæmatoma is frequently preceded by varicocele and by ordinary hydrocele; the determining cause is usually some injury of the nature of a kick or a fall stride legs, or an operation such as the puncture of a hydrocele. In the latter procedure, if the trocar wound the funicular arteries or veins, the extravasation of blood, instead of taking place into the tunica vaginalis, accumulates in the paravaginal space. The latter is attended with considerable pain and a sensation of weight in the scrotum. The scrotum swells up in the course of a few hours, and becomes tender on pressure, and of a bluish black colour. The coloration is due to the transudation of part of the blood through the fibrous tunic into the cellular tissue of the scrotum. The glans and prepuce become œdematous. In the course of a few days the discoloration subsides, and the swelling acquires a characteristic limitation and shape; it assumes the form of a globular swelling above and in front of the testicle, ascending a little way along the cord. The testicle remains quite independent and is easily recognised, so that the diagnosis from ordinary hæmatocele is comparatively simple. The hæmatoma rarely absorbs; its walls tend to become thickened. The treatment consists, therefore, in incision and drainage in recent cases, and in those of old standing, in dissecting out the entire sac.—*Ann. d. mal. d. org. génito-urin.*, Paris, February 1899.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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NEW OPERATION FOR RETROPOSITION OF THE UTERUS.

Paul Gelpi (*N. Orl. M. and S. Journ.*, Aug. 1898) describes the following operation as practised by Prof. Richelot of Paris:—The patient is placed in the dorsal position and careful antisepsis observed; the uterus is drawn well forward by a volsella, and anterior colpotomy performed by a transverse incision about an inch above the level of the external os; the vagina is well separated from the uterus, the bladder dissected away and pushed forward and upward, and the peritoneum opened. A volsella is introduced, and the uterus seized just above the isthmus and tilted forwards. It is then stitched to the cut surface of the anterior vaginal wall by three transverse catgut sutures, which are passed from left to right, first into the superior position of the liberated vaginal flap, at a point near its first margin, then deeply into the substance of the uterus, reappearing at a corresponding point on the opposite side. They are introduced immediately above the volsella, so as to leave the greater part of the body and the fundus free, and should be placed as near together as possible, and when tightly secured leave a vertical incision. The remainder of the vaginal flap is then brought into apposition with the fresh cervical margin of the incision, by means of three or four vertical catgut sutures on either side. Downward traction is thus made on the superior position of the vaginal flap, and the uterus dragged downwards and forwards to its normal position. The original incision now resembles an inverted T. The vagina is then packed with iodoform tampons, which are removed in from five to eight days.

THE CAUSE OF THE CONFLICTING STATEMENTS CONCERNING THE BACTERIAL CONTENTS OF THE VAGINAL SECRETION OF THE PREGNANT WOMAN.

J. Whitridge Williams (*Am. Journ. Obst.*, N. Y., Dec. 1898) believes that the different results which have been obtained from the bacteriological examination of the vaginal secretion in pregnant women, ever since this was first done by Gönner in 1887, depend, not on error in the bacteriological work, but on the faulty methods used for obtaining the secretion. To settle this vexed question, if possible definitely, Williams performed the following three experiments on twenty-five pregnant women who had not been previously examined:—

(1) With a platinum needle he removed some of the secretion from the margins of the hymen and inner surface of the labia minora; (2) with a specially constructed tube (Menge's) he removed some of the vaginal secretion; (3) with a sterilised glass speculum he removed some secretion from parts of the vaginal wall, with which apparently the

speculum had not come in contact. Cultivations were then made in agar from all the three secretions obtained, which he terms vulval, tubal, and specular respectively. The results were briefly as follows:—

In four of the twenty-five cases, the same organisms were found in all the secretions, and as they were identical with those found in cover-slips made from the tubal secretion, Williams holds that in each case he had to deal with the normal vaginal secretion, and that the tubal and specular secretions had not been contaminated from the vulva. In the remaining twenty-one cases, staphylococci were found in the vulval secretion in fifteen cases (60 per cent.), in the specular secretion in ten cases (40 per cent.), while in the tubal cases they were entirely absent. The colon bacillus he found four times in vulval, twice in specular, while it was absent in tubal cases; thus pyogenic organisms were found in the vulval secretion in nineteen cases (76 per cent.), in the specular secretion in twelve cases (48 per cent.), and were uniformly absent in the tubal secretion. The plates were never sterile in the case of the vulval secretion; they were sterile in only five cases (20 per cent.) of the specular secretion, while in the cases where the secretion was obtained by Menge's tube, they were sterile in fifteen cases (60 per cent.), and as yeast colonies (which frequently occur in the normal vaginal secretion) were found in the tubular secretion three times; this secretion was either sterile, or contained only yeast in eighteen cases (72 per cent.).

In summarising he states the following conclusions:—(1) This work tends to reconcile the conflicting results of various observers, and shows they are due to the technique by which the secretion is obtained, not to bacteriological error, the speculum carrying up with it organisms from the vulva. (2) Definitely confirms the previous statements of Krönig and himself, that pyogenic organisms do not normally exist in the vaginal secretion of pregnant women. (3) Auto-infection cannot occur, and organisms found in the puerperal uterus have been introduced from without. Accordingly, prophylactic vaginal douches are unnecessary and probably harmful. (4) Shows the danger of vaginal examination, as a small cylindrical speculum carried up into the vagina, from the vulva, pathogenic organisms in 50 per cent. of the cases. (5) Owing to the risk attending vaginal examination, this method should give place more and more to external examination.

A Simple Method of Plugging the Puerperal Uterus.

Schwarzenbach (*Centralbl. f. Gynäk.*, Leipzig, 1898, No. 36) refers to the instruments already introduced by Kehrer and Beuttner for plugging the uterus after abortion, and describes an instrument for plugging the uterus after delivery at full time, on account of hæmorrhage, as previously described by Dührssen. He quotes the statistics from the Zürich Maternity, showing that in half the cases where the uterus had been plugged there was a febrile puerperium, and describes two cases which terminated fatally on the sixth day, from accidental contamination of the gauze, when being introduced. The instrument consists of a speculum, funnel-shaped, 10 cms. long, 29 cms. in circumference at the external opening, 16 cms. at the internal opening, two volsellum forceps, with a small hook fixed to the edge of each blade, 6 cms. from the point. After introducing the speculum, the cervix is seized with the volsellum forceps, which may then be fixed to the edge of the speculum by the hooks on the blades. The advantage of the instrument lies in the operator's hands being free to introduce the gauze, without any chance of the latter getting contaminated from contact with the external parts or any discharge.

THERAPEUTICS.

UNDER THE CHARGE OF

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Treatment of Corpulence with Thyroid Preparations.

Ebstein, who is well known as an authority on the treatment of obesity, summarises the value of thyroid preparations in this condition as follows (*Deutsche med. Wchnschr.*, Leipzig, Feb. 1, 1899):—(1) Treatment with thyroid gland is unsatisfactory, because the decrease in weight is very inconstant; often it has absolutely no effect. Sometimes it soon ceases to have any action, and may greatly depress the general feeling of well-being. (2) Muscle tissue as well as fat is lost, in those cases where its action is well marked. (3) It is unnecessary, as the dietetic cure is fairly satisfactory, and not so dangerous to the general health.

The Prevention of Premature Old Age (Senilitas Præcox).

This is discussed by Hermann Weber (*Ztschr. f. diätet. u. physik. Therap.*, Leipzig, 1898). Early senility of the nervous system is due chiefly to cardiac and arterial degeneration, and is seen chiefly in the male sex. Its prophylaxis is to be sought in bodily and mental activity and moderation in living. It is very often hereditary, and in such cases treatment should be commenced early in life, about the twentieth year. Habitual moderation in eating and drinking should be enjoined, with a fair amount of athletic exercise in the open air, taken regularly and systematically. If possible one whole day weekly should be spent in the open air, but fatigue should be avoided. Great moderation in eating and in the use of alcohol is especially necessary. Excess in tobacco and in the sexual functions should be avoided. The hours spent in sleep should not exceed eight out of the twenty-four, and after fifty years of age five or six hours is enough. Early rising conserves the intellectual energy. It is especially important to maintain a good flow of spirits, and to avoid a habitual ennui, low spirits, or depressing emotions. The absence of occupation, such as follows retiral from business, often precipitates the changes inevitable to old age. Hence, in every case, some "hobby" should be indulged.

COSAPRIN AND PHESIN.

THESE two bodies are proposed as substitutes for antifebrin and phenacetin, and a short account of their clinical value is given by Rosen (*Therap. Monatsh.*, Berlin, March 1899). They are sulpho-derivatives of the other substances and are much less poisonous. While 7 grs. antifebrin kills a rabbit in about an hour, as much as 90 grs. cosaprin produces no symptoms of poisoning; while 15 grs. phenacetin is the lethal dose for a rabbit, 30 grs. of phesin has no effect, and 60 grs. only causes slight poisoning, the symptoms being the same as in the case of phenacetin. As antipyretics, observations made on animals, in which fever had been produced artificially, showed that larger doses are required, and that the effects are not so lasting, but in no case did any poisonous effects ensue. Phesin is a brownish powder, without smell, with saltish pungent taste, and soluble in water. Cosaprin is a greyish white odourless powder with a slight saline taste, and also soluble in water. Rosen has used them in fifty cases instead of antifebrin and phenacetin. They have invariably been well borne up to doses of 90 grs. daily, and even in dyspeptics, chlorotics, and children no ill effects have ever been observed. Perspiration, sometimes very profuse, is, however, a frequent accompaniment of the action of large doses. As antipyretics in fever, they proved to be comparatively valueless clinically. Rosen advises their employment specially in rheumatic diseases and influenza. In cases of influenza cosaprin was given in doses of 75 to 90 grs. in twenty-four hours, and within a day marked improvement was always manifest, so that the dose could be reduced to about one-half, and in three or four days the symptoms had usually disappeared. In muscular rheumatism, neuralgia, sore throat, etc., the results were equally good. In chronic rheumatism the improvement was practically nil, and the pain was not lessened.

TREATMENT OF CONSUMPTIVES IN SANATORIA.

SOMMERFELD gave an address on this subject to the seventieth meeting of German naturalists and physicians. He states at the outset that he has complete sympathy with the so-called open-air treatment of consumption, as pursued in institutions specially arranged for the purpose, but thinks that, owing to its wide acceptance throughout the whole world at the present time, there is an urgent necessity for a clear understanding of what it is capable of accomplishing. He lays stress on the fact that physicians have great difficulty in determining the points which enable one to definitely state that there is real improvement in the condition of a consumptive patient. Great importance has been laid on an increase of body-weight, without much attention being paid to whether the increase is due to fat or muscle. According to Sommerfeld, it is in most cases merely the subcutaneous fat which is increased, and this as a result of over-feeding. Such fat does not contribute beyond a certain point to improved health, and is often very soon lost when the patient returns to an ordinary life and dietary. In many of these cases the tubercular process continues to spread, and does not seem to be affected favourably in its local manifestations. Increase of fat can be brought about equally well in hospitals or at home, by a superabundant dietary, especially in persons who from any cause have been in the habit of eating little. An increase of body fat is therefore no criterion of the value of the treatment. Much more important is an increase of the muscle tissue and of the lung capacity. In many cases excess of fat is rather a drawback. Hence, carefully regulated exercise and gymnastics to develop the chest should form a principal part of all treatment in incipient and chronic cases. The condition of the sputum, as regards the number of bacilli present, also affords comparatively slight grounds for an opinion as to the general progress of a case, although it has a certain value. The condition which is revealed by physical examination of the chest remains, but here the presence of mixed infection, the amount of inflammatory exudation, and other conditions locally, such as bronchial catarrh, make an exact decision very difficult. The course of the disease is very chronic, there are frequent local changes in the physical signs, and healing takes place only by a very slow process of calcification, or fibrous encapsulation. Observation of a patient extending over a short period of time will not enable us to come to any trustworthy decision, as treatment very often lessens such complications as catarrh or suppuration, and induces an apparent improvement, without greatly affecting the tubercular lesion. At the same time the general health and condition may improve, and although this tends to give the patient a better chance, yet it may not be sufficient to turn the scale against the tubercle bacilli. Another important consideration is, that at any time new bacillary foci may form in the lung, and these may be numerous and frequently difficult to detect. At any time the tubercular infection may become general throughout the lung.

We have as yet no knowledge of the extent to which sanatorium treatment has prolonged or saved life in the mass. Such knowledge cannot be exact in any case, but a decision is impossible until we have statistics, extending over many years, and a large number of cases, of the after-history of patients who have been pronounced cured or greatly improved by such treatment. Finally, such patients are often peculiarly susceptible to reinfection, and the treatment cannot be considered as ended when they leave the institution.

CLINICAL NOTES ON TREATMENT OF ASTHMA.

BEVERLEY ROBINSON contributes an article on this subject (*Therap. Gaz.*, Detroit, Jan. 1899). He states that he has met with very few cases of so-called nervous asthma, or asthma merely functional and without fixed causation, and believes that with advancing knowledge such cases will no longer be described. He says that in cases where asthmatic attacks occur in malarial subjects, Fowler's solution should be given boldly in gradually increasing doses. If, at the same time, the bowels be constipated and the liver inactive, he advises Warburg's extract in 5-gr. doses three or four times daily. If anæmia be conspicuous, arsenic, iron, and quinine are given. To lessen the severity of the actual attacks, antispasmodic remedies, such as belladonna, chloral, and nitro-glycerin may be had recourse to. The smoke from stramonium cigarettes or nitre paper may be inhaled, or, in the severest cases, chloroform, or a hypodermic of morphine and atropine may be necessary. He believes that gout and rheumatism are powerful in causing nervous irritability, and inflammatory conditions of the throat, with a tendency to have attacks of asthma on slight cause. The determining causes may be over-fatigue, errors of diet, exposure to bad weather, anxieties, and other depressing states. Renal inadequacy causes certain bodies to be retained in the system which are ordinarily eliminated by the kidneys, and these bodies give rise to dyspnœic attacks. In such cases colchicum causes their elimination, and does away with the asthma. It is highly probable that anæmia, constipation, dysmenorrhœa, and general neurasthenia are more than sufficient to initiate in certain susceptible women attacks of asthma, which can only be effectually treated where those morbid states are judiciously dealt with. Reflex causes of asthma are sometimes found in certain conditions of the throat and nose, such as adenoids, hypertrophy of the covering of the turbinated bones, polypi, enlarged tonsils, etc., and these must be dealt with. But Robinson thinks that the importance of such conditions has been greatly exaggerated, as the asthmatic attacks often persist after their removal. Where there is chronic gastric catarrh, brought on by errors of diet or alcoholic habits, frequent lavage of the stomach and a regulated diet often greatly lessen the frequency of the spasmodic attacks. An occasional mercurial purge, with the regular use of an alkaline mineral water, also affords relief. But, after all, the great majority of cases are complicated with bronchitis and emphysema. Taking cold from any cause brings on the attack. The patients generally say that they are perfectly well between each attack, yet on closer inquiry it is almost invariably found that they suffer from shortness of breath and palpitation on any slight exertion. On physical examination, more or less pronounced emphysema is found, which always tends to increase. In such cases even the breathing of an impure atmosphere is often sufficient to cause some bronchitis with asthma, and often with very scanty secretion. Where the bronchitis is clearly defined and the secretion slight, the main effort should be directed to increasing the latter by means of small doses of ipecacuanha, tartar emetic, grindelia robusta, chloride of ammonium, or iodide of potassium. Where the bronchial secretion is too abundant, belladonna may be given either alone, or in combination with camphor and quinine. The action of these drugs is relatively slow, and antispasmodics may be necessary during the attack. Where the heart is evidently dilated with much cyanosis, repeated small doses of nitrites or the soluble salicylate of caffeine should be given. Occasionally bleeding, leeching, or wet-cupping are necessary before the desired relief is obtained. Usually, however, such measures are only temporary, and the distended right heart soon becomes again engorged and unable to struggle against the vascular conditions in the lung. When this is the case, one must be particularly careful in using such drugs as amyl nitrite, which sometimes seems to increase the pulmonary congestion, and may cause an alarming condition, especially in old cases with much emphysema. In severe attacks no remedy gives such marked and instant relief as the hypodermic injection of morphine. But there are cases in which morphine seems to increase the patient's dyspnœa, and may bring on sickness and gastralgia. It is always contra-indicated when there is albuminuria.

Change of locality or climate may give freedom from asthma, but it is very difficult to give definite advice about this. Often no good results, sometimes there is temporary relief. As a general rule, a climate suitable for the subjects of bronchitis is also found most suitable for those who have asthma.

INSANITY.

UNDER THE CHARGE OF
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THE PATHOLOGY OF EPILEPSY.

IN an important article in the *Alienist and Neurol.*, St. Louis, Oct. 1898, Dr. N. Krainsky discusses this subject. Beginning with Haig's researches into the excretion of uric acid in epilepsy and migraine, Krainsky confirms the former's observations with regard to the excretion of uric acid in epilepsy. He found a diminution in the excretion of uric acid to precede every epileptic seizure. This diminution usually occurred from twenty-four to forty-eight hours before the fit. If 0·6 to 0·8 grm. of uric acid are excreted daily, no fit will take place; if the daily quantity of uric acid excreted falls below 0·45, or especially below 0·35 grm., a seizure is usually to be expected with certainty on the third day. On the following second day the quantity of uric acid is often normal, and on the third day a seizure occurs with a marked increase in the preceding diminution. Consequently there is no change in the actual amount of uric acid excreted, but only in the time of its excretion. In some cases no diminution of uric acid is to be noticed during the twenty-four hours prior to the seizure, but it falls below the normal for several preceding days, so that the uric acid is not suddenly but gradually accumulated in the organism. Further, a certain relation may be determined between the lessened quantity of uric acid and the intensity and number of the seizures. The greater the amount retained, the more violent will be the expected seizure, and inversely. Notwithstanding this fact, which the author believes he has placed beyond dispute, he does not consider that the uric acid is the cause of the fit; and the only deduction he draws is, that epilepsy is not a purely nervous disease, but is due to an anomaly of metabolism, which has its basis in an intoxication of the organism by a product of disordered metabolism; and that the lessened production and excretion of uric acid is only an indication of this disorder. It is very possible that the epileptic seizure itself is the means of checking the further continuance of this abnormal reaction, and that after it the metabolic processes are permitted to proceed in the ordinary way. The formation as well as the destruction of this toxic substance is probably closely connected with the formation of uric acid, since the latter is so markedly decreased prior to the seizures and increased after them. When remedies favouring the excretion of uric acid were administered, no change occurred in the number of the fits, from which the author concludes that the cause of the seizures is to be sought neither in uric acid nor in its retention in the system. The author then proceeds to investigate the nature of the toxic substance present in the organism in epilepsy. The poison must be either in the brain or in the blood. He made experiments accordingly with the brain of an epileptic who had died suddenly during a violent seizure. An extract was made with great care, and half a gramme of the substance injected under the skin of a rabbit. The only effect was a slight rise of temperature. His attention was then turned to the blood of epileptics and its toxicity. Blood taken by a cupping-glass from a patient in status epilepticus during the time of a seizure, of which 2 c.c. were injected subcutaneously into a rabbit's groin, produced a violent epileptic seizure in from two to three minutes. The seizure was attended by cries, by clonic spasms, and lasted two minutes, when the animal's posterior extremities were found to be paralysed. The rabbit soon recovered, but the paralysis remained. The same day, five hours after the injection, a second seizure occurred, and in four days the rabbit had about ten seizures, from which it died. Similar results were obtained in two other cases. Control experiments were made with the blood of patients in the interval between the attacks, but they were followed by no results. In other cases the rabbits remained perfectly healthy for a few days after the injection of from 5 to 10 oz. of blood withdrawn from patients in the interval between the seizures; but after two to three weeks they became emaciated, paralysis of the posterior extremities set in, and they died in comatose condition. The animals had a good appetite until their death, and even manifested a certain voracity; but no changes were found in the post-mortem examination. The author is unable to explain the periodical recurrence of the seizures in rabbits after one injection—an effect which strongly resembles that of the ferments. Having come to the conclusion that the toxin is not uric acid, and further, having proved by a method which need not be recapitulated here, that it does not belong to the uric acid group, he proceeds to show that it must belong to the group of one of the substances forming uric acid, that is, it must be either a derivative of organic acid or urea. Among the former, carbamic acid must be first of all considered, as it is closely related to urea in the reaction of its formation. Among the derivatives of carbamic, carbamate of ammonium alone corresponds to the required substance, both by its chemical composition, by its extreme instability, and by its high toxicity. Are epileptic seizures then due to the accumulation or formation of carbamate of ammonium in the organism? The following facts support this view:—The origin of carbamate of ammonium occurs at the expense of urea, namely, of that part which is essential for the synthetic formation of uric acid, so that the urea takes up a molecule of water, which can neither enter into the formation of uric acid (hence the lessened formation and excretion of the latter), nor be simultaneously excreted with the total quantity of urea. As the carbamate of ammonium remains in the blood, it accumulates until it produces a seizure; during the seizure it is transformed into urea, with the loss of a molecule of water, which immediately enters into synthetic reaction, and is excreted in the form of the formerly retained quantity of uric acid. Carbamate of ammonium is very decomposable, and, by taking up a molecule of water, is transformed into ammonium carbonate. Carbonate of ammonium is also readily decomposed, and almost always contains carbamate of ammonium in greater or less quantity. The author then proceeds to discuss the toxic effects of the introduction of carbamate of ammonium into the circulation of animals, and quotes the experiments of a large number of investigators to show the poisonous effects of carbamic acid and its salts, especially the observations of Nencki and Pawlow, who made fistulæ in animals, connecting the portal vein with the inferior vena cava. In animals thus operated on, carbamic acid was always found in the urine. Various toxic symptoms were found in these animals, chiefly somnolence with ataxia, excitement with ataxia and blindness, catalepsy with anæsthesia, epilepsy and tetany. In his own experiments the author freshly prepared the carbamate of ammonium by passing dry carbamic acid into dry ammonia gas. After the injection of fixed quantities, 0·3 to 0·4 grm., the animals remained perfectly normal for from ten to twenty minutes; then became somnolent and ataxic. After about five minutes a very marked increase of the reflexes occurred; this condition lasted for from one to four minutes, when usually a typical epileptic fit suddenly occurred, which occasionally attacked the animal while standing. If the seizure was not repeated, the animal recovered completely in about one to two hours; but if, after the first seizure, the reflex exaggeration and morbid twitchings did not cease in ten or fifteen minutes, another epileptic seizure usually occurred, when the subsequent seizures became weaker and of shorter duration. If 0·5 grm. is injected, a number of seizures will occur, separated from each other by more or less long intervals, and the animal usually dies during one of them. From an injection of a larger dose of carbamate of ammonium, typical status epilepticus usually occurred. Those rabbits which recovered from the seizures were perfectly healthy the following day; and yet, after several seizures, the rabbits often died in a post-epileptic comatose condition. The author found that the carbonates of ammonium and of potassium were perfectly harmless, even when injected to the amount of 1 grm. to 1 kilo. of the body weight. For this and other reasons he believes that the toxic effect is due, not to the carbamic acid, but to the ammonium, which acts within the organism as the carbamate of ammonium.

Having proved that carbamate of ammonium is highly toxic to the animal organism, producing nervous depression, true epilepsy and coma, he proceeds to the discussion of the question, whether the carbamic acid or the ammonium itself is the essentially toxic agent, and concludes, from a series of experiments, that the toxic effect in question must not be ascribed to carbamic acid, but to the ammonium, which, however, acts within the organism in the form of carbamate of ammonium. In order to ascertain whether the carbamate of ammonium exists to any extent in the blood of epileptics, he devoted a series of investigations to this point. The question of the presence of carbamic acid in the urine was neglected, because it is usually eliminated from the organism in the form of urea. No carbamic acid may be present in the urine, although existing in the blood, or it may present itself in the form of the sodium salts, and have no connection with the existing intoxication. The result of analysis of large quantities of epileptic blood, taken by means of cupping glasses during fits and in the intervals between them, showed that carbamic acid was present in very large amount in the blood of epileptics. The more severe the symptoms, fits, coma, or excitement, but especially in the status epilepticus, the more evident was the reaction of carbamic acid. Inversely, although in the intervals between the seizures the reaction was always very weak, still traces of it could be obtained. Carbamic was also found in very small amount in the blood of healthy persons, but never to the same extent as in epileptics. It is observed that Drechsel found carbamic acid as a normal constituent in the blood of dogs, and it is also known to be present in healthy human blood, in combination with potassium and sodium. The author is, however, satisfied that he has established its presence in excess in epilepsy. The next question was to decide the presence of free ammonia. The blood was extracted in the same manner. The difficulties experienced on account of the minimum quantity of blood required (100 c.c.) for each analysis were, of course, very great, and the number of experiments was consequently very limited. He found, however, that the quantity of free ammonia in 100 c.c. of epileptic blood varied from 4·3 mgrms. in the interval between the fit to 8·5 mgrms. extracted during the fit. The result of this investigation was that ammonia is present in the blood of epileptics in excessive quantity, being larger when the epileptic manifestations are more severe. The author admits that his investigations are incomplete, in so far as he has been unable to estimate the quantity of carbamate of ammonium in the blood of epileptics, for he believes, in opposition to Professor Nencki, that all the carbamic acid in the blood is not in combination with ammonia, but that some of it may be combined with potassium and sodium also. He also believes that ammonia exists in the form of the carbonate, as well as of the carbamate. Hence he is unable to conclude that the quantities of carbamic acid and ammonia obtained by analysis represent the amount of carbamate of ammonium present in the blood.

If we consider the periodic origin of carbamate of ammonia as the cause of epileptic seizures, the author proposes to enlighten us with regard to the favourable therapeutic effect of several anti-epileptic remedies. The bromides of potassium and sodium mutually decompose the carbamate of ammonium, and form ammonium bromide and carbamate of potassium or sodium. The ammonium bromide formed by this reaction is comparatively harmless, and is moreover excreted from the organism before it can produce the toxic effect of the ammonium salts. From this point of view, the bromide in itself does not seem to be the effectual agent, but rather the alkali, which, in combination with the bromide, most readily enters into the above described chemical reaction. The bromide alkalies answer this purpose exactly, for, as they form none of the elementary components of the organism, they appear as foreign bodies, and exchange their base most readily in favour of the ammonium combined with the carbamic acid. Sodium chloride, normally present in the organism, possesses no therapeutic effect, for the reason that the atomic weight of chloride is less than half the atomic weight of bromide, and because the ammonium chloride would in the presence of the carbonate of sodium be again immediately transformed into the carbamate of ammonium. But the constant neutralisation of the poison cannot prevent its renewed formation; and the problem of the treatment of epilepsy will only be solved when the cause and origin of the abnormal metabolic reaction in the epileptic's organism are better understood. With regard to the pathogenesis of traumatic epilepsy, while remarking that traumatic epilepsy may be entirely different, and the seizures be produced by local cortical irritations or focal diseases, yet it is probable that in many cases the traumatic epilepsy differs in no way from the idiopathic, and that the trauma is only a predisposing factor, on the basis of which the genuine epilepsy is developed, exactly in the same way as a tuberculous joint follows an injury.

DERMATOLOGY.

UNDER THE CHARGE OF

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The Inheritance of Syphilis.

The question of the transmission of syphilis from parent to offspring is no longer contested, but there are conditions of the mode of communication which are still imperfectly cleared up. Finger, in an elaborate and thoughtful essay, has discussed these (*Wien. Klinik*, April and May 1898). In the first place, the inquiry is made whether the transmission of syphilis from parent to child is a necessary or only a possible circumstance? The reply is, that it is not obligatory. In the case of fathers with recent, well-developed, and untreated disease, healthy infants are but seldom procreated, for in such cases the mothers are usually infected; and so, in consequence of the recent syphilis of both parents, the position of the infant is much more unfavourable. Still, should the mother happily escape infection, she may conceive a healthy infant. It is rarer for the child to escape the inheritance in cases of recent maternal syphilis, yet such may also occur. Even in the case of twins, one may be born syphilitic and the other healthy, and remain so. Therefore it may be laid down, that in no phase of the complaint is inheritance a necessary but only a possible result. The syphilitic virus is only mechanically mixed with the substance of the organism or with the blood. It proliferates in a series of local areas, and occasions there pathological changes, but those organs not visited by the virus are completely sound, and can carry on their functions normally. In the communication of syphilis the virus is mechanically attached to the ovum or to the spermatozoon, and one or other may escape. The semen may acquire the virus from the blood current, the testes being sound, or from some erosion with which it may come in contact during emission. The same may be said of maternal transmission, and Lesser explains the fact that the possibility of communication of maternal, lasts considerably longer than of paternal, by the difference in the retention of the germ cells in the two sexes. The ovarian cells are already formed in early youth. Should, therefore, a woman be infected with syphilis, the disease germs can penetrate into the ova, and rest there, till after a long time—possibly ten or more years—such ova are fertilised, and then the dormant virus of syphilis is roused into activity. The circumstances are otherwise in men, whose sperm cells are quickly renewed and rapidly discharged in large quantity, and with them the deposited virus is at the same time got rid of. Another point in the transmission of syphilis is the question whether the father alone, when he is syphilitic, the mother escaping infection, can impart it to his children. Opinions on this have been divided. Many have held that every mother of a syphilitic child is herself always syphilitic, the source here being retro-infection from the fœtus. This would only hold for the first child, as, in the case of succeeding ones, the syphilis would be derived from both parents. If the syphilis sprang from the mother, treatment of the father alone could have no favourable effect on the offspring. But there are undoubted instances where, to a married pair, after one or several children have been born syphilitic, a healthy child follows, so soon as the father only has undergone antisyphilitic medication. This proves that in such cases the syphilis of the children was exclusively paternal. Again, the converse has been shown to occur, in which, under the supposition that the syphilis sprang from the mother of diseased children, she has been treated without obtaining the desired result, the children born being still syphilitic. Further proof is afforded by cases in which the mother of children deriving their syphilis from the father, has, in a subsequent marriage, borne sound children. Still more conclusive are some cases, where a woman bore to a man, affected with syphilis, a diseased child; to another healthy man, a healthy child; and again a diseased one to the first. At the same time, there are cases not entirely free from objection, in which women in a first marriage with a syphilitic husband, bore diseased children; in a second, with a healthy man, still produced syphilitic infants. In such the mother was, to all appearance, free from syphilis. The purely maternal transmission of syphilis, the father being healthy, is pretty generally accepted, and is proved—(1) By the fact that a syphilitic woman produced diseased children to several healthy fathers; (2) by cases in which a post-conceptional syphilis, derived from the father, and communicated to the mother, has affected the infant. Fifty cases are cited in support of this, not all of equal value, but many thoroughly convincing. The earlier in pregnancy the disease is acquired, the more severe, as a rule, the manifestations in the child, but to this there are exceptions, possibly owing to some unexplained delay in the transmission. A further question arises, What influence has the fœtus, deriving its syphilis from the father, on its mother? This counteraction varies. One result is, that it may infect the mother by *choc en retour*, or conceptional syphilis. In this the mother, without any visible initial lesion during pregnancy, suffers from secondary symptoms. She may either receive infection from the fœtus by the placenta, or be directly infected by the father, the initial lesion not having been seen. Nor may there be any glandular enlargement discoverable, the deep pelvic glands being primarily and most severely involved. Another result is the occurrence of late conceptional syphilis. In this, the mother of children deriving their syphilis from the father, neither during pregnancy, nor for a long period after, exhibits any sign of syphilis, till finally—according to Fleiner, chiefly at the climacterium—she develops tertiary symptoms. These are probably due to the transmission of, from the fœtus to the mother, not the virus itself but the toxins. It may be objected that the mother may have had secondary symptoms, but these escaped observation, since it is very rare for any female to be under close scientific observation throughout long consecutive periods. Some cases are cited, however, which provide almost conclusive proof. In a second group of cases, where the mother of syphilitic children, derived from a diseased father, remains quite well, she differs from others, inasmuch as she is completely immune against syphilis. This is known as Colles' law. This has not only been proved by the fact that, while a wet-nurse or one employed in tending the children has contracted the disease from them, she escapes, but it has also been tested experimentally. This immunity must not be confounded with a syphilisation of the mother. The mother has not received from the babe *in utero* the syphilitic virus, but the toxins dissolved in the infantile tissues have passed by osmosis through the placenta, in the mother have acted as antitoxins, and thus induced immunity. Whether this immunity is absolute or relative, of longer or shorter duration, we do not know. In yet a third, but so far as our knowledge goes a small group of cases, the mother remains free from any effect, so far as she is concerned, arising from the presence of a child in her uterus whose syphilis is due to the father. She continues healthy, but is not immune, and can after the birth of the syphilitic infant be infected by it or in some other way. This group consists, therefore, in exceptions to Colles' law, and furnishes the most irresistible proof of the existence of pure paternal syphilis. For the establishment of such cases two postulates must be fulfilled—(1) The hereditary syphilis of the child must be proved beyond suspicion, all possibility of the disease having been acquired must be excluded; (2) it must also be determined unquestionably, that the mother of the child acquired her syphilis after the infant's birth. Fifteen undoubted instances are in existence. For some of these the explanation of Hochsinger may serve. The children received from their father a virus much reduced in activity, which remained latent during intra-uterine life, thus gave rise to no toxins, and only became active after the birth of the child. The impression is conveyed, in other cases, that the mother for some time after delivery was immune, but soon lost this. It has been seen that, notwithstanding the syphilis of the parents, a certain proportion of their children are free from it. Are such children wholly unaffected by the parental syphilis, to such an extent that they may themselves acquire syphilis, or are they not? The general view is, that the offspring of syphilitic parents, if they escape infection, inherit an immunity from their parents which protects them from contracting it. Profeta formulated a law, that a syphilitic mother may suckle, tend, and kiss her healthy infant, but she cannot infect it, even though she may have contagious forms of the disease on the nipples or lips. The infant possesses a true intra-uterine acquired immunity. This immunity has not yet, it is true, been tested by inoculation, nor perhaps is likely to be. Exceptions to Profeta's law, as to Colles', have been met with. In explanation of such, either the toxins, which are abundantly produced in the mother in some way, do not pass the immunity on to the child, or this is of very transient duration. As a further effect of parental syphilis on posterity, it is pretty widely held that it can assert itself after this fashion, that the children are born and remain for long quite healthy, till—principally at puberty or even later—they manifest purely tertiary symptoms. This is termed late hereditary syphilis. No case wholly free from objection has been published, since none have been watched continuously with sufficient care throughout. But, if not proved, such cases are theoretically possible. They are analogous to the late conceptional syphilis of the mother. The direct cause of tertiary lesions is not the syphilitic virus but its toxins. These produce immunity, and by the transmission of the qualitatively and quantitatively greater amounts of toxin, give rise to tertiary phenomena. Both the virus and its toxins can occasion forms of disease closely allied clinically. The transition forms of lesion are explained, inasmuch as they correspond to the intermediate stage, in which the virus on one hand commences to leave the body, and on the other inaugurates gradually a thorough tissue change. But this tissue change is neither uniform nor simultaneous for the entire organism. Those tissues in which, in the early period, there was a localised rank proliferation of the virus, became over-saturated with toxins, and were more rapidly transformed. Thus it occurs that many situations which were the seat of secondary phenomena, are also attacked by tertiary ones. The toxin circulating in the blood is not uniformly distributed in the organism, but is stored up predominantly in certain organs, in which in consequence the resulting alterations are greater. The non-contagiousness of tertiary forms is a notable point, which stands in direct opposition to the existing presence of the virus. The lesions of the transition stage are contagious, and those instances, in which infection has occurred from syphilis of old standing, are accounted for, on the view that the contagious secondary period of the disease extends in such over the normal time limit. Another matter is the possibility of reinfection of an individual affected with tertiary syphilis. The tertiary phenomena are explained by supposing that certain tissues and organs, as a result of the local accumulation of large amounts of the more virulent toxins in the early period, experience changes which predispose them to tertiary disease. Herein lies the generally recognised characterisation of tertiary syphilis as a "local sequela." Immunity is the property of the whole frame, only certain tissues succumb to tertiary disease as a purely local disorder. General immunity becoming extinct, reinfection is possible; the transformation of individual tissues, by preceding intense saturation, constituting the local proclivity to tertiary disease, remains; just as after numerous other systemic poisonings, local disturbances or the disposition to such persists, even though the original poison has already wholly passed out of the body.

A last point concerns the permeability of the placenta. It is—(1) permeable for the virus in one group of cases—this permeability in the direction of mother to child is certain, from child to mother is probable, but hitherto not absolutely proved; (2) impermeable for the virus, but permeable for the immunising toxins in a second group of cases, and both in the direction of mother to child, and the reverse; (3) absolutely impermeable in a third small group of cases. Neither virus nor immunising toxins pass, the healthy system remains quite uninfluenced by the ailment. Under what circumstances and conditions the placenta proves now permeable and again impermeable, we cannot discern. The rule indeed holds for one series of instances, that the sound placenta is impermeable for the virus, the diseased is permeable; but there are marked exceptions to this rule, and we meet with healthy mothers in cases of paternal fœtal syphilis, notwithstanding extensive disease of the placenta materna; with healthy fœtuses in post-conceptional syphilis of the mother, notwithstanding extensive disease of the placenta fœtalis and of the vessels of the umbilical cord. Moreover, what circumstances promote or prevent the transmission of the dissolved immunising toxins, are completely beyond our knowledge. Campbell Williams (*Brit. Journ. Dermat.*, London, Feb. 1899) raises the question, why, in congenital syphilis, when the child is apparently born healthy, the *first* dermal or condylomatous signs of the disease occur so often at about the eighth to the tenth week after birth? This corresponds roughly to the date of the appearance of eruptions in the acquired form. The mothers of such infants do not usually exhibit any indisputable evidence, past or present, of having suffered. Yet, on questioning, they commonly admit that their health has not been so good since as before conception, and if mercury is given them they improve. Now, it has been held that when a mother bears a congenitally syphilitic child, and yet shows no sign of the disease herself, that, through being infected by the medium of her embryo, she obtains, if not an immunity, a modified form of the disease, which does not extend in the same degree to the child *after* birth. His view of the reason why the child shows no signs of syphilis at its nativity, is that, while it is *in utero*, its tissues are practically a part of its mother's, and share in that agency, whatever its exact nature may be, that keeps in abeyance the outward and visible signs in the mother. This influence is presumably a circulatory one, and a reasonable deduction is, that it is some substance in the maternal blood, that not only keeps in abeyance all symptoms of the disease in herself, but likewise in her fœtus. So long as it is subject to this influence, it shows no objective sign, but when it gains a separate existence, and is consequently removed therefrom, the virus latent in its system assumes a slowly progressive activity.

DISEASES OF CHILDREN.

UNDER THE CHARGE OF

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MOVABLE KIDNEY IN CHILDREN.

THIS condition, which occurs with comparative frequency in adult females, has hitherto been regarded as an extremely rare occurrence before the age of puberty, and we fail to find any mention of it in the standard works on diseases of children.

Dr. Comby, who has devoted considerable attention to this subject during the last few years, describes eighteen cases which have come under his personal observation in adolescents and quite young children. He is of opinion that it is much more commonly present than has been previously supposed, and considers that the reason of its being so frequently overlooked is on account of the obscure nature of the symptoms which so often accompany it. He believes that if it were systematically searched for, when obscure abdominal symptoms present themselves in children, it would be found with much greater frequency; and he thinks that if the origin of many of the cases met with in the adult could be traced, it would often be found dating from early infancy. Sixteen of the cases described occurred in girls, and only two in boys, the condition having thus a strong predilection for the female sex, as in the adult.

In some cases the condition is a congenital one, and a floating kidney is found present at birth, whilst in others there is hereditary predisposition, cases being cited of mothers and their daughters being alike the subjects of this condition. An occasional cause is traumatism, as the result of violent efforts, blows, or falls, but the author does not consider this such a frequent cause in children as in adults. The majority of cases owe their origin, according to Comby, to chronic dyspepsia, with a dilated stomach and enlarged liver, and accompanied by great flatulent distension and slackening of the abdominal walls. This excessive dilatation puts a great strain on the ligaments and aponeurosis which maintain the principal viscera in their places, and frequently results in a displacement of the kidney from its bed. The symptoms of movable kidney are often obscure, and in the great majority of cases neither pain nor any other symptom drawing attention to the kidney is experienced. In some children, however, pains more or less violent are complained of, causing an abdominal examination to be made, with the discovery of a floating kidney. Occasionally there is considerable uneasiness in the displaced kidney, and sometimes, owing to the twisting of the ureters, an interference with the escape of the urine occurs, and a hydronephrosis results. In a few cases the pain and uneasiness is continuous, whilst in others they are paroxysmal, coming and going irregularly at longer or shorter intervals. At a later period of life, when menstruation is established, these neuralgic pains accompany the menstrual flow with periodical regularity. Occasionally uncontrollable vomiting or obstinate constipation results, but these symptoms are by no means usual. Almost invariably it is the right kidney which is displaced, it being quite exceptional to find a movable kidney on the left side. The usual explanation given for the great right-sided predominance of this condition is, that the enlargement and congestion of the liver, so frequently found in children, is responsible for the displacement. The displacement, once it has occurred, is permanent, and shows no tendency towards a natural cure, but, as already said, this natural incurability is compatible with perfect health. As a rule, the diagnosis of the condition is easily arrived at by a carefully conducted abdominal examination, the floating kidney being brought between the two examining hands with comparatively little manipulation. We must be careful to exclude appendicitis, and an accumulation of fæcal matter in the cæcum and ascending colon, before being satisfied with our diagnosis. In many of the cases no treatment is required; but when the displacement becomes troublesome, when the kidney is felt to be tender by the patient, the best method of relieving the unpleasant sensations is to encircle the child's abdomen several times with a broad flannel bandage, and thus to some extent give support to the organ by the pressure made on the abdominal walls. If, in spite of this treatment, the pains continue severe, if the urinary excretion is interfered with, if an intermittent or permanent hydronephrosis becomes established, we must have recourse to surgical measures. The operation consists in fixing the kidney by means of ligatures to the posterior abdominal wall. —*Arch. de mal. des enf.*, October 1898, p. 595.

THE TREATMENT OF PNEUMONIA IN CHILDREN.

DR. EMMETT HOLT, discussing this subject at the Pediatric Section of the New York Academy of Medicine, divided the cases into three groups, namely—(1) Those in which death results from exhaustion; (2) those in which it is due to complications; and (3) those in which the lethal result is attributable chiefly to acute toxæmia. As to the relative frequency of each in hospital practice, he estimated that 60 per cent. of the fatal cases belonged to the first group, 25 per cent. to the second, and 15 per cent. to the third class. A hospital patient suffering from pneumonia should have not less than 1000 cubic feet of air space, and such cases should be frequently changed from one ward to another, in order to still further insure a supply of fresh air. It is desirable that water should be freely given, and this can best be done by adding it to the child's food. No medicine should be administered, except to meet some definite indication. The cough can be better managed by inhalations of creasote or eucalyptus than by cough medicines. Moderate counter-irritation is desirable, and can best be maintained by the use of a mustard paste for a few minutes every three or four hours. Where cardiac or respiratory stimulants are indicated, he prefers strychnine, nitroglycerin, oxygen, alcohol, or caffeine, in about the order named. The dose of strychnine should rarely exceed $\frac{1}{200}$ gr. every three hours for a child of 1 year; and while $\frac{1}{400}$ gr. of nitroglycerin might be given hourly for a few doses, it should not be continued in this strength very long, unless the interval is increased. Holt found it very useful where there was evidence of intense pulmonary congestion. One is apt to give alcoholic stimulants too freely, and it is rarely necessary to give more than an ounce of brandy in twenty-four hours. Oxygen is useful, but too expensive to be used very generally. Caffeine sometimes acts very well in doses of $\frac{1}{10}$ gr. High fever is best controlled by the cold pack.

The chief points in treatment may be summarised thus:—(1) No depleting measures are ever admissible; (2) hygienic treatment is indicated, such as fresh air, proper feeding, and good nursing; (3) no unnecessary medication is permissible; (4) many annoying symptoms may be relieved by local measures; (5) the administration of stimulants should be determined solely by the condition of the pulse; (6) high temperature is more safely and effectively controlled by cold than by drugs; (7) greater caution is necessary in the use of powerful drugs than is generally observed; (8) rest is quite as important in the treatment as in any other disease.—*Arch. Pediat.*, N. Y., January 1899.

Congenital Laryngeal Stridor.

The subject of this interesting monograph by Stamm was a well-developed child 8 weeks old, breast-fed, and, save for the respiratory stridor, in perfect health. From birth it was noticed that respiration was performed with considerable difficulty. Each inspiratory movement was accompanied by a loud stridulous crowing noise, while expiration was quite free and natural. On each inspiration there was well marked indrawing of the intercostal spaces at the base of the thorax and of the substernal notch. Notwithstanding the great dyspnœa present, there was no cyanosis. The voice of the child was strong, but when she cried loudly the dyspnœa became much feebler. The heart was normal, and there was no sign of rickets. The stridor continued to be loudly heard during the sleep of the child. The condition described is so frequently confounded with laryngeal spasm, that the author thinks it important to emphasise the points to be kept in mind in forming a diagnosis. Congenital stridor commences at birth, whereas laryngeal spasm usually developes during dentition. In the majority of cases of laryngeal spasm there are accompanying signs of rickets and tetany, neither of which occur in cases of congenital stridor; cyanosis and convulsions are frequently associated with laryngeal spasm, but do not accompany the condition under consideration. Attacks of laryngeal spasm are frequently provoked by the child crying, which has the effect in congenital stridor, on the contrary, of suppressing the dyspnœa. The stridor in the congenital condition continues during sleep, but the attacks of laryngeal spasm only occur during the waking hours. The author considers that this affection should be regarded as a functional trouble, having a cerebral origin. He believes that it is due to an arrest of development of certain centres of co-ordination, presiding over the movements of respiration, and having its seat in the calamus scriptorius, which, according to Horsley, is the centre governing the involuntary movements of the larynx. The prognosis the writer regards as generally favourable; the stridor, in the great majority of cases, gradually growing less apparent, and disappearing altogether in a few months.—*Ann. de méd. et chir.*, Bruxelles, Jan. 1899.

Pericarditis in Children.

M. Baginsky read a paper at the Medical Society of Berlin on sixty-four cases of pericarditis which he had observed in children under 14 years. The affection was caused in twenty-four cases by acute articular rheumatism; in eleven cases it was due to tuberculosis or to pleuro-pneumonia; seven times it was caused by erysipelas; six times by purulent pleurisy; and five times by severe diarrhœa. In six cases the cause was measles, accompanied by broncho-pneumonia; while scarlatina, suppurative otitis, and diphtheria each claimed two cases. The exudation was serous, fibrinous, hæmorrhagic, or purulent. The writer considers that the diagnosis of serous pericarditis is always very difficult, as, owing to the more horizontal position of the heart in children, and its relation to the liver, the information derived from percussion is far from satisfactory. In the fibrinous stage he thinks that little reliance can be placed on the palpation of præcordial friction, and he considers that in auscultating there is often more difficulty experienced in distinguishing between pericardial friction and a murmur produced by an accompanying endocarditis, than is generally admitted. In purulent pericarditis, which occurs so frequently in the course of the infectious diseases, the exudation is seldom abundant. The diagnosis in these cases the author regards as even more difficult than in cases of serous pericarditis. The bacteria which are found in the exudation are the coli bacillus, streptococcus, and staphylococcus. The most frequent cause of serous or sero-fibrinous pericarditis is articular rheumatism. If it occurs in young children it usually brings about an adhesion of the pericardial walls, which is so frequently the cause of the child's death on arriving at the age of puberty.—*Ann. de méd. et chir.*, Bruxelles, December 1898.

LUMBAR PUNCTURE IN CHILDREN.

AT the meeting of naturalists and physicians held at Düsseldorf last September, a paper was read on this subject by M. Pfaunder, who had performed nearly 200 lumbar punctures in Escherich's clinic. In tubercular meningitis the cerebro-spinal fluid drawn during life contained tubercle bacilli in 90 per cent. of all the cases. After death the bacillus was found in every case which was examined. In epidemic cerebro-spinal meningitis the micro-organisms found were the diplococcus of Weichselbaum, and the meningococcus of Heubner. In many of the atrophic children undergoing treatment in hospital the fluid contained micro-organisms. From a therapeutic point of view, whenever intracerebral pressure was directly threatening to shorten life, lumbar puncture acted effectively as a palliative measure. In many cases it relieved the intense pain in the head, the hyperæsthesia, the delirium, the coma, and the convulsions. In several cases of hydrocephalus a cure was brought about by this means. As an operation it might be regarded as free from risk, if ordinary precautions were employed.—*Rev. mens. d. mal. de l'enf.*, Paris, January 1899.

REPORTS OF THE MEDICAL SOCIETIES.

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Forfarshire Medical Association.—This Society met in Dundee on 1*st March* 1899.—Dr. **J. Mackie Whyte** in the chair.—Dr. **Mowat** showed a man, æt. 38, with molluscum fibrosum.—Professor **Kynoch** showed two cysts of the ovary removed by posterior colpotomy from a patient of 26 years of age.—Dr. **E. Thomson** spoke of a patient, æt. 28, who had pain for two years, from whom a large dermoid was removed.—Dr. **Halley** read a paper on the treatment of perforating ulcers of the foot by nerve stretching. In all, the ulcers had followed callosities, and had gone on increasing; in two of the cases they were painless. In all three there was a certain amount of anæsthesia, marked sweating of the foot, or in one case feet affected, and a varying amount of thickened epidermis. In none was there any affection of the central nervous system or of the circulatory system. In all, improvement for a time followed.—Mr. **Greig** then read the first part of his paper on "Two Years' Experience in the Surgical Out-patient Room of the Royal Infirmary, Dundee."

Edinburgh Medico-Chirurgical Society.—1*st March* 1899.—Sir **John Batty Tuke** in the chair.—Dr. **Wm. Elder** showed—(1) A case of absence of the fifth metacarpal bone; (2) a case of localised skin eruption for seventeen years; (3) a case of unmyotrophic lateral sclerosis.—Dr. **Scot Skirving** showed a patient with Charcot's disease, simulating double dislocation of the hip-joint.—Dr. **Cameron** and Dr. **Scot Skirving** showed a patient with total necrosis of the frontal bone.—Mr. **Stiles** showed—(1) A boy with extreme atrophy of the muscles of the calf and thigh in each limb, the result of infantile paralysis; (2) a child, æt. 7 months, with a large hydrencephalocele; (3) a child with coxa vara.—Mr. **Stiles** showed—(1) The viscera from a case of lymphosarcoma of the abdomen, which simulated abdominal tuberculosis clinically; (2) the thoracic viscera from a child who suffered from severe dyspnœa with expiratory stridor.—Dr. **Scot Skirving** showed—(1) A sequestrum of the entire frontal bone; (2) tuberculosis of the mamma; (3) a compound fracture of the carpus.—Mr. **Cathcart** showed a clinical research case, containing various reagents and apparatus for the bedside examination of sputum, discharges, etc.—Mr. **Cathcart** read a paper on "Peritonitis" as a cause of increased peristalsis in the rectum and in other parts of the intestine. Some cases he referred to went to prove that severe tenesmus can be caused by inflammation around the rectum, just as it may result from irritation of the mucous membrane. The pain of peritonitis and the pain of ordinary colic are probably due to the same thing, namely, a spasm of the muscular coat, resulting, in the first, from irritation of the peritoneal covering, and in the second, from irritation of the mucous lining. In peritonitis the other coats become quickly involved, and this probably accounts for there being no symptoms of tenesmus, etc., in certain cases. The diarrhœa of tubercular peritonitis is probably due to irritation of the peritoneum and not to ulceration of the mucous membrane.—Dr. D. C. WATSON read a paper on "The Etiology of Gout." He had recently been conducting some research into the question as to whether the salts of uric acid occurred normally in the blood of various animals, and he had found that in birds the salts of uric acid occur normally in the blood, though this is denied by Garrod. He had also found that in various pathological conditions, and notably in leucocythæmia, uric acid occurs in large amounts, and yet no gouty symptoms manifest themselves in these diseases. It is probable, therefore, that in gout other bodies than uric acid are the cause. The nature of these bodies is not well known yet. They may act so as to alter the tissues in such a way that uric acid is readily deposited. There is probably no relation between the amount of uric acid in the blood and the number of leucocytes.

ROYAL MEDICAL SOCIETY.—*17th February* 1899.—Dr. M'NAIR SCOTT in the chair.—Dr. G. B. SIMPSON described cases of idiopathic and of progressive muscular atrophy, Mr. C. H. J. WATSON, a case of hemiplegia, and Mr. MACKIE one of possible umbilical hernia occurring in a new-born infant.

24th February 1899.—Dr. HERRING in the chair.—Dr. EASON read a communication on "A Physical Sign in Lobar Pneumonia." He found from observation of a series of pneumonias, extending over two years, that where only one lung was affected, apical consolidation was almost invariably accompanied by dilatation of the pupil on the same side. This rarely occurred when the inflammation was confined to the lower lobe. This inequality of the pupils being marked from the outset, proved a useful diagnostic point in doubtful cases. The condition is probably due to some unexplained disturbance of the cervical sympathetic.—Dr. R. A. FLEMING gave a lantern demonstration on his recent researches as to the degeneration of nerve fibres and resultant changes in nerve-cells.

3rd March 1899.—Dr. SIMPSON in the chair.—Dr. WATERSTON described and showed specimens of a case of impacted calculus in the ureter of a patient who had died of phthisis. Although there was marked hydronephrosis of the kidney, the patient had not had any clinical history of a renal condition. He also described an abnormal origin of the internal circumflex artery, namely, from within the pelvis. —Dr. GOODALL described two cases, one of a man, æt. 30, who entered hospital in a state of collapse, with pain in the abdomen and across the back. There was dulness over the right lung base. Aortic aneurysm was found to have burst into the right pleura. The other case was that of a head injury, with unusual symptoms.—Dr. M'NAIR SCOTT described a case of head injury. Hæmorrhage into the lateral ventricles was diagnosed. A tumour of the pituitary body was found, lacerated from result of fall, blood having passed through the third ventricle into the lateral ones.

17th March 1899.—Dr. **Herring** in the Chair.—Captain **Moir**, I.M.S., read a communication on "A New Operation for Varix in the Leg," called "the garter operation." It consisted in a circular incision through everything down to the muscular fascia, all the veins being then ligatured. It had proved a highly successful method of treatment.—Dr. D. S. **Fraser** described a case of tetanus in a boy from a thorn wound in the head, which resulted fatally, in spite of the administration of tetanus antitoxin.—Dr. **Rose** showed a diphtheritic cast of the trachea and bronchi from a child. Antitoxin was administered and tracheotomy performed. The child recovered.

Glasgow Pathological and Clinical Society.—*13th March* 1899.—Dr. **Fleming** in the chair.—Dr. J. M. **Cowan** showed specimens and described certain features of a case of acute endocarditis. There was a double aortic murmur with some incompetence. While in Belvedere the spleen became the seat of an embolus. This ultimately became infected, and broke down, resulting in the formation of a large abscess.—In the absence of Dr. **Samson Gemmell**, Dr. **Cowan** showed the heart, with microscopical preparations, from a case of acute endocarditis.—Dr. **James Carslaw** showed specimens from and read notes of a case of malignant endocarditis, following pneumonia.—Professor **Ralph Stockman** illustrated, by means of microscopical specimens, the action of arsenic on the blood and bone-marrow. Arsenic was given in small doses to animals for lengthy periods, and the changes noticed were chiefly these—increase in redness and amount of the bone-marrow, with thickening and increase of the ends of the long bones. Microscopically the blood vessels and the cells of the bone-marrow were increased in number; the fat cells were diminished.—Dr. A. R. **Ferguson** showed microscopic specimens of the bacillus of bubonic plague.

OBITUARY.

Sir John Struthers, M.D., LL.D.

Although a native of Dunfermline, and Professor of Anatomy in Aberdeen for twenty-six years, Sir John Struthers was very closely connected with Edinburgh. Here he was a student, and here he graduated; here he was infected with his love for anatomical science, and began to lecture on that subject; within but two years of graduation he became Assistant-Surgeon, and afterwards full Surgeon to the Royal Infirmary; and last, but not by any means least, he learned to appreciate the value of practical teaching, and the benefit conferred on the student by strict attention to actual detail; he was thus led to advocate in a most persistent manner, throughout many years of his after-life, the introduction into the medical curriculum of a greater number of practical classes. It is with this action that his name will be chiefly associated in the future. He was led by these opinions, when appointed Professor of Anatomy in Aberdeen, to so improve the practical equipment of his department, that his museum and anatomical specimens soon became second to none in the country. After the reorganisation of his own department, Struthers, appointed to the University Court, applied himself to benefit the rest of the medical arrangements on similar lines, following this up by reformation of the Aberdeen Infirmary, which was undoubtedly most advantageous to that institution. When sent to the General Medical Council by his University, he was given facilities for the wider dissemination of his views as to the need of increased practical and clinical teaching of students, and it is probable he did much to educate the profession with regard to this question, and to bring about a general recognition of its benefits, a recognition which led to the adoption by the Council of the five years' regulations, with obligatory attendance upon a greater number of practical classes.

His life was spent in work. When he retired from his Professorship of Anatomy in 1889, and came to Edinburgh, he at once set about obtaining the wherewithal with which to occupy his leisure. He became a Manager of Leith Hospital, and reorganised it; he devoted hours to work in the Royal College of Surgeons Museum; when President of the College a few years ago, he largely remodelled many of its practical departments; appointed a Manager of the Royal Infirmary, he made use of some of his energy in procuring the erection of additional surgical operating theatres, and in formulating other schemes which seemed advisable to his reforming mind.

Sir John Struthers was a skilled anatomist, his love for details suited his subject; he early appreciated the value of the study of comparative anatomy; he was one of the first in Scotland to accept Darwin's theories, and to explain them in a popular sense; his work on the "Morphology of the Humpbacked Whale," and his numerous other anatomical papers, human and comparative, testify to his ability, and to his powers of laborious observation. He was by nature a reformer and organiser, apt, perhaps, in his later years to overlook the inconveniences brought upon individuals or individual groups in the application of a rigid general scheme, however well advised the main purpose of the scheme might be; a little prone to be stiffened in his resolve to carry out his schemes by the appearance of opposition, especially if it came from members of his own profession; but withal intensely sincere in his belief of the good which would accrue from them, and in most of them this belief has been confirmed by results.

A. Lockhart Gillespie.

William Rutherford, M.D., F.R.S.S., Lond. and Edin.

By the death of William Rutherford, the University of Edinburgh has lost one of its greatest teachers and physiology one of its most ardent workers.

By all his old pupils Professor Rutherford will always be remembered as a great teacher. His personality, his individuality (marked as they were), his thoughtfulness for his students, his love for congenial company, his enthusiasm for what he considered to be in the interests of the students, are somehow or other all merged in Rutherford the teacher, though the liking and respect for these qualities only serve to render our admiration for the teacher more marked and enduring.

When one reads accounts of Rutherford's early training and later work, it is possible to appreciate at once that Rutherford early recognised that his bent was teaching some subject in which demonstration must play a prominent part. Had he selected anatomy as his subject, his genius would probably have made him as successful as that succession of great men occupying the Chair dealing with that subject in the Edinburgh School. He might have been a great teacher of physics or of chemistry; but, fortunately for the thousands of students who have passed through his hands, his opportunity came in physiology, and he was not the man to neglect it. A Borderer of Roxburghshire, Rutherford came into the world in 1839, the youngest son of the large family of a sturdy farmer, blest with a stout frame and a big brain, but a somewhat nervous or shy and retiring disposition, which his school training appeared to have done little to dissipate. Rutherford came to Edinburgh determined to take any position that brains and hard work could obtain for him, whilst his taste for music and art, and his fondness of approval and desire for sympathy, made him attempt to fight down that shyness which hampered him throughout his life.

I am told by those who knew him at this period—and he formed one of a group of students who afterwards became very distinguished men—

that he was almost painfully anxious not to hurt the feelings of others, and very desirous to gain their approbation—this being accompanied by a morbid sensitiveness, which made him shrink from anything but the most friendly criticism; this extreme shrinking sometimes making him exceedingly brusque and, to those who did not know him, unapproachable. Those who knew him in late life can well understand that to his friends Rutherford was a genial, artistic, hard-working fellow, although to those who did not know his good qualities, and who had not got behind the outer crust, he was sometimes looked upon as somewhat of a rough diamond.

During his undergraduate career Rutherford worked hard, not only at his ordinary studies, but at his favourite hobbies, music and art; and after graduating with honours and a gold medal, he became House-Physician to Haldane and House-Surgeon to Spence. He then demonstrated in the Anatomical Rooms under Struthers.

In 1864–65 he studied under Du Bois Reymond in Berlin, and then visited a number of the other famous continental laboratories, especially that at Leipzig, where he fully appreciated the advantage of working under Ludwig. On his return to Edinburgh he succeeded Argyll Robertson as assistant to Professor Bennett, who at that time had had the wisdom and foresight to introduce histology into the routine work of the medical student. For four years Bennett and his assistant worked away, gradually extending the practical work of the class, until ultimately some physiological chemistry was introduced, the experimental work also being more and more illustrated. At the end of this period of excellent training and discipline, in 1869 Rutherford was appointed Professor of Physiology in King's College, London, where he introduced and developed the methods of teaching that he had carried out under Bennett in Edinburgh. Now devoting himself entirely to the teaching of physiology, he devised new experiments, by means of diagrams illustrated his lectures more fully, and gradually rendered the work of his class more practicable; and though in recent years the laboratory teaching has become perhaps more popular in London than is perhaps possible in Edinburgh, the earliest of this kind was introduced from Edinburgh by Rutherford. We are told that as Fullerian Professor at the Royal Institution, Rutherford did for physiology what Tyndall had done and was doing for physics. Those who knew Tyndall as a lecturer must hold that this is one of the highest compliments that could be paid.

In 1874, on the death of Professor Bennett, Rutherford, as was but natural, was recalled to Edinburgh to fill the Chair of his old master. His conception of the dignity of his position as a teacher of physiology was such, that he deemed it incompatible with the claims that it made on his time and energy that he should undertake clinical teaching, as his predecessor had done; and he was thus enabled to carry on the development of his Chair by introducing the experimental method more extensively, by devoting such time as he could spare from the preparation of his lectures to the keeping of the practical work up to date. All this his students and his colleagues appreciated to the full, and had Rutherford done nothing but organise the great teaching department in the new University he would have deserved well of his Alma Mater.

He was a perfect master of demonstration, to which his patience, his industry, and his wide knowledge of his subject all contributed largely; but he was something more. Some of the experimental work that he had done—that, for instance, on the inhibitory fibres of the vagus, on the secretion of bile, and upon certain points of histology, should always be associated with his name; whilst his assistants and pupils will be the first to acknowledge that his advice and criticism have started them on paths of research, and have guided them therein, often to most successful issues.

Professor Rutherford was essentially a student's friend. Always willing to encourage and to help anything that made for a corporate life in the University, in the way of work or amusement, he was recognised by the students as one of themselves, whilst his advice and help were never sought in vain by those who were in need of them; and one could well understand that, when the grave in Ancrum Churchyard received the mortal remains of William Rutherford, many students, colleagues, and old friends alike, felt that there had passed from amongst them one who, whatever faults of manner and of mind he may have had, especially during one sad period of his life, had under all a warm heart, a keen desire for the welfare of his students, and a determination to do what lay within his power to teach them physiology, to make them good physicians, and to help them, by encouraging them in their recreations and amusements, so to slacken the bow sometimes, that they might work better, more vigorously, and with greater keenness after these spells of rest. William Rutherford was a great teacher and a great influence in the University. Men as great, teachers as successful, and as great friends of the students may follow him, but none will quite fill the niche in the University that he occupied so long and so well.

G. Sims Woodhead.

ANALYTICAL REPORTS.

ANTIDIPHTHERITIC AND ANTISTREPTOCOCCIC SERUMS.

WE have received from Messrs. Parke, Davis, & Co., specimens of Antidiphtheritic and Antistreptococcic Serums. All the serum preparations of this firm are tested at the laboratories of the Royal Colleges of London, and the vials are said to contain 25 per cent. more antitoxic units than the label states, this precaution being taken to allow for the depreciation of strength by keeping. In hospitals where large amounts of serum are used in the course of a year, and where fresh supplies are constantly being obtained, this is of no particular advantage; but to druggists who do not sell much antitoxin, and who may have it some time on their hands, it is important that the vial sold should contain at least as large a dose as the label leads the purchaser to believe. It is therefore satisfactory to know that loss of strength by keeping is provided for.

The great advantage of this particular serum appears to us to be its very high concentration. As confidence in serum treatment has increased, there has been a tendency to give larger doses, and it has been found that, within limits, the larger the dose the better the patient does. With dilute serums it is a great nuisance having to fill a syringe twice or thrice to give a dose of say 4000 units. On the other hand, the price of the serum usually varies with its concentration, and we are obliged to either refill the syringe or to pay nearly double for the antitoxin. Messrs. Parke, Davis, & Co. charge the uniform price of sixpence per 100 units, irrespective of the concentration employed, and we consider this a great advance.

One of the specimens sent us contains a so-called prophylactic dose of 500 units. This is so concentrated that it could readily be given in an ordinary hypodermic syringe, which every practitioner has in his possession. While our experience of prophylactic doses is not great, it is obvious there are many circumstances in which they could be advantageously employed by the general practitioner.

The serum we have used has so far done very well. It is certainly a quite safe preparation, and is kept in hermetically sealed vials, from which it can be drawn straight into the syringe.

The same firm have placed on the market a most excellent syringe, of handy size, in a metal case. The whole is readily sterilised. Its distinctive features are an expanding piston, which is always in working order, and a special needle of large calibre with which to fill the syringe quickly and easily.

SPARKLETS.

(Aerators Limited, London.)

THE now widely-known apparatus for the domestic production of water and other liquids charged under pressure with carbonic acid gas immediately before use, which bears the name of Sparklets, has been submitted to us for an opinion. We have already had experience of the advantage afforded by this ingenious method of carbonating water, etc., doing away with the necessity of the conveyance of it in bulk and already made; one bottle and a supply of metal capsules, in shape like to the gelatin capsules of the chemist, each about the size capable of containing a drachm of fluid, but each charged with carbonic acid gas under high pressure, comprise the necessary paraphernalia. Insertion of a capsule with its elongated end downwards, and pressure exerted upon it by screwing down a cap, cause a sharp-pointed metal pin to penetrate through the material, forming the opening at this end of the metal case; but the discharge does not occur until the liberated carbonic acid gas has no outlet save into the water or other fluid poured beforehand into the interior of the bottle. In a few seconds the liquid is fully aerated. It will be found advisable to wait about ten minutes before making use of the liquid: if taken at once, the aeration is not so complete. The bottles supplied are made of strong syphon glass, covered with a cane network.

For invalids the invention should be of great service. The character of the liquid aerated is of no consequence—milk, lemon squash, wine, champagne, which has lost its sparkle in an opened bottle, medical draughts, home-made alkaline, and mineral waters, can severally be rendered more palatable, and to many more easily borne, by the aid of Sparklets.

TABLOID KRAMERIA AND COCAINE.

(Burroughs, Wellcome & Co.)

BURROUGHS, WELLCOME & Co. have not lost much time in providing, in a tabloid form, the Krameria and Cocaine Lozenge, introduced into the new pharmacopœia last year. The proportions are the same, Krameria extract 1, and hydrochloride of cocaine $\frac{1}{20}$ gr. The tabloids are prepared with a pleasant sweetish basis, and afford, in a very portable form, all the advantages possessed by the officinal trochiscus, the astringent and stimulating effects of rhatany root, and the soothing power of cocaine, along with its action in removing congestion of the palate and fauces.

MONTHLY NOTES
ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,

MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

METEOROLOGICAL MEANS FOR THE EAST OF SCOTLAND DISTRICT, FOR THE FOUR WEEKS ENDING FEBRUARY 25, AND FOR THE YEAR FROM JANUARY 1.

	TEMPERATURE.			RAINFALL IN INCHES.		SUNSHINE. HOURS.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees. Above 42°.	Accumulated Day-Degrees. Below 42°.	Actual.	Accumulated from Jan. 1.	Actual.	Accumulated from Jan. 1.	
Mean .	37°·4	*^{1} 36°	171°	2·4	5·0	58	90	2°·9
		^{2} 64°	349°					
Observed	37°·9	^{1} 38°	153°	2·1	5·8	66	107	2°·9
		^{2} 42°	338°					
Difference	+0°·5	^{1} +2°	−18°	−0·3	+0·8	+8	+17	±0
		^{2} −22°	−11°					

VITAL STATISTICS.

MORTALITY IN FOUR LARGE TOWNS IN THE EAST OF SCOTLAND, DURING FEBRUARY, AND SINCE JANUARY 1. Actual Numbers and Rate per 10,000 per annum.

	TOTAL.		CONSTITUTIONAL DISEASES.		CIRCULATORY SYSTEM.		NERVOUS SYSTEM.		RESPIRATORY SYSTEM.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	^{1}1,173	205·3	209	36·7	100	17·4	125	21·9	276	48·5
	^{2}2,431	212·7	416	36·4	217	19·0	262	22·9	574	50·2
Actual Deaths and Rate per 10,000	^{1}1,271	222·4	229	40·0	98	17·1	125	21·9	303	53·0
	^{2}2,579	225·7	464	40·6	238	20·8	251	21·9	571	49·9
Difference from Mean	^{1} +98	+17·1	+20	+3·3	−2	−0·3	±0	±0	+27	+4·5
	^{2}+148	+13	+48	+4·2	+21	+1·8	−11	−1·0	−3	−0·3

	DIGESTIVE SYSTEM.		URINARY SYSTEM.		PNEUMONIA.		PHTHISIS.		CANCER.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	^{1} 65	11·4	30	5·2	92	16·1	108	19·0	48	8·4
	^{2} 136	11·9	56	4·9	189	16·4	211	18·4	95	8·3
Actual Deaths and Rate per 10,000	^{1} 64	11·2	26	4·5	122	21·3	119	20·8	43	7·5
	^{2} 155	13·5	54	4·7	244	21·3	237	20·7	110	9·6
Difference from Mean	^{1} −1	−0·2	−4	−0·7	+30	+5·2	+11	+1·8	−5	−0·9
	^{2} +19	+1·6	−2	−0·2	+55	+4·9	+26	+2·3	+15	+1·3

* NOTE.—The figures ^{1} denotes the data for February, ^{2} for year to date.

The mean temperature of February 1899 over the East of Scotland was rather above the average, but only to an amount represented by 2 day-degrees over 42°, and 11° below that point, while the rainfall was slightly below, the mean daily variability at, and the hours of bright sunshine considerably over, the normal figures. The weather type during the month was distinguished by the presence of anticyclonic systems of but moderate intensity at the beginning and at the close, with a marked absence of west and south-west winds, north-west to south-east predominating. The wind-pressure was very low, only reaching to two-thirds of the normal. From the beginning of the year there is little to note save some deficit in both the warm and cold days experienced, and an excess of sunshine, amounting to nearly 19 per cent.

The number of deaths registered was ninety-eight over the figure expected, or as 8·3 per cent. to it. The largest excess over the average for February occurred from respiratory diseases, the excess of deaths from pneumonia (32 per cent. over that looked for), however, more than accounting for this increase. Influenza caused forty-two deaths, or 7·1 per 10,000 per annum in the four larger towns of the East of Scotland.

For the first two months of the year the points worthy of notice are, the increase in mortality, total, and from constitutional, circulatory, and digestive diseases, from pneumonia, phthisis, and cancer.

Last year, in February, with a mean temperature only 0·8° above this year, but with more sun, the total mortality was 35·9 per 10,000 below the average, and the deaths from respiratory diseases 19·6 per 10,000 less than usual. The number of fatal cases of influenza reached seventeen last year against forty-two in 1899; while there was a marked excess of deaths from digestive diseases.

EDINBURGH MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

CHRONIC SPLENIC ENLARGEMENT WITH RECURRING GASTRO-INTESTINAL HÆMORRHAGES.

By William Osler, M.D., LL.D., F.R.S., *Professor of Medicine, Johns Hopkins University, Baltimore.*

Excluding the enlarged spleen of leukæmia, chronic malaria, cirrhosis of the liver, heart disease, and rickets, the cases of so-called primitive enlargement of the organ fall into two groups.

First, a series in which the spleen is enlarged without causing any symptoms, other than those due to mechanical pressure. In the past few years I have seen four patients, all women, apparently in perfect health, who complained only of a feeling of pressure in the abdomen, in all of whom the spleen was much enlarged. In two cases in which the organ was freely movable and caused a great deal of discomfort, my colleague Halsted opened the abdomen and successfully packed the spleen in position with gauze, an operation much less serious than splenectomy and very efficacious. Both these patients have been seen more than two years subsequent to the operation, and have remained quite well. In a third case, a girl was sent into the gynæcological department, supposed to have an ovarian tumour. She was robust and strong, with good colour, and had been hard at work. She subsequently had a twist of the ligaments and sphacelus of the spleen, with enormous enlargement, adhesion to the abdominal wall, redness, and inflammation. The organ was freely incised by Halsted, and an enormous quantity of necrotic spleen tissue removed; the patient made a good recovery. This condition is, I think, more common than we suspect. The spleen is, as a rule, only moderately enlarged. In some cases there has been a history of past malaria, but in a majority the condition is one of, so far as we can tell, primary enlargement. It must not be forgotten that only a blood examination can determine whether or not such patients have leukæmia, since in this disease, as is well known, an individual may look well, and have indeed nearly a normal number of red blood corpuscles.

Secondly, cases of enlargement of the spleen with anæmia, Griesinger's anæmia splenica, the splenic pseudo-leukæmia of H. C. Wood, or, as it is sometimes termed, the splenic variety of Hodgkin's disease. I prefer the name splenic anæmia, introduced by Griesinger. The term should be restricted to the class of cases in which a progressive anæmia develops in connection with a primitive splenomegaly. The relation of the enlarged spleen to the anæmia is still in doubt, whether the perverted state of the blood is due to splenic inadequacy (which does not seem at all probable, since removal of the spleen has cured some cases), or whether both the enlarged spleen and the anæmia are due to some chronic toxæmia. In my experience the cases have not been uncommon. I have seen four cases during the present year, one with Dr. Graham of Toronto, two reported in this paper, and a fourth, which is at present under observation.

Among peculiarities which have been illustrated by my cases, I may mention the following:—The remarkably chronic course, extending from three to twelve years; the chlorotic features of the blood, the hæmoglobin value, often not more than 50 per cent.; the peculiar bronzing of the skin, not a jaundice, which was not present in any of my cases; and, lastly, hæmorrhages, which may be toxic, as in leukæmia, and widespread, or mechanical, resulting directly from the condition of the enlarged spleen.

It is to this latter point I wish to call particular attention in this paper, to a condition in which for many years hæmorrhages occur from the stomach and bowels. The bleedings are profuse, and in the cases here recorded have occurred during a period of from nine to twelve years, while in the intervals the patients have regained their flesh and strength and have been able to carry on their occupations.

Hæmorrhage in chronic enlargement of the spleen has long been recognised. Latour,[1] who seems to have clearly recognised the association, quotes two interesting observations from the older writers; in one (Obs. 741), Franck's case, a woman who had for several years a tumour in the abdomen, died from hæmatemesis; the spleen weighed 14 lb. In the other (Obs. 742), a case from Morgagni, a young woman, æt. 20, had gradually grown pale for two years, and had pains in the chest and abdomen. A tumour developed in the left hypochondrium. She had several attacks of vomiting of blood. The spleen was larger than the liver, and weighed 4½ lb. There were polypoid concretions in the splenic vein. The best section on the subject in any modern work is given in Watson's "Practice," in which he gives the following explanation:—"It seems to me highly probable that one at least of the offices of the spleen is to provide a receptacle or reservoir for this blood, when its free passage through the portal vessels is temporarily obstructed. It then becomes a sort of safety valve (if such an illustration be allowable), which obviates the danger that might otherwise arise to more vital parts from any great or sudden disturbance of the venous circulation. The stress of the congestion is continually felt in the submucous capillary system; and the hæmorrhage which is apt in such cases to occur from the loaded membrane receives a simple solution, upon principles almost purely mechanical."

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[1] "Hist. phil. et med. des hémorrhages," 1828, tome ii. p. 318.

There are to be found in the literature many isolated cases. In this Journal for January 1869, George W. Balfour and Thomas Grainger Stewart report a very unusual case. A young man, æt. 20, had an enlarged abdomen for five years. In May 1868 he had vomiting of blood, and on admission in June he had ascites. There was no leukæmia. After death the spleen weighed 3 lb. 6 oz., and the liver 1 lb. 15 oz. There were aneurysmal dilatations of the splenic vein and thrombi in the portal vein.

Grissolle[1] reports an interesting case in which the spleen weighed 4 kilos., and Douglas Stanley[2] reports a fatal case in splenic anæmia.

As showing the occurrence in the tropics, I may mention the case reported by Croskery[3] from Jamaica.

In the large treatises, enlargement of the spleen as a cause of gastro-intestinal hæmorrhage is either not discussed at all, or only in a very cursory way. No mention of it is made in the very full section of diseases of the spleen by Lockwood in Loomis and Thompson's "System," or by Stockton and Jones in the same "System," on hæmorrhages from the stomach. Except in connection with leukæmia, it is only referred to incidentally in Allbutt's "System"; there is no reference to it in the monograph on the spleen by Litten in Nothnagel's "Handbuch." Stengel does not mention it in the section on the spleen in the "Twentieth Century Practice," and among recent writers on the diseases of the stomach, Ewald, Martin, Hemmeter, and Einhorn make no mention of the association.

When in Montreal my attention was called to the subject by the occurrence of several interesting cases, and in 1882 I published a short paper[4] and reported four cases. Of these, two occurred in leukæmia, and were of special interest, inasmuch as the hæmorrhage was the symptom to call attention to the condition; in one case, indeed, the diagnosis of leukæmia was only made post mortem. Of the other two cases one was an instance of ordinary splenic anæmia, in which, in association with a greatly enlarged spleen, the patient had in January and July 1879 severe hæmorrhages from the stomach, and later in the same year died in a third attack. The fourth case possibly comes in the group to which I particularly wish to call attention in this paper. The patient, a child æt. 11, had a greatly enlarged spleen without leukæmia. There had been swelling of the abdomen for two or three years, and at her ninth year, two years before I saw her, she had a severe attack of hæmorrhage from the stomach. There was an indefinite history of a previous attack, four years before her visit to me. A month before I saw her she had a very severe attack, in which she vomited in a couple of days nearly three quarts of blood. In the intervals she had thriven well, though she always looked a little pale. The red blood corpuscles were 2,250,000, white corpuscles 6,696 per cm.

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[1] *Gaz. d. hôp.*, Paris, 1850, p. 257. [2] *Brit. Med. Journ.*, London, 1895, vol. ii.

[3] *Dublin Hosp. Gaz.*, 1858, p. 39.

[4] "Hæmatemesis in Chronic Splenic Tumour," *Canada Med. and Surg. Journ.*, Montreal, December 1882.

In 1890 a case of hæmorrhage from the stomach, in asssociation with splenic anæmia, was admitted to the Johns Hopkins Hospital. A young man æt. 20 had lived in a malarial district, but had never had chills and fever. In April of 1889, after feeling badly for a month or so, he vomited blood, and afterwards passed dark blood in the stools. In May, his physician, for the first time, noticed an enlargement of the spleen. In June he applied at the out-patient department of the hospital, when he had an extreme degree of anæmia and a greatly enlarged spleen. On admission, 14th January 1890, he had a great deal of pigmentation of the skin, and the spleen was very large, reaching nearly to the navel. The red blood corpuscles were slightly over 2,000,000 per c.mm., and the leucocytes a little over 12,000 per c.mm. He gained rapidly in weight, had no further hæmorrhage, was discharged on 24th March, and has not since been heard from.

The three cases here reported are characterised, as I stated, by a remarkably protracted course of from nine to twelve years, during which time hæmorrhages, at times of great severity, took place from the stomach and bowels.

Case 1. — *Clinical Summary. — Recurring attacks of hæmatemesis and melæna between* 1885 *and* 1897*; excellent health in the intervals; chronic enlargement of the spleen; death in an attack. Anatomical Summary.—Chronic hyperplasia of the spleen; liver smooth, macroscopically showing no signs of cirrhosis, microscopically showing only slight fatty changes.*—W. M., æt. 35, referred to me by Dr. Brewer of New York, 9th December 1892, complaining of hæmorrhage from the bowels.

The patient had always been a healthy man, of a strongly nervous temperament. He had had no special illnesses. His habits had been good; and he denied having been a heavy drinker; had usually taken light wines, not whisky. He was a composer, and had worked hard at his profession. Ten years ago, at Orange, N. J., he had an attack of malaria. Eight or nine years ago he had a well-defined attack of syphilis, for which he was treated thoroughly during two years. He had had no signs since. About a year and a half after, while at dinner, he felt nauseated, faint, and dizzy. He lay down for a couple of hours, and in a short time passed, per rectum, a quantity of dark material which was said to be blood. About three years afterwards he had an attack of vomiting, and brought up dark material with blood and clots like liver. He was in bed for a week, and was very pale and anæmic. Dr. Janeway saw him at this time, and thought he possibly had ulcer of the stomach. He gradually recovered, regained his colour, and, with the exception of slight dyspepsia for a time, he seemed to get quite well. He did not lose in weight, had a good appetite. Three weeks ago he noticed that there was dark material with the stools. This reappeared several times, and last week, on two or three evenings in succession, after large injections, he passed smaller quantities. At one evacuation about a week ago he is sure there was clotted blood in the stools. Since that time they have been normal. Neither in this nor in the preceding attack have there been any sharp pains or colicky attacks, just a little sense of uneasiness and distress. There has been no straining, no pain in the bowels.

Present condition.—Medium-sized man, black hair and eyes, a little pale, but not anæmic; tongue clean; pulse regular; no fever. Well nourished; superficial veins slightly bluish, not distended; panniculus well preserved. Abdomen full, natural-looking, a little more prominent in left than in right hypochondrium. Skin clear, no rash. Respiratory movements of abdomen normal. In left hypochondrium, just below ribs, a prominent mass projects exactly six cms. from costal margin in nipple line. The edge is rounded, not specially sensitive on pressure. Towards the ensiform cartilage it is not at all well defined; when a sharp breath is drawn, it comes down almost to the level of the navel. It comes from beneath the ribs, and feels like an enlarged spleen. The notch, however, is not palpable, nor is there the clearly defined margin towards the middle line so usual in splenic enlargement. The upper limit of splenic dulness is at the seventh rib, and extends to the costal margin in the mid-axillary line. The edge of liver is just palpable on deep inspiration beneath costal margin; upper limit of dulness in nipple line at seventh rib. It does not extend to the costal border. There are exactly four cms. of vertical liver flatness in nipple line. It is obliterated in mid-sternal line. In mid-axillary line it is greatly reduced, not more than two fingers'-breadth. When patient turns on the left side, percussion in the mid-axilla gives scarcely more than a finger's-breadth of liver dulness. On inflation of the stomach there is no marked distension.

The blood examination was practically negative: there was no leucocytosis; unfortunately the blood count was mislaid. The blood was frequently examined after he had recovered completely from the attacks, and showed no special alterations. The urine was clear, acid, no albumin; the pigment ring with nitric acid was intense.

On 12th February 1893, Dr. Brewer wrote as follows:—"Since you saw him, Mr. M. has had three severe hæmorrhages. Between his attacks he gains very rapidly, becomes strong and well, has an excellent appetite, perfect digestion, and looks the picture of vigorous health. The hæmorrhages nearly always follow some physical exertion. It seems incredible to me that a man can have cirrhosis enough to give him such violent hæmorrhages and yet have such perfect digestion and appear so absolutely well at other times." Dr. Brewer noted that immediately after a hæmorrhage the spleen was greatly reduced in size, while two weeks later it reached five inches below the ribs. This observation, he states, was confirmed by Dr. Janeway and Dr. Walter James.

Subsequent to this date, I heard twice from Dr. Brewer about the case. On one occasion he wrote: "Mr. M. had a very alarming hæmorrhage last May. It followed a generous meal of plain, digestible food, and there was no evidence of gastric or intestinal disturbance. He vomited a very large quantity of freshly clotted blood, and for many days passed black stools. On the following day, he had another hæmorrhage, in which I thought he would die, for his pulse could not be counted. He became extremely pale, and was only revived by very energetic hypodermic stimulation. There was, however, no return of the hæmorrhage, and he slowly gained strength. It was not until three weeks had passed that he was able to go out again. I had not seen him for seven months until the day I received your letter, when I was summoned to see his wife. He had been in perfect health since May, had not even passed a black stool, had gained from 12 to 15 lb., and had had an excellent digestion. The spleen is still enlarged. I cannot find the liver abnormal."

Dr. Brewer writes, that from 1892 until 1897, the date of his death, this patient had five or six severe, almost fatal hæmorrhages, and a number of other short periods in which he passed black stools. "After each severe attack he would recover completely, regain his weight and colour, and for many months would enjoy an excellent appetite and perfect digestion."

The autopsy was performed by Dr. Blanchard, and no lesion whatever was found in the œsophagus, stomach, or intestine, the mucous membrane of which was very carefully looked over on three separate occasions. The spleen was very large and hard. The liver was somewhat smaller than normal, was slightly fatty, but not cirrhotic. Portions of the organs were very kindly sent to me for examination.

In this case, which had attracted a great deal of attention, the diagnosis could not be positively made. I was inclined to lay a great deal of stress upon the diminution in the liver volume, though I did not think the condition an alcoholic cirrhosis, but rather some anomalous form associated with his acquired syphilis. Though I had had the experience spoken of in connection with hæmorrhage in enlarged spleen, I did not think this likely to be the cause of the hæmorrhage without the anæmia, which goes almost invariably with enlargement of the spleen, either idiopathic or in malaria. The third possibility of duodenal or gastric ulcer I did not entertain.

I put away the notes of Mr. M.'s case, and though I have seen many instances of enlarged spleen since 1892, in none were there associated hæmorrhages. During the present year two cases have been under my care with almost identical symptoms to those presented by Mr. M., namely, hæmorrhages from the stomach and bowels at intervals for a period of many years, in association with enlarged spleen.

Case 2.—*Clinical Summary.—No history of malaria or of syphilis; nearly ten years ago first attack of hæmatemesis; since then, at intervals of about a year, very severe attacks, in which he vomited blood and passed blood in the stools; enlarged spleen; exploratory laparotomy; stomach and duodenum normal; liver smooth, not cirrhotic; removal of enlarged spleen; recovery.*—C. D. B., æt. 33, Fincastle, Virginia, farmer; admitted first on 9th March 1898, complaining of hæmorrhages from the stomach and bowels, and pain with an enlargement in the left side of the abdomen.

Family history.—His father is living and well, æt. 75; his mother died in child-birth, æt. 42. He has four brothers and three sisters living and well.

Personal history.—With the exception of the illness to be described, he has been a very healthy man; had, as a child, whooping-cough, chicken-pox, and measles. He has not had syphilis. He drinks in moderation; neither smokes nor chews tobacco.

Fifteen years ago he began to have dyspepsia, and for two years he had severe indigestion, frequently vomiting his food. He went to Nebraska, and lived there for several years in comparative comfort, occasionally, however, taking patent medicines. He seems on the whole, of late years, with the exception of the attacks to be described, to have had a very fair digestion, and not to have been seriously troubled with his stomach.

Nine years ago he had the first hæmorrhage from the stomach, and vomited, he thinks, at least four quarts of blood. He was very much prostrated, and on his recovery he went to California. After fifteen months he had a second attack, in which he brought up a very large quantity. Since then he has had attacks at intervals of from twelve to fifteen months to the present time, the last occurring on 12th December 1897. All of them have been severe, and he has brought up varying quantities, from a couple of quarts to a gallon and a half of blood. He has also passed blood in hard, dark "chunks" in the stools. The last attack covered a period of four days. It came on when he was feeling very well and without any obvious cause. He thinks he brought up at the most moderate computation at least a gallon and a half of blood. He has never passed blood in the stools except after hæmorrhage from the stomach.

Four years ago he noticed a pain in the left side under the ribs, which gradually grew worse. Three years ago he first noticed a lump in this region. This has grown larger, but he does not think it has increased much within the past eighteen months. After the attacks he is exceedingly pale, and takes months to recover, but he says that in the intervals he has felt well and is able to do his work satisfactorily. He has no special pain in the abdomen, has never had severe gastralgic attacks, has never vomited food which he has taken two or three days previously, and never of large quantities. He always has to be a little careful in eating.

His condition on the first admission was as follows:—He was a medium-sized man, with dark hair. He was dark in colour, and said he thought he had become somewhat darker during the past few years. No discoloration of the mucous membranes. He looked anæmic, and the sclerotics were pearly. He was well nourished, and the fatty panniculus was preserved. The lymphatic glands were everywhere palpable, but not enlarged. Examination of the heart and lungs showed nothing of any moment except the presence at the apex beat of a soft systolic murmur, which was transmitted to the mid-axilla.

The abdomen looked natural. The superficial veins were not dilated. Beneath the left costal margin the surface was rendered prominent by an ovoid mass (just opposite the tip of the tenth rib), which descended with each inspiration. On palpation the abdomen was a little tense and rigid. The tumour mass seen beneath the costal margin was rounded, descended deeply on inspiration, felt firm and hard. On its right border there was a slight depression. Anteriorly, it extended beyond the prolongation of the parasternal line. It was not painful on pressure; no friction was heard over it on deep inspiration; no bruit. The upper limit of splenic flatness was at the lower border of the seventh rib. On bimanual palpation the mass could be freely moved, and corresponded in all respects with an enlarged spleen. The liver was not enlarged; the edge could be felt just outside the border of the right rectus; did not seem firm or specially rounded. The liver flatness was somewhat high, in the fifth interspace, and extended to the costal margin.

Stomach.—On inflation the epigastrium became more prominent, particularly to the right of the middle line; marked peristalsis could be seen. The lower border of the stomach tympany did not reach the navel. Stomach contents removed after a test breakfast gave the following: strongly acid to litmus, reacts to Gunzburg, 8 c.c. of decinormal sodium hydrate required to neutralise acid of 10 c.c. of filtered stomach fluid; no lactic acid present.

Blood examination.—Hæmoglobin, 25 per cent.; red blood corpuscles, 3,072,000 per c.mm.; leucocytes, 2800 per cm. The urine was normal. The patient remained in hospital only a week, improved somewhat, and took his food well. As we were uncertain as to the nature of the trouble, he was advised to return in October for an exploratory operation. When he left the hospital the hæmoglobin was 47 per cent., red blood corpuscles 3,150,000 per cm., and the leucocytes 3100 per c.c.

On 26th October the patient returned. He had had no attack of hæmorrhage, and his general condition had improved. He had gained in weight and strength. When he left the hospital on 18th March he weighed 136 lb., on his return 155 lb. The appetite had been very good, and he had had no attacks of gastralgia. His colour looked very much better, though the general hue of the skin still had a somewhat sallow appearance. Examination of the abdomen showed the same condition of enlargement of the spleen as made in the previous note. There was no enlargement of the liver, the edge of which was palpable just below the costal margin.

On the 28th the test breakfast was given, and showed no changes in the secretion. The blood count showed red blood corpuscles 4,754,000 per c.c., hæmoglobin 57 per cent., leucocytes 6428 per c.c. Of these there were polynuclear 84·4 per cent., small mononuclear 4·4, large mononuclear 5, transitional 3·4, eosinophiles 2·8. Three normoblasts were seen in counting 500 leucocytes. An occasional irregularity in the corpuscles was seen, and a slight variation in the shape of the red corpuscles. The patient looked very much better, had a good appetite, had gained in weight, and as he was anxious to have something done, an exploratory laparotomy was performed by Dr. Cushing, 9th November. The peritoneum was everywhere perfectly smooth; the liver was smooth, natural-looking, and showed no signs of cirrhosis. A very careful inspection was made of the stomach; there was no cicatricial puckering of the peritoneal surface, nor could Dr. Cushing anywhere feel signs of thickening or induration about the pylorus or other parts. The duodenum and pancreas felt perfectly normal. Under these circumstances, and as it seemed highly probable that the hæmorrhages were due directly to the enlarged spleen, Dr. Cushing removed it. There were a good many adhesions at the posterior border. The capsule of the spleen was a little thickened. Sections showed a marked degree of fibrous hyperplasia.

The patient recovered well from the operation, and the subsequent history to date is as follows:—He has done remarkably well. The red blood corpuscles have remained about 4,000,000, with a relatively low hæmoglobin. There has been an increase in the leucocytes since the operation. This point has been made a special study by Dr. Cushing, who will report upon it subsequently.

Case 3.—*Clinical Summary.—No malaria; no syphilis; eleven years ago, first attack of hæmatemesis; for four or five years recurring attacks of melæna; in* 1892 *second attack of hæmatemesis; an occasional attack of melæna; January* 1898, *severe hæmatemesis and melæna; great enlargement of the spleen; marked anæmia of the chlorotic type.*—A. B., æt. 38, Durham, N. C., admitted to Ward C on 10th November 1898, complaining of hæmorrhages from the stomach.

Family history.—Father died at the age of 56, of heart disease; mother at the age of 40, of apoplexy. His brothers and sisters are healthy and strong.

Personal history (taken by Dr. T. M'Crae).—He had the usual diseases of childhood. He has never had malarial fever, typhoid fever, or rheumatic fever. He has never lived in a malarial region. No history of lues. He uses a good deal of tobacco; has been a very moderate drinker. Ever since boyhood he has not had a strong digestion, and has often uneasy feelings after eating, with belching of gas. He has never had nausea or vomiting. His bowels have been rather constipated. Of late he has taken a purgative about once a week. He has never had hæmorrhoids. He has been married ten years, and has two healthy children. His wife is healthy; she has had no miscarriages. His occupation, as manager of a tobacco factory, has kept him indoors a great deal; he has not had very much exercise. His average weight has been about 150 lb.

Present illness.—Eleven years ago (he remembers the date accurately, as it was the year before he was married) he had the first attack of hæmorrhage from the stomach. Prior to this date he was free from any stomach troubles, and never had had any gastric pain or distress. He was walking along the street early one evening, and whistling at the time, when he suddenly felt his throat fill up, and, coming to a lamp-post, he found his mouth full of blood. He went into a chemist's shop a block or two away, and a doctor there gave him some salt and water, and the blood stopped after he had brought up four or five mouthfuls. He slept well that night, but in the morning when he sat up in bed he began to bring up dark masses, looking like pieces of liver, and after it some fresh blood. The amount altogether he does not think was more than a pint. He stayed in bed for two days, and felt weak for ten days. There was neither nausea, gastric pain, nor coughing with this attack. He did not notice any change in the stools, but at this time he did not know the importance of watching them.

For the next four or five years he did not have any vomiting of blood, but every three or four months he would pass, for two or three days, black, tarry stools without pain or discomfort of any kind. At first he did not know their significance, but he soon began to notice that after each attack he felt weak and out of sorts.

In December 1892 he had a second attack of vomiting of blood. He had gone home in the afternoon, and felt somewhat weak, particularly after having gone to the water-closet. He was put to bed by his wife. He became very pale, collapsed, and nauseated. He then vomited nearly pure blood, which was dark in colour and contained clots. During the night he vomited at intervals a large amount, and he was so prostrated that on the following day he could not lift himself or move in bed. For several days after this, the stools were large, dark, like tar, and very offensive. Following this attack he was nauseated for at least a week, but without any pain in the stomach. He was laid up for six weeks, and it was quite six months before his ordinary strength returned. He was very pale, and, as his friends used to say, "looked like a dead man." It was during this time that his physicians, on examining the abdomen, found that the spleen was enlarged. He cannot say whether it was enlarged before this. He says that it has remained in about the same condition and of about the same size ever since. At times there were slight pains in the region of the spleen.

For the next two years and a half he was very careful about his diet, and was quite free from any gastric symptoms. He regained his strength, but never recovered a perfectly good colour. Gradually he widened his range of diet, and at the end of 1895 he was eating all sorts of ordinary food. From this time to January 1898 he was fairly well. He thinks he may have at times passed tarry stools, but they never were accompanied by any special prostration or paleness, and he had not to give up work.

In January 1898, one night, on going to bed, he suddenly felt very weak, and knew at once there was a hæmorrhage. In a few minutes nausea came on, and he vomited, bringing up almost pure dark blood, as much, he thinks, as a pint and a half. After this he had no more bleeding, but for a day or two the stools were tarry. He was very pale and weak after this attack, and it was nearly a month before he returned to business.

In March he noticed again the tarry stools and the same weak sensation, which passed off after keeping quiet for a few days. From this time until Saturday, 29th October, he was very well. On that morning he got up, took a good breakfast, and went to work. Within an hour he became nauseated, and kept very still, and sent for his physician, who, after feeling his pulse, said that he would have more tarry stools. He had a feeling of soreness and fulness in the stomach, but no pain, and he did not vomit. He went home to bed, and for two or three days passed dark-coloured stools. He returned to work in a few days, and has kept at work until he came to the hospital.

Apart from these attacks his health has been very good. He has never had jaundice, but has constantly had a sallow, muddy colour, which always deepens after an attack.

On asking him more particularly about certain features, he states as follows :—He has never had any severe pain. There has at times been some soreness in the region of the spleen, nothing more. This has been particularly after an attack, and he would often have to caution his servant to be careful, as he brushed him before going out, to do so very lightly over the abdomen. His weight and strength have kept about the same, except at the time of the attacks. His appetite has always been good, and he has taken a general diet; in the intervals between the attacks his digestion has been very good. He has been constipated at times, but as a rule his bowels have been regular. Eighteen years ago he had once an attack of cramp colic, accompanied by purging and vomiting, due to something he had eaten.

Condition on admission.—The patient was a fairly well-nourished man (weight, 135 lb.). The complexion was sallow and muddy, not yellow or icteroid. Some patchy pigmentation over the forehead. The tongue was pale and flabby; the gums and mucous membranes were pale.

Thorax.—Large and well developed; pulmonary resonance everywhere normal.

Heart.—The point of maximum impulse was in the fifth interspace; the area of superficial flatness was diminished; the sounds were clear; the second accentuated in the pulmonary area. The pulse was 84, soft and easily compressed.

Abdomen.—On the left side below the costal margin there was slight fulness. On palpation, occupying the left side, there was a smooth, painless mass which projected from beneath the costal border. To the right it did not extend quite to the middle line; in the anterior axillary line it reached almost as low as the crest of the ilium. At its right border there was an edge with a well-marked notch. It descended freely with inspiration. On bimanual palpation it was freely movable. The area of splenic dulness was much increased, beginning in the mid-axillary line at the eighth interspace.

Liver.—The edge was readily felt at the margin of the right rectus, and seemed normal. The area of dulness was reduced. There was no dilatation of the stomach. There was no glandular enlargement.

Blood.—Hæmoglobin, 30 per cent.; red corpuscles, 4,018,000 per cm.; leucocytes, 6500 per cm. Differential count of 400 corpuscles —polynuclear, 73; small mononuclears, 10; large mononuclears, 12; transitionals, 3; eosinophiles, 2. There was no special poikilocytosis.

There were no retinal hæmorrhages. The urine was of low specific gravity, 1008 to 1010; it contained no albumin, no sugar, and no casts. The knee-jerks were present.

The patient remained in the hospital a week, during which time he had no fever; the appetite was good; the general condition was improved. He was urged to take iron and arsenic for two or three months, and then to return for an exploratory operation.

REMARKS.—The special feature of hæmorrhage I look upon as entirely due to mechanical causes. In none of them have there been associated cutaneous or retinal hæmorrhages, such as we very often see in leukæmia. I do not think anything can be added to Watson's explanation, which I have already given. In support of this, I may refer to the anatomical fact that a very large portion of the blood from the stomach is discharged into the splenic vein. Based on the measurements of the areas of all the main branches of the splenic artery, it has been estimated that of its blood 60 per cent. passes to the spleen, and 40 per cent. to the stomach (Mall, Krause). The veins of the vasa brevia passing from the fundus of the stomach are very large; and though I can find no estimate of the relation which they bear to the other veins of the organ, yet they certainly must drain a very large section of the organ.

The diagnosis of conditions associated with enlargement of the spleen is important, but somewhat complicated, owing to the number of lacunæ in our knowledge of the etiology of the various forms, and of their relation to one another. As I said before, we must recognise a primitive splenomegaly with a practically normal blood-count. The cases of chronic enlargement of the spleen, extending over many years, present, as a rule, an anæmia of a chlorotic type, with low hæmoglobin and low leucocyte count. The more pronounced does the corpuscular anæmia become, the more striking are the changes in the red blood corpuscles, and in advanced cases the blood may be like that of a pernicious anæmia. The most confusing and puzzling condition, however, is that in which, with enlargement of the spleen, the condition of leukæmia may be presented during one month, and in the following that of a simple splenic anæmia. A patient at present under observation, had in May and July an extreme degree of leukæmia, while at the end of September, with a moderately enlarged spleen, he had a simple anæmia with a slight increase in the polynuclear leucocytes.[1]

And, lastly, comes the question of the diagnosis from Banti's disease, primitive splenomegaly, with an associated terminal cirrhosis of the liver and jaundice. I have never met with an instance; but two of the cases I here report illustrate how careful one must be in the diagnosis *intra vitam*. All three cases here reported have the chronic character and the severe hæmatemesis, features upon which Banti lays a good deal of stress. There were no changes whatever in the liver in Case 1 microscopically, and macroscopically in Case 2 the liver looked perfectly normal. It is to be noted also in the clinical report on both these cases, that the area of liver dulness seemed considerably reduced, and in Case 1, I was rather inclined to regard the hæmorrhage as of hepatic origin.

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[1] Case clxxxv. in Bennett's "Clinical Lectures," 3rd edition, p. 870, was of this nature.

Then, moreover, a certain number of cases of splenic anæmia present ascites quite early in their course, and an instance of this kind has been recently under my observation in a physician from Illinois, who had an enlarged spleen for at least four years, with ascites in May 1895. Death occurred in the early part of this year, with ascites. The autopsy showed only an enormously enlarged spleen without cirrhosis of the liver.

TUBERCULOSIS AND ITS PREVENTION.[1]

By G. Sims Woodhead, M.D., F.R.C.P.Ed., F.R.S.E., *Professor of Pathology, University of Cambridge.*

(Plate II.)

One of the most encouraging aspects of the fight which large communities have constantly to wage in the cause of health is, that at last there is a general desire on the part of those most closely affected to gain some knowledge of the natural history of the diseases to which they are most prone, and to attack intelligently and radically the question, not, How are these diseases to be cured ? but, How are these diseases to be prevented. It is because of the existence of this desire that I am here, at the invitation of the West Riding County Council, conveyed through Dr. Kaye, their Medical Officer of Health, to say something about tuberculosis, a disease which, since the proof of its infective nature was first obtained, and, in a more marked degree since its bacterial origin was fully demonstrated, has been occupying the attention of men who, working steadily and quietly, have gradually brought our knowledge of the question to such a stage that the discussion of "Tuberculosis" has become one of the burning questions of the day; and there is, at last, a very general appreciation of the necessity for taking energetic steps to rout a scourge which has been making havoc, not only amongst our children, but amongst our young men and maidens, and even amongst our cattle. Let us see exactly what this tuberculosis is.

Tuberculosis in man.—Tuberculosis is, as we have to deal with it to-day, most frequently met with as a disease of the lungs, in which the sponge-like tissue of these organs becomes solidified in patches, these patches as a rule occurring near the upper part of the lung, especially just under the collar-bone. After a time these solid patches (Plate II. Fig. 1) become softened, they break down, and the broken-down substance may find its way into the air tubes, through which it is coughed up. If we examine this coughed-up matter under the microscope, we find that it is made up of fragments of dead lung tissue; and by means of special methods of staining with aniline dyes, very minute bacteria (Plate II. Figs. 2 and 3), tubercle bacilli, are found lying in this dead material. We shall have more to say about these bacilli immediately. Should the patient succumb to the disease and the lungs be examined, it will be found that the solidified patches in the lung are cheesy-looking, *i.e.*, they are composed of a material which in colour and substance resembles a soft granular cheese, and in those cases where this cheesy material has been coughed up we find that what are called cavities remain;—large holes in the lung lined with this cheesy-looking material. At the root of the lung, there may be what are known as kernels or enlarged glands, very similar to those found in the necks of weakly children. In these again is the same cheesy stuff, in which also tubercle bacilli may be demonstrated. In children, similar tubercular nodules are often met with in the glands or kernels round the intestines, and in other parts of the abdomen, whilst they also suffer from a kind of inflammation of the coverings of the brain. In both of these conditions the same peculiar bacilli are found. These tubercle bacilli have the power of setting up a definite kind of inflammation, which is often followed by a softening and a breaking down of the tissues, both new and old. Tubercles may be found in the kernels or glands of the neck, in the skin; in fact, in almost any part of the human body to which the bacilli can by any channel or means gain access. It is an interesting fact, however, that in the human subject the lungs and the intestines are the parts that are specially affected, and it must be remembered that it is just these parts that are continually, or from time to time, in close relation with the outside world; the lungs through the air and the particles that are carried with it, and the intestines through the food that passes into them.

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[1] A Lecture delivered at the Yorkshire College, Leeds, 10th March 1899.

Tuberculosis has gradually come to be recognised as an infective disease, and as being set up by some substance which is carried directly or indirectly from a patient already suffering from tuberculosis to an individual who, up to the time of the introduction of this matter, is free from the disease.

Tuberculosis in cattle, etc.—It is a well-known fact that, in the case of most diseases, human subjects only or other animals only are affected, but we find that tuberculosis, in addition to attacking man, affects a large proportion of those animals which are kept in captivity or in a domesticated condition, *i.e.*, where they live in houses and are constantly coming into contact with the human species. For example, monkeys brought into captivity often suffer from tuberculosis. Cats and horses also contract the disease. And the list might be extended very considerably; but these examples, along with the following, are quite sufficient for our purpose.

The milch cow, perhaps, more frequently than any other animal, becomes the victim of tuberculosis; and it is somewhat interesting to find that here, as in the human subject, the lungs are the organs most frequently affected. (In the horse (Plate II. Fig. 2) and the pig (Plate II. Fig. 3), the food tract and the spleen are the parts usually involved.) Here there may be found in the lungs large yellowish, cheesy-looking masses, surrounded by a greyish, firm jelly-like tissue. In the neighbouring glands or kernels the same yellowish material may be seen, whilst in both the lungs and the kernels there may be hard, gritty particles or nodules, made up of lime salts. On the covering of the lungs there are frequently found what are technically known as grapes—little soft fleshy masses, sometimes like round buttons hanging by a thread, sometimes little flat circular projections—often much redder, though sometimes paler than the surrounding lung tissue. Here, too, the covering of the lungs and the lining of the chest, in place of being glistening, are dull, thickened, and irregular. In cattle, however, as in pigs and horses, the abdominal organs may also be affected. In such cases grapes are seen on the intestines, on the covering of the liver, and in various other positions (Plate II. Fig. 4). The glands or kernels may be enlarged, sometimes enormously so, and usually contain distinct cheesy, yellow masses.

Tuberculous affection of the udder.—Similar cheesy masses may be met with in the kidney, in the milt or spleen, and in some cases—and this is a very important and striking fact—the udder (Plate II. Fig. 5) is affected. Of this form of tuberculosis, *i.e.* tuberculosis of the udder, it may, for several reasons, be well to speak somewhat in detail, especially as in certain respects it varies somewhat from the more or less typical form above described. For instance, it usually occurs in small masses, in which there is more of the grey, jelly-like tissue, and less of the yellow, cheesy material already described, which in some cases may be entirely absent. On examining a cow in which tuberculosis of the udder has developed, it will generally be found that one of the quarters is larger and harder than the others. This hardness is very characteristic, and is described as being almost stone-like. In spite of this enlargement of the quarter, there is no tenderness or any indication of pain or inflammation. On examining the kernels, it will be found that the one above the affected quarter is both larger and harder than are the others. The other quarters and their kernels may of course, in time, be gradually affected; but it is seldom, even in very advanced cases, that the whole four quarters become involved, although I have seen cases in which this has occurred.

We have here, then, a disease which manifests itself in much the same way in different animals and in man, a disease which under certain conditions runs a very definite course, attacking first perhaps a single small point, but gradually making its way throughout the tissues and organs of the body, sometimes begining in the lungs, sometimes in the intestines, and then attacking in turn surrounding or distant parts. It is an infective disease, that is, it can be passed on from one individual to another, and it is a spreading disease when once it has obtained a foothold in the tissues of a patient.

The tubercle bacillus the real cause of tuberculosis.—At this time of day it is scarcely necessary to tell you that Koch's tubercle bacillus is claimed to be the direct causal agent, the fountain and source of the disease. Let us see very briefly what this bacillus is, and then what evidence we have that it is the prime factor in the production of the disease, although it may require to be assisted by a series of secondary factors in order that it may exert its full effect.

I may perhaps best bring home to you the nature of the action of the tubercle bacillus, by giving you a concrete example, and one that I have observed frequently. On taking the milk from a tuberculous udder, and allowing drops of it to run over a flat, clean glass plate, small solid opaque specks may be found sticking to the plate. These, on microscopic examination, are found to be minute scraps of udder tissue, in which the presence of tubercle bacilli may usually be demonstrated. If milk containing such specks be given to a pig, it will be found at the end of from five to eight weeks that swellings have developed under the jaw and in the throat of the animal fed; he becomes lean and unthrifty, and all attempts to fatten him fail most dismally. When the animal is killed, the tonsils (Plate II. Fig. 6) may be enlarged, ulcerated, and tuberculous, or, if this be not the case, the glands or kernels immediately round them contain the cheesy points that have been noted in other tuberculous parts; then, running down from the glands in the neck, grape-like masses may be traced on the covering of the lung, although at this stage the lung itself may be very little affected. In the spleen of such a pig, three or four or more yellowish masses, from the size of a pea to that of a walnut—the larger masses always being soft in the centre—may be present; the glands around the intestines may be enlarged, and there may be tuberculous ulcers in the intestine itself. Even the backbone in the neck, or the bones in one of the limbs, may be affected with tubercle (Plate II. Fig. 3), though not usually at such an early stage as that above described. These experiments have been done so repeatedly on pigs and other animals, that we must accept the occurrence of the disease as evidence that milk from a tuberculous udder contains something which has the power of setting up tubercular processes. What is this something? and where does it come from?

The tubercle bacillus.—We have already seen that in all the tubercle we have spoken of, tubercle bacilli may, by the use of appropriate methods, be demonstrated.

These tubercle bacilli are active, living organisms, because, as demonstrated by Koch, if a few of them are taken on the point of a needle from the tissues in which they lie, and with the needle used as a spud, are planted out on blood, or rather solidified blood serum —the clear yellow fluid that separates when blood is allowed to clot —it will be found that at the end of about a fortnight, if the serum with the planted-out bacilli be maintained at the temperature of the human body, small dry scales, which gradually increase in size until they form fairly large masses, make their appearance. When particles of these scales are stained and examined under the microscope, they are found to consist of enormous numbers of bacilli which have all the characters of the bacilli that were found originally in the tubercle nodules. These bacilli have, in fact, grown from the few that were originally planted out from the tuberculous tissues. If a healthy pig be fed with milk which has been boiled, you will find that he does not develop any tuberculous mischief; but if, after feeding such an animal for some time with this pure or boiled milk, you add to it some of the growth of tubercle bacilli, and nothing else, you will find that there will be developed exactly the same kind of tuberculous processes that were set up in the pig fed with the milk from the tuberculous udder; thus showing that the one and only thing required to convert a milk that will not give tuberculosis into one that will, is the addition of tubercle bacilli. If animals may be infected in this way, must we not conclude that the human subject may also be similarly infected ? and if the tubercle bacillus alone is the cause of tuberculosis in animals, is it not probable that the tubercle bacilli in the udder of a tuberculous cow are the cause of the tuberculosis that affects the child who consumes the milk from that udder ?

I have already spoken of tubercle bacilli as actively growing organisms. Like all living organisms, whether plant or animal, they cannot resist the action of great heat—the temperature of boiling water is fatal to both plant and animal life—and therefore the tubercle bacillus succumbs to boiling heat. Similarly, like plants and animals, it succumbs to the action of certain substances called antiseptics, many or most of which exert a definite coagulating and devitalising action upon the protoplasm of which they are composed. This susceptibility to the action of heat is of course a fact to be carefully borne in mind wherever diseases produced by micro-organisms have to be dealt with, and it is especially important in connection with tuberculosis, which, as we have seen, may be spread through the milk in which they are contained.

Milk, as we know, is the one article of diet which under ordinary conditions is not submitted to the process of cooking. We drink milk warm from the cow, or again after it has cooled down, but we seldom think of drinking boiled milk, or cooled boiled milk, although in certain forms of food, in which milk plays a part, the process of cooking is necessarily carried out. It is because so much raw milk is drunk that danger lurks in the tuberculous udder, and it is for this reason that dairy cattle should be kept under the most rigorous observation.

Is tuberculosis hereditary?—Is tuberculosis contagious, or hereditary, or both? is a question that is sometimes asked. On this occasion it would perhaps be better to use the term "infective," as indicating the nature of the process better than the term "contagious." From what we have already said, I think we need scarcely re-discuss the first question. I would simply state that I am now thoroughly convinced that there can be no tubercle without the presence of the tubercle bacillus, and that in every case the disease is transmitted only through the agency of this bacillus. This belief in the minds of most men forms the basis on which sanitary reform must in future be carried out, as far as tuberculosis is concerned. Such being the case, I am firmly of the opinion that heredity, using the term in the sense in which it is ordinarily used, can account for very few, in fact, for practically none, of the cases of tuberculosis that occur either in the human subject or in the lower animals. Please do not misunderstand me. There *are* cases of congenital tuberculosis, especially in the lower animals, *i.e.* where the animal is born into the world suffering from the disease; but this is only where the dam is suffering from general tuberculosis, or where the cotyledons of the placenta are affected by a tuberculous process. How small a proportion of calves suffer from congenital tuberculosis, is evident from the fact that out of 185,765 examined in the Copenhagen slaughter-house (statistics of which I was able to obtain), only 369, or less than one-fifth per cent., showed any trace of tuberculosis; although amongst 132,294 cattle examined at the same period, 17½ out of every 100 showed some evidence or other of the presence of tubercle. So that, allowing that every one of the calves inherited tubercle (which was by no means the case), an enormously greater proportion of cattle must have acquired the disease after birth. The evidence that even this small proportion of calves did not inherit the disease may be summed up under two headings—(1) Amongst such a large number of calves a certain number of them must have received tuberculous milk from some of the 17½ per cent. cattle affected; (2) then, again, it was found that very young animals, *i.e.* those only a week or two old, seldom suffered from tuberculosis; that is, the disease was not born with them; and I again assert that, had the tubercle bacillus been kept away from the 369 calves, very few of them would have succumbed to tuberculosis. So thoroughly convinced of this was Professor Bang of Copenhagen, that, having obtained a sum of money from the Danish Government, he made arrangements to breed healthy cattle from tuberculous stock, by removing the calves as soon as they were born, and feeding them with milk in which he knew there were no living tubercle bacilli. He has thus been able, without any importation of stock from outside, to gradually bring down the number of tuberculous cattle, and to bring up the number of non-tuberculous stock to such a point that he anticipates a time when he will have entirely eradicated tuberculosis from his herd. This, of course, is a valuable experiment, and one that may be repeated by breeders; and any pedigree stock, however tuberculous, may gradually be bred into a healthy herd. Bang's experiments certainly afford conclusive evidence that tuberculosis is not hereditary.

Speaking on this subject at Birmingham this year, I tried to bring it home that this statement means "that no member of a tuberculous family need despair of evading the dread enemy of his race; that children of tuberculous parents may be placed under such conditions that they will never contract the disease. They do not inherit tuberculosis from their parents, though they may undoubtedly inherit those weaknesses of constitution and tissues which render them peculiarly liable to harbour and nourish the parasites that in their parents have done such damage. All that is necessary to do to enable the child to withstand the attacks of tuberculosis, is to strengthen the tissues, build up the constitution by good food, fresh air, and exercise; and while you are doing this, to place him under such conditions that the tubercle bacillus can gain no access to these tissues, and by and by there will come a time at which the attacks of the bacillus are as futile as they are against the child who inherits from his parents all those protective agents with which healthy children are endowed, for the warding off of the onslaught of the tubercle bacillus and other disease-producing micro-organisms."

All this, of course, constitutes a most important addition to our knowledge of the disease, an addition which enables us to forecast that, in process of time, we shall be able to stamp out tuberculosis in cattle, if we can only put the progeny of these cattle under conditions favourable to the building up of sound healthy bodies, and unfavourable to the multiplication, development, or persistence of the tubercle bacillus; whilst we may safely argue that what is true in the case of cattle also holds good for the human subject, so that we may look forward to a time when tuberculosis and phthisis will also be no more amongst us.

The transmission of tuberculosis through meat.—We will now for a short time leave the question of the disease itself, and turn our attention to the mode of transmission of tuberculosis from man to man, and from animals to man. We might take these modes of transmission in the reverse order of their importance. In the first place, it must be recognised that in this country, at any rate, the transmission of tuberculosis through the eating of the meat of tuberculous cattle occurs in a comparatively small number of cases. Indeed, if this were not the case, it is difficult to imagine to what extent tuberculosis might not advance amongst us. It has been calculated, the calculation being based on a considerable number of observations, that something like 49 per cent. of the dairy cattle in this country react to the tuberculin test, and allowing that we have 5 per cent. of error, this would still leave over 40 per cent. of tuberculous cattle in our dairies. Of course, in many of these cases the disease is comparatively slight, and may be fairly localised, in which case it is easily removed when the animal is dressed, and may constitute but a a slight danger to the consumer; but there still remain cases in which the disease is so far advanced, and is so extensive, that should any part of the animal be eaten uncooked or under-cooked, there is a certain risk of the person consuming this meat contracting tuberculosis.

The proportion of the whole of the tuberculous cattle in which this occurs may be small, but however small the percentage may be, when taken as an absolute number, it is bound to be considerable; and until we have much more careful inspection than is in most places at present possible, and until the recommendations of the Royal Commission are embodied in an Act of Parliament, and such an Act is put into force, the only advice that one can give to individual members of the community is—"See that your meat is well cooked."

The transmission of tuberculosis through milk.—The milk question, as will readily be gathered from what I have already said concerning the action of milk from tuberculous udders, presents a much more serious aspect. It may be granted that unless the udder itself is affected, the milk from a tuberculous animal will not contain tubercle bacilli, and therefore will be incapable of setting up tuberculosis. It may also be granted that in only 1, 2, or 3 per cent.—the number varying according to different observers—of tubercular cattle, is the udder affected. It must still be borne in mind that tubercular lesions of the udder, though probably indiscernible in their very early stages, make very rapid progress, and, unless a constant outlook is kept, the disease may be well advanced, and bacilli coming in enormous numbers into the milk, before it is recognised at all; whilst, as I once saw, in some preparations shown by the late Principal Walley at the Edinburgh Medico-Chirurgical Society, tubercle bacilli may be present in the milk, and lesions which can be easily demonstrated on cutting into the udder, may occur before it is possible, by any of the ordinary means at our disposal, to make out any tubercular growth in the udder whilst the animal was still alive. How important this question is, Dr. Niven and Professor Delépine at Manchester, Dr. Hope and Professor Boyce in Liverpool, and many of the medical officers and veterinary surgeons in this district, have from time to time demonstrated.

In a series of fifty milks sent from Liverpool by my friend Mr. Shelmerdine, Dr. Cartwright Wood and I were able to demonstrate the presence of the tubercle bacillus in five samples, or in 10 per cent. of the whole; whilst Professor Kanthack and Dr. Sladen, examining a series of milks from the Cambridge supplies, found tubercle bacilli in something like 25 per cent. of the specimens examined.

It may be objected, How can it be proved that these tubercle bacilli were capable of setting up tuberculosis? The answer to that objection is, that in some cases the presence of the bacilli was recognised only from the fact that the milk was capable, when injected under the skin of a guinea-pig, of setting up a distinct tubercular process, and that, in the tubercle nodules formed, numerous tubercle bacilli could be demonstrated. There can be no doubt, then, that the milk from tuberculous udders is a a far too common cause of tuberculosis, especially in young children, who are so subject to slight disturbances of the intestinal tract.

The tuberculin test.—It is not my intention, nor would you wish me, to go into the whole question of the diagnosis of tuberculosis in cattle, but the tuberculin test has been brought so prominently forward, and has been so fully relied upon by those who have had most experience of its use, that it may be well to say something of this test, which still remains as the result of Koch's great work on the treatment of tuberculosis, in which failure was at first sight supposed to be so signal. In the earlier experiments, where the exact conditions necessary for the success of this method had not been determined, it was to be expected that failures would occur in the hands even of those who were most competent to obtain good results; but we may now accept it as proved that the tuberculin test, when carried out properly, enables us to diagnose tuberculosis in a very large percentage of cases which, before the advent of tuberculin, must necessarily have escaped observation. The best veterinary authorities, both in this country and abroad, after a very extensive experience of the use of tuberculin, are thoroughly satisfied that the reaction obtained by its use affords as accurate a means of diagnosis as we are likely ever to obtain whilst the animal is living, a very small percentage of cases remaining undiagnosed when tuberculin is intelligently used.

Certain breeders of cattle, and I am glad to say many dairymen — in Scotland and the North of England especially — voluntarily use the tuberculin test, and I look forward to a time when associations will be formed, members of which will undertake to supply, or to use, only such milk as is guaranteed to be taken from animals that have been subjected to the tuberculin test. In order that you may understand what this tuberculin test is, it may be well to indicate that tuberculin consists of the products of the tubercle bacillus, formed when this organism grows for a long period in meat broth to which a small proportion, say 5 or 6 per cent., of glycerine has been added. You know that these bacilli secrete or excrete certain substances which have the power, when put into our bodies, of causing a rise of temperature, and also of causing a certain amount of local swelling. In order that only these products may be left to act, the tubercle bacilli are first killed by being subjected to the temperature at which water boils, and are then removed by a process of filtration through Berkefield or Pasteur-Chamberland filters. If a considerable quantity of this filtered fluid be injected under the skin of a healthy man or brute, it is possible to produce some local swelling, and to cause a rise of temperature; but in a patient, human or brute, suffering from tuberculosis, a very much smaller dose similarly injected—a dose which does not affect the healthy patient in the slightest degree—is sufficient to cause a characteristic rise of temperature. It is by exact dosage that we are able to determine whether the patient is suffering from tuberculosis or not, and larger or smaller doses give quite unreliable results. It is absolutely necessary that certain important points should be attended to in making this test.

Driven animals should never be tested. Cattle being tested should be allowed to rest in the house for some time, and during the whole time that they are under observation they should be fed as usual and carefully protected from draughts. The dose marked on the bottle is usually that for a medium-sized animal. The injection is made into the subcutaneous connective tissue, preferably in front of the shoulder or on the chest wall, with the usual precautions as to cleanliness. The temperature should then be taken at the sixth hour, and for every three hours afterwards up to the fifteenth, and if, during this time, the temperature rises to 104° F., or higher, there can be little doubt that the animal is tuberculous; but if it remains under 103° F., we have an indication that the animal is free from the disease. Where the tuberculin reaction is obtained, nothing but the most rigorous search at the post mortem examination should be allowed to stand in the way of a positive diagnosis, as the reaction appears to be obtained when the most minute naked-eye lesions are present. If the temperature remains between these points, the case is a doubtful one, and, according to Principal M'Fadyen, should be retested at the end of a month. This same authority points out that the tuberculin test is not reliable in the case of animals in the last stage of the disease, especially in those in which the temperature is 103° to begin with (in such cases the disease may of course be diagnosed without the aid of tuberculin). He also assures us that the tuberculin test does not render the milk in any way dangerous.

It has been suggested that the Board of Agriculture should supply tuberculin free to anyone who wishes to use it as a test; the advantage of such a course being, that the Board of Agriculture, working along with the Royal Veterinary College, would be able to guarantee the tuberculin sent out, and to give definite instructions to the veterinary surgeons entrusted with the task of making the diagnosis. If, at the same time, the Board would insist upon a return of the results obtained, they would soon accumulate statistical data that would be of enormous value in dealing with this question. I am so thoroughly convinced that a thorough testing of animals would lead to a great saving to dairymen and cattle-feeders, that I think it ought not to be necessary to call for legislative action in this matter, but that dairy cattle should be registered and systematically and periodically inspected (this in the interests of milk consumers) I am fully convinced. Let us for a moment consider what an enormous waste the attempt to feed up tuberculous cattle involves. Without being actual "piners" or "wasters," such cattle as a rule may be said to be unthrifty. If you have a fat beast that gives evidence of being tuberculous, the best plan is to kill it at once, in order that it may not go off in condition; whilst if a thin animal is tuberculous, it is in most cases a mere waste of good food to attempt to get it into good condition, and practically amounts to throwing good money after bad. It is for this reason that self-interest on the part of the farmers should make it absolutely unnecessary for Government to step in, except in so far as it might give powers to local sanitary authorities to assist stock-breeders and farmers in the application of this test, and, by supplying good veterinary inspectors, insure its efficient carrying out. In connection with this part of the subject, I should like to draw the attention of those in authority to the necessity that exists for a thorough regulation and inspection of dairy-farms, dairies, and milk-shops. Not an isolated or perfunctory inspection, but a regular inspection of registered farms, etc., by which it would be possible to trace any milk to its source, and to insist on a record being kept of all registered cattle, milk from none of which should be allowed to be sent from the farm unless the cattle have a clean bill of health. It is sometimes objected that it is impossible to do all this. If, however, we bear in mind that tubercle bacilli in milk are bound to be taken by some susceptible patients, and those only too frequently children, we shall surely insist upon taking every precaution to protect ourselves and our children against a perfectly preventible danger, even at some little trouble and expense. It is upon this fact, the fact that the danger is perfectly preventible, that I wish so strongly to insist. Elsewhere I have laid down the lines on which much valuable work might be carried out in preventing the spread of tuberculosis through the milk.

To bring this about, however, it will be necessary for all interested to enter into hearty co-operation. The community as a whole, and even that part of it which is interested in the raising of stock, would then, in the long run, gain enormously by the eradication of tuberculosis.

The question of compensation.—At the same time the fact must be faced that in individual cases there might be temporary loss, and although such cases would probably be very few indeed, it might appear to be advisable, if for no other reason than to impart confidence, that some fund should be raised from which compensation should be given in cases where any hardship might be involved in the carrying out of the necessary precautions, some of which I take the opportunity of placing before you in the shape of a few suggestions.

Suggested recommendations and precautions.—1. The tuberculin test might be applied in every case where cattle are changing hands. The use of tuberculin would practically be equal to a warranty, for which most buyers would be willing to pay at least half the fee of the veterinary surgeon who might apply the test. It might be objected that at fairs such a warranty would be of little value, but surely this might be got over by means of brands on the cattle, and the enforcement of penalties for false statements.

2. The application of the tuberculin test to all dairy stock. This to be carried out from time to time as found necessary by a veterinary surgeon, who should be retained, say at a contract fee, to inspect stock at short intervals of a month, and all animals suspected of being unsound every fortnight. This is of special importance in regard to udder tuberculosis, as it is often very difficult to tell whether tuberculosis is advancing or not until it gets into the udder, where, as we have seen, it may develop with such extraordinary rapidity that, although indistinguishable at one visit of the veterinary surgeon, it may be well developed on a second visit a fortnight later. This tuberculosis of the udder, however, is sometimes (without the use of tuberculin) the first distinctly recognisable indication of tuberculosis in the animal. In my report to the Royal Commission on Tuberculosis, I stated that—"Should the intervals of inspection be greater than a fortnight, the value of this inspection would be enormously diminished as far as protection of the consumers of milk against what must now be recognised as a very grave danger goes. Beyond this, however, it has been pointed out that the tubercular lesion of the udder may occur at any period of the disease, not only when it has been localised to the glands or to special cavities, but also when the disease has become much more generalised."

I also suggested that the veterinary inspectors should have instructions to recommend if no compensation be given, but to insist when compensation is offered—

"(*a*) That all tubercular animals, whether affected with udder disease or not, should be eliminated from a dairy supply, removed from the farm, and slaughtered at as early a date as possible, it being pointed out to the farmer that every day that the animal lives diminishes the value of the carcase, the milk being no longer of the slightest value.

DESCRIPTION OF PLATE II.

FIG. 1.—Small tuberculous nodule growing in a perivascular lymphatic sheath of the lung of a guinea pig infected with tuberculosis material. Here one of the kinds of the consolidation of patches of lung tissue may be seen. Magnified 250 diameters.

FIG. 2.—Tubercle bacilli from the lung of a horse suffering from tuberculosis. In some cases the groups of bacilli arranged within the boundaries of a cell outline are distinctly seen. Magnified 800 diameters.

FIG. 3.—Mass of tubercle bacilli from tuberculous focus in bone of a pig. Magnified 800 diameters.

FIG. 4.—Tubercle nodules in the wall of the heart of a tuberculous cow. From a lantern slide kindly lent by Principal M'Fadyean, of the Royal Veterinary College, London.

FIG. 5.—From a photograph of the tuberculous udder of a cow. The small nodules are well seen over the whole of the cut surface.

FIG. 6.—Tuberculous tonsils of a pig fed with milk from a cow suffering from tuberculosis of the udder.

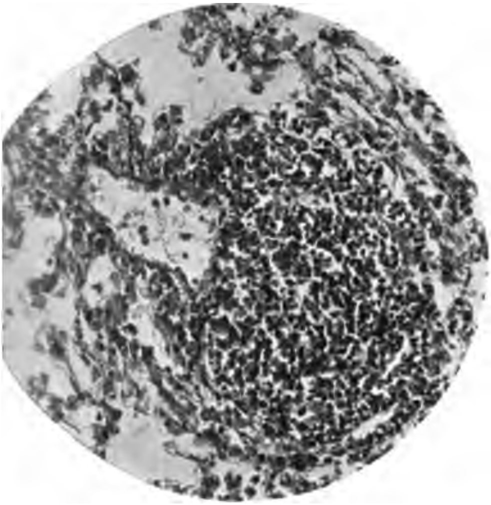

FIG. 1.

FIG. 2.

FIG. 3.

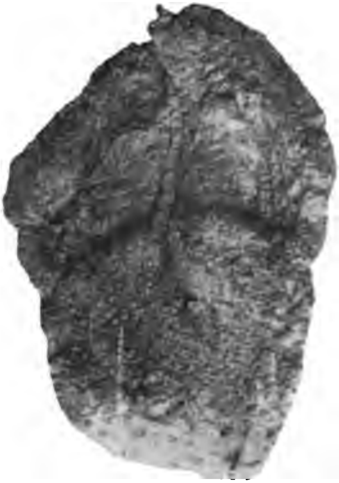

FIG. 5.

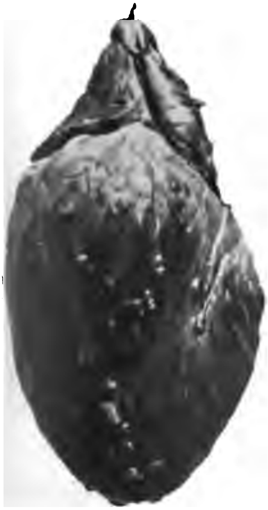

FIG. 4.

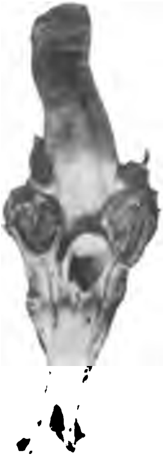

FIG. 6.

"(*b*) That every animal suffering from any disease of the udder should be carefully isolated and milked after all the other cows have been milked, or by a separate milker; this milk should from time to time be carefully examined both chemically and microscopically. If used at all, it should be carefully boiled, and only after such treatment should it be given to pigs or other animals, and even the risk of doing this should be carefully pointed out to the farmer. Only after a certificate has been given by the veterinary surgeon in consultation with the medical officer of health, should the milk from any cow in which udder disease is suspected be allowed to pass into circulation.

"(*c*) The registration of all dairy premises and of every animal bought for or sold from these premises should be carefully made out and kept by the veterinary inspector, and notes on the condition of every suspected animal should be entered up at each visit. Some system of notification, combined with one of compensation, would add enormously to the value of this system of inspection, and would greatly facilitate its working."

Recommendations of the Third Royal Commission on Tuberculosis.—The Royal Commissioners on Tuberculosis, after hearing all the evidence and considering the facts that had been brought before them, believe that tuberculosis is a disease which may be rendered exceptional instead of as at present widely prevalent; and further, that by disseminating sound information as to the methods of detection and the measures to be taken for isolation, and by distributing tuberculin gratuitously, or at a low cost, tuberculosis may be considerably reduced, and possibly to an extent which would prevent any serious loss to stock-breeders, or any important interference with the cattle trade. Amongst the suggestions made by them, perhaps the most important are those dealing with public abattoirs and inspection offices, the stamping of all meat offered for sale, so that it may be insisted upon that none but meat killed in licensed slaughter-houses shall be offered for sale, whilst their instructions to be carried out pending the issue of regulations by the Local Government Board should be carefully studied by those interested in the provision of meat free from tuberculous contamination.

For purposes of reference, it may be well to give these suggestions in full:—

"The entire carcase and all the organs may be seized—

"(*a*) When there is miliary tuberculosis of both lungs.

"(*b*) When tuberculous lesions are present on the pleura and peritoneum.

"(*c*) When tuberculous lesions are present in the muscular system, or in the lymphatic glands embedded in or between the muscles.

"(*d*) When tuberculous lesions exist in any part of an emaciated carcase.

"The carcase, if otherwise healthy, shall not be condemned, but every part of it containing tuberculous lesions shall be seized—

"(*a*) When the lesions are confined to the lungs and the thoracic lymphatic glands.

"(*b*) When the lesions are confined to the liver.

"(*c*) When the lesions are confined to the pharyngeal lymphatic glands.

"(*d*) When the lesions are confined to any combination of the foregoing, but are collectively small in extent."

In view of the greater tendency to generalisation of tuberculosis in the pig, we consider that the presence of tubercular deposit in any degree should involve seizure of the whole carcase and of the organs.

In respect of foreign dead meat, seizure shall ensue in every case where the pleura have been "stripped."

The prevention of tubercular phthisis.—In a most thoughtful address on the prevention of phthisis, with special reference to its notification to the medical officer of health, Dr. Newsholme of Brighton recently brought together a series of arguments to enforce the theory that, although phthisis or tuberculosis is infective to a minority of the population only, it is infective over a very long period, whilst those of us who have made a large number of post mortem examinations know that in people who die from other diseases, scars, the result of a tubercular process, may be found in the lungs—scars that have healed and, unless we know the history of such cases, would scarcely be recognised as remnants or evidence of the presence of tuberculosis. Dr. Newsholme further points out, one-fourth of the total deaths from all causes between the ages of 15 to 55 are due to pulmonary phthisis, so that, when other forms of tubercular disease are taken into consideration, an even larger proportion, it is certain, must succumb to tuberculosis. It is evident if the disease can be transferred from patient to patient, even though some people are immune to the disease, that with such an enormous proportion of the population suffering from the disease we have a number of centres from which it may spread, each tuberculous patient, like each tuberculous cow, constituting a distinct source of danger to those by whom he or she may be surrounded. It is now known, and has recently been well put by Dr. Arthur Ransome, that the work of the medical officers of health, and the improved conditions of life amongst our population, have been exerting an enormous influence on the death rate from tuberculosis in England and Wales, and it is evident that the same steady decline is still going on, so that we have hopes that in time tuberculosis may become almost an unknown disease amongst us. What I wish to insist upon most strenuously, however, is, that with a thousand deaths per million (taking that as a fair average at the present time) any steps that we can take to accelerate the disappearance of phthisis from our midst, or rather to prevent its dissemination from patients and animals already affected, must exert a very definite and rapid influence on the death rate. It is sometimes indicated by those who do not understand the question, that the tubercle bacillus may be especially dangerous because it has been able to continue its existence and even to multiply outside the human body; because it is not necessary for it to pass into an animal or a patient in order that it may be maintained in a virulent condition; and that, therefore, with such a large number of patients suffering from the disease the virus must practically be present everywhere, and we must all of us be inhaling or taking in the tubercle bacillus in some form or other. From the point of view of the medical officer of health, these considerations have been pushed to a dangerous extreme, because they are apt to lead people to think that it is impossible to get beyond the range of action of the tubercle bacillus, with the result that a kind of Eastern fatalism or *laissez faire* has been induced. The tubercle bacillus may, under certain conditions, remain and grow in wall paper and even in certain soils, though this as yet has not been fully proved; but we may take it for granted that outside the animal body the conditions of nutrition and of general temperature are so unfavourable, and these unfavourable conditions are so helped by sunlight, by dessication, by oxidation, and the numerous other influences that interfere with the growth of micro-organisms (especially those that have a distinct parasitic existence), that if we could cut off the supply coming from patients, both human and brute, the danger from infection would soon be reduced practically to the vanishing point.

The value of notification.—In regard to the prevention of phthisis spreading from human patients, I most frankly confess that I am greatly in favour of compulsory notification; and I am in favour of this because I think it is one of the means of impressing upon the public, and even in some cases upon ourselves, the fact that there is danger of the spread of infection from tuberculous patients, unless proper precautions be taken by the patient and his friends to destroy all excreta and secreta that may contain tubercle bacilli.

The very fact that we consider it necessary to speak of tuberculosis as a notifiable infective disease, should render our task of disinfecting a patient and his surroundings a much easier one. It has been objected that tuberculous patient sare often dangerous long before the disease is recognised, and that up to this point, whether we have notification or not, they are doing irreparable damage. Can that really for a moment be looked upon as a sound argument against taking all necessary precautions when once the disease has been recognised? A patient may go on expectorating tubercle bacilli for months, or even years, after the disease has been diagnosed. It is surely worth our while to have such a case notified, in order that full directions for the behaviour of a patient and for the disinfection of his sputum and surroundings through the agency of a skilled staff in every case, and at as early a date as possible, may be given.

The infectivity of tuberculosis sputum.—Patients who are allowed to expectorate in public places, and in their own rooms, in positions where the sputum may become dried and be carried about by every current of air, are disseminating seeds of disease which may find good soil in which to germinate in those who have any susceptibility to tuberculosis; whilst, as often pointed out, such patients are actually a further danger to themselves, unless they are taught to get rid of all expectorations, and allow none to pass into the stomach, and so to the intestines.

Further, if notification led to nothing more than the attraction of the attention of the medical officer of health and his inspectors to the houses in which phthisis is most prevalent, it would, from the sanitary point of view, be well worth making it compulsory. A phthisical patient properly looked after is a danger to no one but himself; but a phthisical patient who knows nothing of the precautions that should be taken for the prevention of the spread of disease is a continuous source of very real danger to every non-immune person with whom he comes in contact; whilst even this immunity is a less persistent factor when all sanitary precautions are not taken to maintain a high standard of health in the people who live in the houses in which tuberculous patients reside, than when these are carefully carried out. It is now necessary above all things to teach that every particle in our body is susceptible to external influences in a much more marked degree than we are accustomed to think. The food, the exercise, the amount of sleep, the drainage and ventilation of our houses, all act on the tissues of the body, improving their resisting powers when the conditions are good, increasing their susceptibility to disease when the conditions are bad, and preparing the patient to resist or to receive disease, as the case may be.

The specific nature of the causal agent of the disease.—Taking all this into account, it must be remembered that no specific infective disease, such as tuberculosis, typhoid, and the like, can be introduced into a community or set up in an individual by anything but the distinct and specific bacilli that we now know are associated with these different diseases. You may prepare the soil, but unless the seed is sown there is no crop; while, on the other hand, you may sow the seed, but unless the soil is well prepared, you will get little or no fruit from the seed. Once let the community understand this, and it will become an easy matter for county councils, district councils, and similar authorities, to obtain powers through the exercise of which medical officers of health and their veterinary colleagues and inspectors will be able to wage a war against tuberculosis which ultimately must result in its extinction. Even at present, under the greatly improved conditions of recent years, tuberculous diseases, according to Dr. Newsholme, causes 50 per cent. more deaths than the aggregate of the seven general zymotic diseases that are now notifiable; whilst, more important still, it is a far more preventible disease than some of them. This being the case, I think it may be well to sum up very briefly the remedies that I hold are necessary for the gradual extermination of tuberculosis. I shall not speak of the treatment of tuberculosis, as that is a question beyond the scope of this lecture; but I should like to lay down the following points as regards its prevention:—

Summary of suggested preventive measures.—In the first place, take every precaution to prevent the dissemination of tuberculous virus, either from the human subject or from animals. In the human subject the sputum is the most important means of spreading the tubercle bacillus. So long as it is moist it is easily dealt with, and if it can be collected and destroyed or sterilised at once, it can never do any harm; but if it be allowed to dry, become pulverised and mix with dust, it at once becomes a medium of infection, which may result in the spread of the disease to other patients. It is obvious that no tuberculous patient should ever think of kissing children, or, in fact, of kissing anyone; and a tuberculous patient should always, except in hospital, where of course all precautions may be taken, sleep in a room by himself. Every house in which tuberculous patients have lived, or are living, should be thoroughly ventilated, and the windows should be kept open as much as possible, it being the duty of the medical officers of health to see that through currents of air are maintained, and that there should be sufficient good ventilation to prevent the rising of ground air, and also to prevent the breathing of air that has previously been used. Dust in a house where a consumptive patient resides is to be avoided by all possible means, and the instructions given as to the cleaning down of walls and of floors with moist cloths should apply, not only when a house is systematically disinfected, but at all times. Consumptives should live in fresh air, both in the house and outside. Sunlight and open windows are the best treatment and the best preventive.

Notification of the disease amongst catttle, especially dairy cattle, is equally important, and I look forward to the time when local authorities will appoint veterinary surgeons, who, working along with the medical officers of health, will keep a register of all cattle in their district, and will compulsorily test with tuberculin all dairy cattle, and all other stock at the request of cattle-breeders. Compulsory registration of milk shops, and regular inspection by medical officers of health, or their inspectors, must come in time, but in the meantime I would advise all members of the community to assure themselves, by some means or other, that the milk they obtain is not derived from any dairy in which the cattle are tuberculous. Every one of you here would refuse to drink milk that had been contaminated in any way by water, even suspected of containing the typhoid bacillus, and none of you would care to give to your children milk from a house in which there is a case of scarlet fever. The effect of the presence of the tubercle bacillus in milk is not manifested so rapidly as is that of the scarlet fever germ; but the disease germ is none the less active in producing disease, because you cannot always trace the exact time at which it commenced to do its work. Lastly, official slaughter-houses must in time become a part of our system of protecting ourselves against tuberculosis, especially in the larger towns, whilst a system of inspection and marking, similar to that adopted abroad, must also be merely a question of time.

The instructions of the Royal Commission are certainly so very definite, that they may form a basis of operations for medical officers of health, by means of which they may keep well within the law, and at the same time limit the distribution of tuberculous meat. With powers to act on the lines laid down, their duty, at present or until recently a very difficult one, would become much simplified. Dealers and butchers would know exactly what to expect, and all honest dealers would be able to protect themselves, whilst the dishonest ones surely need scarcely be protected. The sooner we educate our masters up to this point the better it will be for us and for them. We are sometimes told that this source of tuberculosis is more prolific of disease than that. Do not let us quarrel about such matters. Let us look the whole question in the face, take all possible precautions to get rid of the tubercle bacillus; and once we have been successful in doing this, tuberculosis will have few terrors for us. Indeed, it might be pointed out that, in spite of its present ravages, and with so much in favour of the disease and against the patient, tuberculosis is after all, in proportion to its incidence, one of the most curable diseases with which the physician has to deal.

ON ACUTE MANIA FOLLOWING SIMPLE OVARIOTOMY.

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There is no novelty in recording a case of insanity following any form of abdominal operation. All sorts of psychoses have been observed following such operations, including melancholia, mania, dementia, and hystero-epilepsy; and I should not venture to record this case, were it not that it has two elements which render it specially interesting.

CASE 1.—Mrs. G., married, but sterile, was sent to me, from Ayrshire, with a simple ovarian tumour. The prognosis of the case was excellent. The patient was in very good health, was 44 years of age, and had never been in bed from any illness; she had never tasted alcohol, and had an excellent family history. She was sent to a nursing-home, and the operation was performed with ease. There were no adhesions or complications of any kind, the tumour being a simple ovarian cystoma. In the course of the forenoon the patient recovered from the shock of the operation, and by evening she expressed herself as feeling quite well. The pulse and temperature were normal. The following day she continued well. On the third night she became restless and slightly incoherent in her speech, with a slight rise of temperature (100° F.). One naturally thought of some septic condition, but there was no distension of the abdomen, and the wound was sweet. A dose of calomel, followed by a saline purgative, was administered, with the usual result. Next morning she became much more violent, expressed numerous delusions, and could scarcely be kept in bed. From time to time she seemed to become rational, but rapidly became acutely maniacal again. She was absolutely sleepless, and therefore various narcotics were administered—bromidia, paraldehyde, hyoscin, etc., but without any satisfactory result. Her pulse and temperature began to rise markedly; she refused all forms of nourishment, asserting that they were all poisoned, and it was practically impossible to feed her per rectum. That night, the fifth after the operation, her pulse began to fail, and on the following day she died from sheer exhaustion. There was no history of direct hereditary tendency to insanity, but there seemed to be some traces of mental weakness in collateral branches of the family.

My diagnosis was, acute mania following the operation; and in order to confirm this a post mortem examination was made, when everything in the abdomen was found perfectly natural, and with no indication of septic infection.

The points of special interest in the case which I have just recorded are, first, its very early development after the operation, occurring as it did on the third day. Howard Kelly reports one case occurring shortly after operation, but the exact time is not given. Most of the recorded cases occurred later, and many of them during the period of convalescence. Of the other three cases I have met with, two occurred in the second week and one in the fourth week after the operation, and all recovered.

The second point of interest in the case is the rapid and fatal issue. I can find no records of any such case, most of those recorded either recovering or remaining permanently insane. Further, most of the recorded cases have been in women who either had well-marked hereditary tendency to insanity, or who had themselves at some previous time been insane. Spencer Wells records two cases of insanity during convalescence after ovariotomy, but both were in patients of lunatic families. Savage of Birmingham, out of 483 cases of double oöphorectomy, had four cases—three recovered, and one committed suicide six months after the operation. Keith had six cases of insanity in sixty-four cases of hysterectomy. Three of these were acute, three became chronic. Howard Kelly, out of 2000 abdominal sections, had eight cases of insanity. Five recovered completely, two remained insane, and one committed suicide after her return home. In 1000 cases of abdominal section performed by myself, I have met with mental disturbance four times,—three times after ovariotomy and once after hysterectomy. All of these patients recovered completely, except the one whose case I have just recorded.

It seems to be beyond a doubt that this accident after an abdominal operation occurs very exceptionally in women of sound mental health themselves, or with a clean hereditary record. Nearly all the cases recorded have occurred in women who themselves have shown some mental instability, or whose family history shows insanity in some form. The question, however, as to its occurrence after ovariotomy is somewhat different. Here an entirely new element is introduced, when the ovaries become the subject of operative interference; and the records of many operators show that removal of the ovaries and of the uterus is productive of various forms of insanity in a much larger proportion of cases than are ordinary operations not involving these structures. This is not difficult to understand. In every phase of ovarian activity, from the period of puberty onwards, a state of mental instability is produced, more or less extreme, according to the constitution and general predisposition of the patient. Disease or even small malformations of these organs produce mental phenomena which may amount to insanity, and occasionally these mental conditions are relieved by removal of the affected organ; and in this connection I should like to record the following case:—

Case 2.—E. C. was sent me, on the recommendation of Dr. Clouston, suffering from acute mania at each menstrual period, while between the epochs she was quite quiet and sane. She was so violent when in my ward that she had to be isolated in the padded room of Ward 6 in the Infirmary. After careful consideration of her case with Dr. Clouston, I removed her ovaries, during an intermenstrual period. One of them proved to be somewhat enlarged and cystic. She had no return of her insanity during the years she remained in Edinburgh, and menstruation ceased entirely. Later she went to Los Angelos, in California, and from there she sent me a post-card regularly for two years, during which she remained quite well. During the last five years I have not heard of her.

I only interpolate this case to show the profound influence the ovaries have upon the nervous condition of the patient. This case, however, is just the converse of the previous one, and the opposite of one's usual experience, which is, that removal of the ovaries rather upsets the mental balance than otherwise. Personally, this is the only occasion on which I have removed the ovaries with the object of curing a mental condition, and in this short communication I do not, of course, refer to women who are operated on for any condition during continuous insanity. Many such cases are recorded in asylum reports.

The prognosis as to life in the insanity occurring after operation is always good. Besides my own I have no record of an immediately fatal case.

With regard to the mental recovery, it simply is a question whether the patient has a good personal and family history or not. If not good, the patient is apt to remain permanently insane, and *vice versâ*.

MEDICO-LEGAL CASES.

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Cases connected with the Action of Fire on the Body.

Case 1.—The following case, which occurred some years ago, raises several questions of interest to the medical jurist, and, under certain circumstances, might have given rise to a charge of homicide, and the arrest of an innocent person.

Mrs. A., æt. 36, had been delivered of twins about three weeks before; her confinement was normal, and recovery satisfactory. The twins, however, died four days before the circumstance which I am about to relate took place. Her husband, after his midday dinner, went out at 2.15 P.M., leaving her with her child, apparently cheerful and in good health. On returning home at 7 o'clock in the evening, he found her lying dead on the floor in front of the fireplace, her head resting upon the fire-bars of the grate. On the floor, near the body, was a bed-pan and syringe, with which she had evidently been giving herself a douche. The child was sleeping on the hearthrug. The fire in the grate was nearly out, and had not, apparently, been a very large one. The whole of the left side of the head and face was scorched, and the skin in places severely burnt. The scalp over the left frontal and temporal bones was charred, and the bone over an area 2 in. in diameter was exposed. The hair on the left side was burnt and partly destroyed. Her clothes and the rest of the body were uninjured by fire.

The post mortem examination was performed next day. Body, medium height, spare; rigidity strong over the whole body; well-marked post mortem lividity. No marks of violence or injury except the condition of head above described.

Heart.—Right side distended with fluid blood. Left ventricle contained a small quantity of similar blood. Valves competent and normal. Muscular substance pale, very flabby, and more friable than normal. Microscopic examination showed some fatty degeneration of fibres. The aorta was of normal size, and healthy.

Lungs.—Few fibrous adhesions; no fluid in pleural cavities; marked congestion of lung substance, and larger vessels full of fluid blood; bronchi contained a quantity of frothy mucus.

Liver.—Pale; slight fatty degeneration; not congested.

Spleen.—Anæmic; normal.

Kidneys.—Healthy; not congested.

Stomach.—Distended with gas and some food; mucous membrane pale and anæmic.

Uterus.—Double the size of the normal, non-pregnant organ; retroflexed, substance soft and flabby, pale, and anæmic. Some red-coloured blood escaping from the os. In the cavity the mucous membrane was coated with a little fluid blood, and two small irregular tears were noticed in it, each about 1 in. long and $\frac{1}{8}$ in. deep. The rest of the genital organs presented a normal appearance.

The larynx and trachea were normal; the latter contained some frothy mucus.

Head.—In addition to the external conditions described above, the scalp was noticed to be unusually dry and bloodless. The skull bones were also drier than usual, and, where the skin had been burnt through, the bone was blackened, from charring of the surface. The cranium was intact, and was not fractured. On removing the skull-cap, a large brown mass, evidently a blood-clot, was seen lying upon the surface of the dura mater, over the frontal and temporal areas, while above this again, nearer the vertex, there was a quantity of a whitish brown fatty material. The brown clot was about $\frac{1}{2}$ in. in thickness, and consisted of blood which had been altered by heat. The dura mater was leathery and dry, especially upon the left side. It was not congested, and no injury of any vessel could be detected.

The brain was flattened in the area subjacent to the clot. The membranes were not injected, and the vessels were healthy and not ruptured. The brain substance was drier and firmer than usual upon the left side, and the whole brain was pale and bloodless.

The first question which is of interest in the case is as to the cause of death. Did the deceased die suddenly from failure of the heart while douching herself, and fall to the ground with her head resting on the fire-bars? or did she merely faint, and die from the injuries produced by the fire? or, again, did rupture of a vessel cause the effusion of blood, and, by compression, produce unconsciousness? or, lastly, was the effusion, which was similar to a middle meningeal hæmorrhage, the result of violence, and the cause of death—the murderer having attempted to hide the effects of his violence by burning?

It is a well recognised maxim, that while hæmorrhage into the substance of the brain is most commonly the result of disease, hæmorrhage external to the dura mater is, with few exceptions, caused by external violence. At first sight, therefore, looking to the position of the hæmorrhage, and the fact that the brain and its vessels were healthy, one would have been perhaps justified in coming to the conclusion that it was the result of the rupture of some vessel, caused by external violence. Marks of violence on the skin might have been destroyed by the action of the fire, while, on the other hand, it is well known that such effusions of blood may result from violence, without any fracture of bones, or, indeed, without any external evidence of violence being present. If this view of the cause of the hæmorrhage had been adopted, the case at once became one of the most serious nature, involving a charge of homicide. The circumstances, however, gave rise to no suspicion, and it was proved that the woman had been quite alone during the afternoon, until the body was found as detailed.

The opinion formed at the time as to the cause of death was that the deceased had died suddenly from syncope, and that the burns were caused after death. No doubt there was little in the condition of the organs to account for sudden death, but, on the other hand, it is well known that sudden deaths take place almost every day, in which no greater pathological changes are to be found than in the present instance. The heart muscle was very flabby, and was undergoing fatty degeneration, and with this condition present in a woman during the puerperium, sudden syncope, as a result possibly of over-exertion, is not unlikely to occur. There were no data to decide whether the burning had taken place during life.

The possibility of the deceased having merely fainted and succumbed to the injuries caused by the fire, cannot, I think, be entertained, because the burning must have been very gradual, more of the nature of scorching than the direct action of fire; and had she fainted merely, the stimulation of the heat would probably have quickly brought her round, or at least she must have come to herself before the effects of the heat had rendered her incapable of movement. Moreover, the blood in the heart was fluid—this is frequently seen in cases of sudden death, but rarely, if ever, where the person dies slowly. No doubt, fluidity of the blood is characteristic of death from asphyxia, but there was no other evidence of death from asphyxia—her clothes were not burnt, nor anything else which in burning might have given rise to asphyxiating gases; there was no obstruction in the air passages or constriction round the neck, and although the trachea contained some frothy mucus, this is not inconsistent with sudden death.

The presence of the clot within the cranium was an element in the case which was difficult to explain. I could not discover any record of a similar appearance having been observed in cases of burning, except where there was evidence that it had resulted from violence.[1] He cites the case of a woman whose partly charred body was recovered from a house which the husband was supposed to have set on fire. The right side of the face was charred, and on removing the skull-cap a reddish brown mass was found lying between the bone and the dura mater. The clot consisted of altered blood. There was no fracture of the skull. The question to be decided was, whether this effusion was the result of violence (the husband had been arrested on suspicion, as a result of the opinion given by the local medical man in regard to the condition), or whether it was the result of the great heat to which the head had been exposed.

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[1] A paper published by Professor Strassmann of Berlin, last year, upon "An Appearance in Death from Burning," throws light upon the subject.

Strassmann confessed that he would have been inclined to attribute the condition to violence, had he not met with a similar appearance a few weeks before, in a body which was burnt as a result of an explosion. On removing the skull-cap in this case, a clot was found over the frontal area where the action of the heat had been most intense; and around the clot, lying free, there was a mass of melted fat. The suggestion occurred to him, that possibly the condition was caused by the blood in the bones and surrounding soft parts being expressed by the heat, and he instituted experiments to determine the question. Great localised heat applied to a human skull, and also to the skulls of animals recently killed, caused the blood contained in the bone to be driven out, and in the case of the experiments on animals produced a distinct clot and condition similar to that observed by him in the two cases cited. He concluded that there is little doubt that such effusions of blood are produced as the result of great heat acting upon a localised area of the skull.

The explanation of the condition is of no little value from a medico-legal point of view. The fact that effusions of blood may be found within the skull, similar in all respects to traumatic extravasations, but independently of any violence exerted on the body, is one of the greatest importance, and must be kept in view in cases occurring under similar circumstances to the one I have described.

Case 2.—The following case is interesting, as showing how in the same body there may be burns of the most severe description, involving complete charring and destruction of a limb, while other portions of the body remain wholly uninjured.

An alarm of fire was raised in a house occupied by an old woman 82 years of age. The room was so full of smoke that the neighbours could not enter, but in the course of about a quarter of an hour a fireman went in, and found a table in the centre of the room and the floor beneath it on fire. At the side of the table he found the body of a woman lying underneath an armchair, which was also on fire and partially consumed. It is presumed that the deceased, who was very frail, in trying to light the gas had allowed her clothes to catch fire, and in her fright had overturned the chair upon herself. Her clothes were completely burnt except some portions of those covering the trunk. Her limbs were contorted, the legs being drawn up and the arms flexed and raised in front of the body. The whole of the skin of the body below the neck was burnt; that over the trunk, right arm, and leg being hard and leathery; in some places the cuticle was raised in blisters, containing fluid and showing injection of the vessels at the base; in other places the skin was charred and split in long rents with irregular inverted edges, showing the muscles beneath. The left leg below the knee had disappeared, only the charred stump of the tibia and fibula remaining. The skin of the thigh above was charred and divided by great rents, exposing the muscles, which, however, were not themselves much burnt. One of these rents divided the skin of the popliteal space, and gave an appearance as if the deceased had been hamstrung, but the distinction from such an injury caused by a cutting instrument could be made from the fact that, while the skin and subcutaneous tissues were divided, the flexor tendons running across the wound were intact. The skin of the left arm was likewise severely burnt, and had rents similar to those described, running longitudinally downwards. The hand was completely charred, the fingers being absent, and only a blackened stump indicating the metacarpal bones.

The skin of the abdomen and back was tense, dry, and leathery, and of a dark brown colour, but was not burnt through at any point. The face was congested, the lips were livid, and the eyes injected. The skin of the face, the scalp, and hair, which was abundant for a woman of her age, was quite unaffected by the action of the fire.

The interest of the case lies in the peculiar and suspicious position in which the body was found, which, under other circumstances, might well have given rise to the belief that an attempt had been made to get rid of the evidence of violence by setting fire to the body and house; and also, a point of considerable interest, is the rapidity with which the whole of one leg below the knee was completely destroyed. It was thought, judging from the time when smoke was first noticed, and the alarm of fire given, that not more than twenty minutes, at the outside, could have elapsed before the rescue of the body; and, from the amount of damage done, the fireman did not think that the fire altogether had lasted more than thirty minutes. In this case we have an instance of remarkably rapid destruction of a limb by burning. Hofmann, writing in regard to the time required for complete charring of a body or parts of one, says that, from his own experiments, portions of a body, such as limbs or the head, when placed in a large stove, with a good draught and fed with wood, required an hour in the flame, and a further hour's exposure to the red-hot embers, in order to destroy the soft parts and calcinate the bones.

The fact of the face and head remaining totally uninjured in a case of such severe burning, especially where not only the lower but also the upper limbs were so markedly affected, is interesting. Probably the chief reason for this is to be found in the fact that these were the only parts of the body not covered by clothes; it is also probable that, in some manner, they were protected from the fire by the back of the chair.

CASE 3.—The next case illustrates the fact that even when the body of a newly-born infant has been exposed to fire, to the extent of complete destruction of certain portions, it may still be possible to determine many of the questions which are important from a medico-legal point of view.

A bundle, containing the charred remains of an infant, was found in a quarry. The head and face were uninjured, but the rest of the body was charred and partially destroyed. Both lower limbs were absent, with the exception of the two femoral bones, which were wholly devoid of the soft parts in their lower two-thirds, and which stood up, fixed at right angles to the trunk. The lower epiphyses were absent. The skin of the buttocks and lower half of the trunk was black and charred, like burnt leather. In the pubic region a small hard blackened mass was made out to be the scrotum, thus determining the sex of the child. The skin of the abdominal wall was burnt through in the left iliac region, and, from the opening, what appeared to be meconium was escaping. The skin over the chest and back was hard and leathery to the touch. There were two large rents, one in the neck and another extending from between the shoulders to the right loin, which exposed the muscles, and, in the case of the latter, penetrated to the ribs and abdominal cavity. These wounds had undoubtedly been produced by a splitting of the skin, in consequence of the action of heat, but from the appearance of the edges and deeper parts it is probable that rats or other vermin had attacked the body. Both arms were severely burnt; the skin was charred, and in many places destroyed, exposing the deeper tissues, which were also burnt. The right hand was absent.

From the appearance of the remains one might well have doubted whether a further examination could throw any light upon the questions which it is important to determine under the circumstances, namely, what was the age and maturity of the child? Had it lived? And, lastly, what was the cause of death? How far a careful examination afforded answers to these questions, the following account will show:—

The remains weighed 4½ lb., and measured, from the crown of the head to the lower extremity of the buttock, 11 in. The head was of the usual dimensions of a full-time child. There was well-marked head moulding. The pupillary membrane was absent, the conjunctivæ were injected but showed no petechial ecchymoses. The lips were livid. The nostrils and mouth contained no foreign body. There was no apparent sign of decomposition. The umbilical cord was absent, only a charred nodule indicating its point of attachment.

It was impossible to reflect the skin over the thorax owing to its dry and friable condition, and it had to be removed along with the sternum. The thoracic organs were free from any sign of decomposition; the appearance of the lungs at once denoted that they had respired; the lungs were prominent, distended, with rounded edges, and presented the light red "marbled" appearance characteristic of respiration. They showed no petechial ecchymoses on the surface, and, on section, blood and some frothy mucus escaped from the divided tissue. They floated buoyantly, both with the heart and thymus gland attached and when subdivided into small pieces. The lower part of the right lung was dry and leathery from contact with the skin and diaphragm, both of which were much altered by heat. The heart contained fluid blood, and presented a normal appearance. The foramen ovale was patent. The anterior surface of the liver was pale, dry, and leathery, and adherent to the charred skin over it. The rest of the organ was normal. The spleen and left kidney presented no unusual appearance. The posterior surface of the right kidney was dried and altered by heat, and a portion of it had apparently been eaten by vermin. The stomach contained a small quantity of tenacious blood-stained mucus. The small intestine was empty. The large intestine was filled with meconium, and there was a rent in the descending portion from which the meconium was escaping. The testes were present in the scrotum. There was no foreign substance in the larynx or trachea. The mucous membrane of these parts was congested. On reflecting the scalp, a well-marked caput succedaneum was found over the left parietal and occipital bones. The cranium was uninjured. The meninges were injected; the brain was normal. The conclusions drawn from the examination were—(1) That the child was probably mature; (2) that it had fully respired, and therefore lived; (3) that life terminated shortly after its birth; (4) that in all probability it died from asphyxia, an attempt having been afterwards made to get rid of the body by burning.

The evidence in favour of its being mature rests upon—The absence of the pupillary membrane; the presence of the testes in the scrotum; the weight of the remains, namely, 4½ lb., and if there be added 1 lb. as the combined weight of the legs and various small portions of flesh eaten by vermin, together with the missing hand, this would bring up the total weight to 5½ lb.; if to the length of the remains, namely, 11 in., 7 in. be added as the length of the leg, we have a total length of 18 in.; the general appearance and size of the head. The ossific nodes at the ends of the femora were of course absent, while the nails were also absent, so that no help could be got from these data.

As to the child having lived and fully respired there could be no question, from the condition and appearance of the lungs, while the fœtal contents of the stomach, and the condition of the large intestine, make it highly probable that the child died shortly after birth. The determination of the cause of death is difficult. There were no data by which an absolute opinion could be given as to whether the burning took place before or after death, but in all probability it occurred after death. It is difficult to believe that a mother, however heartless, could stand by and see her infant being burnt alive, in addition to which the opportunity of burning a living infant without exciting suspicion is infinitely more difficult to find than merely trying to get rid of a body by such means. The absence of any carbonaceous or soot particles in the mouth and air passages also points against an attempt at burning during life.

Apart from the injuries caused by fire, there was no evidence of violence on the body, while none of the organs showed any traces of disease sufficient to account for death. On the other hand, the condition of the lungs and the fluidity of the blood in the heart, in the absence of any disease or apparent cause of death, may at least entitle us to hazard the opinion that the cause of death was asphyxia, though how this was produced it was impossible to determine from the examination of the body.

The actual facts of the case were never elucidated, but it was made out that eleven days had elapsed between the birth of the child and the finding of its body.

POISONING OR STRANGULATION?

CASE 4.—*Poisoning or strangulation?—Suicide.*—The following case is a somewhat remarkable one, and had the facts not clearly indicated suicide, there can be little doubt that the discovery of the body, under the circumstances about to be related, might have been held to be strongly presumptive of homicide. A girl, æt. 19, of loose character, engaged a room in an hotel late one night. Nothing was noticed in her manner or behaviour to indicate any mental excitement or depression, and she appeared to be cheerful. Next morning at 8.45 A.M., she was called by the landlady, and answered by saying, "All right." At 9.10 she was called again, and shortly afterwards was heard to open the door and take in her boots. At 9.30, as no sound of movement was heard in the room, she was called again, but no answer being received, the landlord broke open the door. She was lying upon her back in bed, with the bedclothes pulled tightly over her head and drawn down behind her neck, so that her head was completely enveloped in them. Her neck was tightly constricted by a silk scarf, which was tied in a knot under the angle of the lower jaw on the right side, and then passed a second time round, and was tied again under the chin. The ligature was so tight, that a penknife, which was used to sever the scarf, could only be inserted with difficulty between it and the skin. On the floor near the bed was a quantity of vomited matter, while at the other end of the room, on a table near the window, there was a tumbler containing a small quantity of brownish oily fluid, and a 2 oz. blue poison bottle, both smelling strongly of A.B.C. liniment.

At 10.30, when I saw the body still lying on the bed, the face and lips were deadly pale, the tongue firmly held between the teeth, and the pupils dilated. In the skin of the forehead, of either side of the nose, of both upper and lower eyelids, and of both cheeks near the angles of the lower jaw, there were innumerable punctiform ecchymoses; indeed, so numerous were these on the forehead and eyelids, as to give the appearance of a reddish blush to the skin. Under the conjunctivæ of the upper and lower lids in both eyes, there were also numerous ecchymoses. There was no froth at the mouth or nose. The vomited matter consisted of ordinary semidigested food of a whitish colour, and neither it nor the body gave out any special odour. Encircling the neck were two pale, in places slightly bluish, furrows or constriction marks, about ¼ in. deep. Under the angle of the jaw on the right side, the skin was reddened or injected, but the cuticle was not abraded. Both constriction marks were situated above the thyroid cartilage in front, and ran backwards horizontally, being less distinct behind than they were at the sides and in front.

A post mortem examination of the body was made next day, when the following conditions were found:—

Body.—Well nourished and developed. Rigidity present in trunk and limbs. Post mortem lividity of a deep purple colour, uniform on all underlying portions of the body. Nails of hands deeply livid. Punctiform ecchymoses, as detailed above. The constriction marks noticed the day before had become indistinct, and in some places all trace of them had disappeared. In the skin on the right side of the neck were three small superficial cuts, containing no blood, and with pale non-injected edges. These were probably caused in the attempts to cut the constricting bands, when the body was first discovered. The diaphragm extended to the fifth intercostal space on either side.

Heart.—No ecchymoses on its surface. Left side contracted, and contained a little fluid blood. Right auricle and ventricle flaccid and semi-distended with some loose dark clot and fluid blood; there was also a small piece of white post mortem coagulum. Large vessels leading from heart distended with dark fluid blood. Blood elsewhere throughout the body was fluid. The aorta was very narrow, scarcely admitting the forefinger. The heart was otherwise normal and healthy.

Lungs.—The right lung overlapped the heart considerably, and its anterior border and middle lobe were emphysematous. Both lungs showed several ecchymoses on their surface, were markedly congested, and contained frothy mucus in the bronchi. They showed no trace of disease, and gave out no odour.

The liver, spleen, and kidneys were all markedly congested, but otherwise normal.

The stomach was contracted, empty, and gave out no odour. The peritoneal surface was not injected. The mucous membrane was covered with mucus. Rugæ prominent at cardiac end in greater curvature, and in this area the mucous membrane was uniformly injected, while in a patch about 2 in. in diameter the red hue was deeper, and there were a number of recent small blood extravasations under the mucous membrane. Towards the pyloric end the mucous membrane was pale. There was no corrosion or ulceration.

The duodenum contained some dark yellow bile, and about 3 in. from the pylorus there were numerous small extravasations of blood beneath the mucous membrane, which was congested. The small intestine was empty, the mucous membrane for a length of at least 6 ft. was congested, and at intervals the free edges of the villi were of a uniform bright red colour, due to extravasation of blood in the submucous tissue. The large intestine contained fæces and was normal.

Uterus retroflexed, old adhesions matting together ovaries and adjacent tissues.

The larynx and trachea were full of fine, pink-coloured, frothy mucus; the mucous membrane was markedly congested, while on the under surface of the epiglottis, and within the larynx, there were a number of small ecchymoses. No fracture of bones or injury of muscles. In the loose connective tissue behind the larynx there was an extravasation of blood the size of a crown-piece, but of irregular shape; also, in the deeper tissues on the anterior surface of the spinal column, there were one or two small extravasations.

Head.—In the fascia covering the temporal muscles, also in the frontal area, were numerous extravasations of blood, varying in size from a pin-head to a pea. Dura mater congested; meninges very markedly congested; brain, slight œdema, greatly congested.

I have given the post mortem appearances at some length, because from them it is alone possible to decide whether death was the result of poison or of strangulation.

Suicide by strangulation is very uncommon, not as Hofmann, in his book on "Medical Jurisprudence," remarks, "on account of any real, but rather on account of a fancied difficulty in carrying it out; the rarity of this mode of suicide is probably accidental, for it seems to me to be comparatively easy for a person to compress his throat by means of a constricting band, so that the air passages, or at least the large veins, are occluded, when unconsciousness must soon come on, and also death ensue, if, by means of a knot or lever, the compression is maintained after the onset of unconsciousness."

Was death due to strangulation in the present instance? Many of the post mortem appearances were consistent with this mode of death, some almost diagnostic of it. Among the first may be mentioned the strongly marked post mortem lividity, the congestion of the lungs and petechial ecchymoses on their surfaces, the congestion of the various internal organs, the fine froth in the air passages. Among the latter must be classed the ecchymoses upon the face, eyelids, nose, cheeks, and those under the scalp and in the larynx. Small blood extravasations on the surface of the lungs, and in the connective tissue in various parts of the body, are not uncommon in various forms of death apart from violent asphyxia, but extravasations situated as described in the above case, and so numerous, I have never seen except in death arising from forcible occlusion of the air passages and blood vessels, such as takes place in hanging and strangulation. Only one condition in the body may at first sight appear inconsistent with death from asphyxia, namely, the fact that the blood in the right auricle and ventricle was loosely coagulated. But while fluidity of the blood is a very constant post mortem condition in death from asphyxia, it is well known that clots of considerable size may be present in the heart and great vessels, even when death has occurred from acute asphyxia. The condition, it must be admitted, is, however, unusual, but we know so little as to the cause of the fluidity of the blood in asphyxia, that it is possible the liniment, some of which may have been absorbed, accounts for the slight coagulation observed in the heart. In all other parts of the body the blood was perfectly fluid. It may be asked, was the constriction of the neck sufficient to account for death? and in my opinion it was. The evidence of the landlord is, that only with difficulty could he force his finger behind the handkerchief, while he found such difficulty in cutting it with his pen-knife (and in his attempts cut the skin in three places), that he was obliged to employ scissors, which he was able to force more easily between the neck and the constricting band. It is improbable that the constriction was sufficient to occlude or compress materially the carotid arteries, but it must have compressed the venous trunks, while its position above the thyroid would force the base of the tongue upwards and backwards against the posterior pharyngeal wall. Thus we would have the circulation to the brain greatly impeded, and the air passages more or less completely occluded.

Was death, on the other hand, the result of poisoning? A.B.C. liniment is a potent poison, as a case which came under my notice recently, where death occurred in twenty minutes after a dose of two tablespoonfuls, administered accidentally, proves. But in this latter case the mouth, the abdominal cavity, the stomach contents, the lungs, and other organs, smelt strongly of the liniment, while in the former case no odour could be detected within an hour after death at the mouth, or next day at the post mortem examination in any organ of the body. In the instance of undoubted poisoning, the blood was in a similar condition to that of the case under discussion; there were no ecchymoses to be seen anywhere, the mucous membrane of the stomach was comparatively pale; the other organs, except the spleen, were greatly congested.

The circumstances appear to indicate that the deceased swallowed the contents of the bottle (the amount of A.B.C. liniment originally contained in it could not be ascertained), and that then she vomited all or the greater proportion of the poison, and having no more to take she determined to complete the attempt on her life by strangulation. That some of the poison was retained, the injection of the mucous membrane of the small intestine seems to indicate, this effect pointing probably to the local action of the camphor and chloroform in the liniment, rather than to the more potent poisonous constituents, aconite and belladonna.

Taking all the circumstances of the case into consideration,—the short time which elapsed between the last time she was heard to move and the finding of the body, the fact that if she took the A.B.C. liniment, it was quickly rejected along with the contents of the stomach, and, on the other hand, the well-marked evidence of obstruction to the circulation and respiration, I think the case must be regarded as one of strangulation, and if any of the poison was absorbed that it merely played a secondary *rôle*.

A VISIT TO A GERMAN INSTITUTION FOR THE TREATMENT OF PULMONARY DISEASE.

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A recent visit to one of the more widely known Sanatoria in Germany has led to my being asked to put down in writing an account of such impressions as were conveyed to my mind by it, to which I have ventured to add a few deductions, suggested by these impressions. The visit was made at a period of the year usually regarded as that of midwinter. The clerk of the weather, however, who appears for some years back to have been suffering from delusions about what constitutes winter, at the end of last January seemed to have forgotten to arrest the autumnal flow, over Western Europe, of cyclonic systems from the Atlantic. My visit resembled one made in spring rather than one in drear January. Though this was so, the advantages conferred by mild weather, as far as travelling went, were counterbalanced by the perturbed state of the Channel. I was accompanying a lady patient over to Falkenstein, and apprehended that a rough passage across the strait might produce ill effects on one who was a very poor sailor. And here just a word as to the measures taken to ameliorate this. Forty grs. of potassium bromide were given daily in two separate doses for five days, and one of 40 grs. on the morning of the crossing. Bromides may prevent a fair sailor from suffering the torments which attend irregular equilibration, but I have not found them able to do more than lessen the troubles of the really bad sailor. The bad sailor will suffer whatever you do, but his pain and distress can be greatly alleviated by the use of pure cold water at the time, in addition to the previous exhibition of bromides.

When sickness occurs, make the victim drink a large tumbler of cold water. Hot water cannot be taken fast enough, warm water is nauseating. After the water has been rejected, repeat the dose, and persevere with this until the stomach gets tired of rejecting that which has neither taste nor smell, and does not nauseate, and the gastric nerves, locally non-irritated, will signal to the disturbed centres for equilibration and emesis in the brain, that the stomach has no work to do of a useful nature, and thus help to restore peace. The water itself, if taken freely, absolutely prevents any of that painful retching which occurs when the stomach is empty, often so violent, and productive of injury to the parts strained.

The village of Falkenstein is situated about twelve miles north of Frankfort-on-the-Maine, on a spur of the Taunus range of hills. The Sanatorium is 1300 ft. above the sea-level; it faces south; is guarded from winds from other points of the compass by low-topped hills; is backed by rising tree-clad slopes, intersected with paths through the pines so universally found in this part of Germany; looks out over a wide well-kept park, past the picturesque features of Cronberg's village and castle, to the dim outlines of the hills of the Rhine valley above Mainz, across the broad flat plain of the Maine. An admirable site for a pure-air Sanatorium. No doubt, with the wind blowing from Frankfort way, or from the manufactories of Mainz, the air may not be innocent of impurities. Blowing from other "airts," there is small chance of contamination. Indeed, on arrival on a clear, cloudless January day, with but a touch of frost in the air at Frankfort and at Falkenstein, the more bracing effect at the latter place was very evident.

So we arrived at the Sanatorium, not in a sledge, as is commonly the winter means of conveyance, but in a carriage and pair; not in cold or snow, but with a warm sun shining out of a clear sky. The building itself is composed of a large main block, surmounted by a minaret-shaped tower, with two wings, each connected by a covered way with separate buildings. A deep covered verandah extends right along the south face of the main building and the sides of the wings turned towards it, a step or two above the ground level, and is furnished with numerous couches, in which patients lie tucked up in furs for many hours each day. Within the building the entire absence of carpets and mats is very obvious, as is the ubiquitous presence of spittoons filled with water, and the numberless notices displayed forbidding the use of handkerchiefs or rags for the reception of the sputum, and insisting upon the employment of Dettweiler's pocket sputum-flask, or the numerous spittoons. Notices—as, "Gentlemen are begged not to spit into the basin," are even to be seen.

On the ground floor are all the administrative offices and public rooms; the kitchen, baths, and other similar rooms are, however, below this in a sunk flat. The medical staff are provided with a suite of rooms at the further end of one of the wings, comprising waiting, examination, laryngological rooms, and a laboratory. Dr. Hess, the well-known successor to the still better known Dettweiler, is in charge of the institution; Dr. Dettweiler, who now lives at Cronberg, only visiting it about twice a week.

The system carried out at Falkenstein is not so severe nor so autocratic as that insisted upon at Nordrach. The patient is advised, not coerced. The results are admirable. In addition to numerous meals, several glasses of milk, warm from the cows. are expected to be taken each day, and taken *slowly*. Beginning with four glasses, the number disposed of gradually reaches to about double. No medicinal treatment for the lung condition is adopted. If the larnyx is affected, some topical application may be made, but not invariably. The treatment depends entirely upon long hours of rest on couches in open verandahs, exposed to pure, dustless air; upon the ingestion of a large amount of food, given frequently and not in large quantities at a time, much of it in the form of milk; in walks of gradually increasing length, as the patient becomes stronger, along the paths over the Taunus Hills; these walks only bearing a very small proportion in length of time to the number of hours spent in the recumbent position. Many people are at a loss to understand how patients keep up their warmth under such enforced rest; but unpleasant feelings caused by cold do not appear to be experienced by patients, notwithstanding their lack of active exercise, thanks—first, to their furs and wraps, used after the manner of the Chinaman's winter attire; as the temperature falls the covering is increased, with a rise of the thermometer it is reduced; second, and perhaps more really, to the large supply of heat calories supplied them. As they ingest, and presumably make use of, a large amount of food-stuff, the absence of even moderate physical exertion, and the constant presence of easily accessible oxygen in a normal and unadulterated condition, along with the small degree of denudation the absorbed products needs must suffer in connection with the upkeep of an almost quiescent nervous system, allow the thermogenetic centres of the patients ample material for dedication to heat-producing processes, apart from the creation and upkeep of nervous or muscular energy,—in fact, enable them to improve the metabolic standard of the tissues. No person in health, properly fed and rightly reared, should be unable of himself to supply the degrees of warmth necessary to neutralise the small loss of heat occurring when he is enveloped in non-conducting coverings. If he is incapable of doing this, he either is not healthy or has not been supplied with sufficient or proper food. The more readily the thermogenetic centre of a phthisical patient is able to neutralise loss of heat, or the thermolytic mechanism to keep the outgo equal to the manufacture, the greater their owner's independence of the tubercular poison, and the more pronounced the resistive action of the tissues against it.

Lower fever by the use of antipyretics acting through the nervous system, and you disturb the thermotaxic mechanism; inhibit or kill some at least of the bacilli by administration of antiseptic substances, in adequate doses, and you disturb both digestion and metabolism; shield the body from draughts, from cold, from fresh air, and you prevent the proper access of the most important element for the complete nourishment of the tissues and for the power of the body to resist the agents of infection.

The changing of any damp garments directly upon return, and the avoidance of sallying out of an over-heated, ill-ventilated room into the open, and of entrance at once into such an atmosphere on return, are all that appear necessary. If a patient does suffer harm from such excursions, his indoor existence cannot be led under proper conditions. As an extreme instance of the truth of this, the custom at Nordrach of permitting the patients undergoing treatment at that institution to go out in wet weather, and to lie down afterwards for considerable periods of time without removing their damp or soaked clothes, may be instanced. Consumptives, even, do not suffer from a thorough wetting if they do not change their clothes afterwards, provided they live largely in fresh air and can make use of a large amount of nutriment, such as they are there supplied with. This Spartan method, however, must surely be employed with some degree of caution and selection; though successful in many recorded cases, how many unrecorded cases are there in which it failed to cure, or caused an aggravation of symptoms?

There is no need to enter into more minute details about the various meals and the active foods provided at Falkenstein, they have often been catalogued before. It is sufficient to say that there is an abundance of food, that fresh milk is given several times a day between the meals in tumblerfuls; but that no coercive measures are employed, as is the rule at some of the other Sanatoria. If a patient has no appetite, or cannot without discomfort eat more than his appetite or mental fortitude is able to permit, forced feeding is not resorted to. The patient is encouraged to eat as much as he can, and is told the reason why.

The Sanatorium treatment is not fitted for all: the previous disposition of the patient gives no sure guide as to what his demeanour will be, or how far his mind will help his body when undergoing it; should he fall early into a despondent state under it, his future history is not very hopeful. But the numerous successful instances recorded suggest a trial, at least, of the chances of cure or arrest offered by the treatment. The fact that no drugs are used (except topical applications for laryngeal lesions, and alcohol perhaps when there is fever), that rest in open air which is pure and not dust-laden, however hot or cold or misty, along with daily walking exercise, proportional to the condition and strength of each patient, and at Falkenstein along hill paths among trees and pines, and the bedroom windows being kept open, should frequently be able to so alter the soil of the body, or the fluids which irrigate it, as to render it incapable of supporting the bacilli of tubercle, is one which is of far-reaching importance.

A phthisical patient among foreigners is a very different being from one domiciled in a native institution. He follows implicitly all the rules (if he does not, his individuality counts for little, and his retirement from the scene is speedy), while a very probable factor in his progress towards recovery is to be found in a mental suggestion of the great power conceived to be possessed by one who speaks another language than he, or speaks it properly. The inability to ascertain the depth of knowledge possessed by one who converses in a foreign tongue, even if some limited acquaintance with it has been acquired on his part, or if the foreigner be able to speak a few words of English, invariably conduces to an increased feeling of respect for the physician's powers, and consequently to a more deeply felt mental suggestion as to the happy effect of his therapy.

Apart from this, the further a patient is removed from the ordinary scene of his daily duties, and the more absolutely he is separated from all his relatives, the greater is his chance of recovery. I was told at Falkenstein that those patients who are accompanied by relatives, however near, or however devoted and self-effacing, react much less favourably to the treatment than the unfriended and solitary.

It has been asserted that climate has but little effect upon the successful carrying out of the sanatorial method. But it seems to me that, although directly it may have little or none, indirectly it has much; and this much, because the amount of food which can be ingested with digestive impunity varies with climatic environment. Arguing from this thesis, the over-all better results achieved by sanatorial methods of treatment in regions with but moderate climates, over those obtained in the more popular warm countries, might possibly be regarded as following the power of coping with a more generous diet in the former sites. There is no doubt but that, despite the number of hours spent daily in the recumbent posture, the amount of nutriment ingested and assimilated by the patients, without detriment to or discomfort from their digestive organs, is surprising. This may be arises from several causes—the simple but varied dietary, *plus* much new milk; the withholding of all medicines and drugs, many of which must often weaken the powers of the alimentary tract and lower the metabolism of the body; the continual presence of free and pure air, and the prevention of any breathing of a vitiated atmosphere; and the marked mental quiescence which, in the majority of cases, settles down upon the frontal convolutions after a few days' stay, and which persists until the patient has so far recovered as to be allowed to move about more freely and to lessen his daily recumbent rest.

The bacilli do not love oxygen; they dislike a rich blood, so supplied with the absorbed products of digestion that it can more readily support the cell protoplasm of the tissues, formerly too weak to combat their power, and can yield a sufficient force to make it resistant—perhaps even to allow it to elaborate antitoxic substances during the struggle induced by the absorption of, and reaction, against the chemical poisons formed by the bacilli.

Again, the state of somnolency so commonly observed to follow the enjoyment of a large meal, either in mid-day or later, in healthy people, has been shown to be due to the presence of proteid bodies in the blood, absorbed from the alimentary tract in too great an amount, or at too rapid a rate for their complete modification proper for the organic needs of the body. It may be that the food taken in, up to just the point of intolerance, may, in the absence of muscular exertion, keep the blood supplied with more proteid than it requires for itself and for the whole organism, and which it is unable completely to deal with in the presence of these same proteids. Albumins and globulins, even perhaps proteoses, could certainly lead to signs of over-proteid poisoning, and such may be the explanation of the post-prandial-like condition which so commonly supervenes. In such a state, not only is the brain affected by the substances circulating in the blood, but it suffers from the increased blood-flow through the abdominal viscera, in response to demands for assistance to grapple with the constantly appearing food-stuffs. In the majority of healthy people the processes of digestion are markedly influenced by the state of their mental cells; the emotions sway their conduct readily; the absence of any emotional source of disturbance must be of great value in the assimilation of a large diet. Often those of the insane, who have little power of mental exertion and in whom no irregular stimulation of their emotional, inhibitory, or excitatory centres is caused by the diseased condition of their brain substance, can consume without sign of harm very large amounts of food.

One of the difficulties which I conceive may well occasion an obstacle to the universal adoption of this form of treatment, is presented by *Spes phthisica.* The consumptive is intoxicated by the metabolic products of the bacilli, and it may be also by the substances elaborated by his blood in its fight against them. The bacilli ingeniously (from their point of view) create the feeling that the obvious results of their struggle for a living wage are as nothing to the landlord of the soil. The only rational treatment perhaps is that alluded to. Replace the *Spes phthisica* by a more assertive *mens sata.* Practically poison the nervous system with the products of digestion. If there is no fever, exercise, carefully graduated in degree, will help in strengthening the offensive forces; with fever, the too assertive bacilli are soon admonished and quieted by absolute all-day rest, along with the same amount of food as before, fresh air *per fenestram,* and some cognac.

If man would cease from expectorating, save into antiseptic-filled spittoons, fires, rags afterwards to be burnt, or into small pocket-bottles easily carried and easily cleansed, the micro-organism of tuberculosis would find life a much harder struggle for existence. But every one should be prohibited from performing this frequently useless insanitary practice, often commenced in imitation, then habituated, because one free expectorator, unaware of the presence of bacilli in his sputum, can yield material enough, when dried and incorporated into all the varied particles which go to make up dust, to infect multitudes. Especially in the rooms and cottages of the labouring and rural classes, in railway carriages, and in all other public vehicles, should this necessity be strongly brought home to the evil-doers. Perhaps the discovery of the properties of glass has had much to do with tubercular affections, and this because man by it can make his dwellings more air-tight. Though the windows of former centuries may have been narrow and innocent of glass, or may have been loosely filled up with other materials, the more imperfect impediments to the entrance of free air helped to combat those bacteria that are cherished by corruption of air from animal confinement. In those days, no doubt, consumption was common among the classes who lived, or at least slept, in rooms with no windows at all, unless may be the doorways had no door to close. A draught is usually but fresh air seeking to restore an artificially altered, polluted, as well as heated atmosphere, to the condition which nature has set up as her ideal; as a rule, where draughts are, there also over-heated and more or less bad air will be found, and those who suffer injury from them are generally those who live in houses in unhygienic atmospheres. Draughts may predispose to inflammatory attacks, but only if the previous conditions have lowered the powers of the body below the point of resistance against infection, except, indeed, where chilling of the body is considerable, but then it is probably more than a draught which is the cause, and must be coincident with contact with the poison responsible for the resultant disease. No individuals ever deserved more to die of pneumonia than did Nansen and his companions, according to the idea of the causation of inflammatory pulmonary complaints, by chills and draughts. Their almost unexampled chills and wettings, however, were encountered, far removed even from the widespread race of bacteria, and they suffered nothing. So with phthisis—a misty day, rain, wet ground, or frost do not of themselves affect the condition, the first only perhaps if bronchitis be also present.

Probably every inhabitant of civilised countries continually comes in contact nowadays with the bacillus of tubercle. If he be possessed of blood and tissues capable of resisting it, or destroying it, or incapable of supplying its metabolic needs, his body repels the invasion. Another time, however, when he is not in such strong health, the bacillus may be able to establish a footing. In such a case, a most frequent occurrence, restoral of bodily strength alone suffices to destroy the bacillus, whose presence is often quite unsuspected; proof in many of them only being obtained if there be an autopsy later on, after death from some other disease. But on every occasion when an invasion of the tubercle bacillus has to be met by the bodily forces, and is successfully encountered, is it not probable that some power is given the blood, though it may be only temporarily, to more effectually withstand succeeding attacks?

Drugs which bear a bactericidal action upon the bacilli in the body may arrest their growth, or even destroy them locally, but they do not alter the nature of the soil; and their continued administration, to guard against recrudescence, leads to digestive troubles. They may be employed with perfectly successful results in the treatment of tubercular affections of subacute types, as those common to lymphatic glands, where local advantages for defence and offence and the usual enclosure of foreign invaders by thick bulwarks of non-infective tissues, suffice to circumscribe the area involved, and seem to render the disease more amenable to medicinal control. In the lung the delicacy of the tissues, the constant movements for respiration, and the more or less violent disturbance caused by coughing, lend lessened scope for this defensive action; but if already exhausted and contaminated air be not inspired for hours both day and night, the large supply of blood to the lung serves to prevent universal infection.

I am fully conscious of my temerity in setting down the foregoing lines, upon what appears to myself to be an insufficient foundation of special knowledge of the subject. But I have ventured to digress somewhat widely from the actual details of the Sanatorium in question, in the belief that the treatment there illustrates the great importance of strengthening the soil, by preventing the access of bacilli by all means, save those which weaken the body in their course of action, and thus render it an easier victim to any later infection; by a life with an abundant access of fresh air, day and night, in a climate which permits of the easy digestion and absorption of a great amount of food without discomfort or alimentary trouble; and because of its demonstration that consumption is not brought on by draughts or by fresh air, unless indeed the tubercle bacillus is allowed to gain a footing in tissues insufficiently resistant from ill-nourishment or improper hygiene to repel it, as for instance during an attack of bronchitis or catarrh; but that it is hastened by closed windows, stopped up keyholes, over-heated dwellings, and over-clothing. No doubt cases of acute phthisis will occur as long as the agents of infection are present with us, in individuals predisposed by heredity, personally devoid of the power of resistance to their attack, or rendered unable to overcome them by faulty habit, or from pathological conditions, such as diabetes mellitus. In such, the sanatorial treatment, it is to be presumed, cannot prove of service. In cases of galloping consumption, the amount of reinforcement needed by the body is far too great and too urgent to be adequately supplied by this method, while drugs, pushed to the point necessary to allow of their action, but weaken the body further.

CLINICAL LECTURE ON A CASE OF TYPHUS, AND SOME LESSONS TO BE DRAWN FROM IT: BEING ESPECIALLY A PLEA FOR THE INCREASED APPLICATION OF BACTERIOLOGICAL SCIENCE TO PRACTICAL MEDICINE.

By W. S. Greenfield, M.D., F.R.C.P., *Professor of Pathology and of Clinical Medicine in the University of Edinburgh.*

(*Concluded from page* 388.)

I have called your attention to some of the dangers of reliance upon inefficient means of disinfection. Let me now refer more particularly to the fact that it may lead to neglect of the common means of cleansing, and may in this way be directly injurious.

Trivial as this matter may seem, I am convinced that a large part of the failure to check both infection and the secondary complications of disease arises from neglect or ignorance of very simple and commonplace facts in relation to cleanliness, and I shall make no apology for mentioning some of them.

The ordinary precautions used in the laboratory and in everyday surgical practice to insure thorough cleanliness and sterilisation are only an application of the facts of bacteriological science; and simple as they are, success depends on their rigid observance.

You will find that the completeness of the measures for the disinfection and removal of dust is a pretty sure criterion of the thoroughness of sanitary precautions in a hospital or ward.

The presence of masses of accumulated dust of long standing, in places where there are many patients, and especially where there is either infectious disease, or any disease which increases the liability to secondary complications, is a sure proof of neglect of proper means to prevent infection.

In the case of many infectious diseases the accumulation of dust means the storing up of the contagium, if it is of a kind which can persist, which thus creates a concentrated focus for its dissemination. In the case of other diseases, it may mean the accumulation of pyogenic organisms or of tubercle bacilli which can produce secondary complications in those who are susceptible. And the risk of such infection will obviously be greater where the patients are numerous, and where they are continuously exposed to the inhalation of the contagium for considerable periods.

For various reasons, it is of great practical value to consider contagia from the point of view of *dust*. But, you may say, dust as we see it in rooms consists of particles of stone, carbon, fragments of wool, cotton, etc., all sorts of things, but these are not especially injurious, still less infectious. But if with these substances there may be particles which can carry or retain contagia, you cannot sort and sift the dust, but must deal with it all together.

A very simple and long-recognised fact, but one not always sufficiently present to our minds, is the "particulate nature of contagion." In other words, that all contagia, so far as we know them, are living organisms of extreme minuteness. These minute particles must be subject to the same physical laws as dust. And the same holds good of some animal parasites or their ova, and of fragments of fæcal matter, epidermis containing contagia, etc., which, though not of such extreme minuteness, are small and light enough to be carried in the air.

It is usual to speak of the contagion of some diseases, *e.g.* of typhoid and cholera, as being especially conveyed in fluids; whilst others, *e.g.* whooping-cough and influenza, are spoken of as being especially air-borne, carried by the breath or from exhalations, and diffused in the air.

It may be that there are some contagia which can only be conveyed by direct inoculation, though the number must be extremely small. Apart from these, there are few, if any, known contagia which cannot, under certain conditions, accumulate in dust, or be carried as floating particles in the air. And there are very few of which we do not know definitely that this is the case. We speak of sewage "emanations," of "exhalations," "effluvia," and so on, in relation to infection. The old idea of the gaseous nature of contagia is, I suppose, extinct. What we really mean is that the contagious particles are carried in the air or in gases.

There is abundant evidence that even in cholera and typhoid, in which the disease is usually spread through water or milk, it can be directly taken by inhaling such "effluvia."

As regards cholera, there are the observed facts of the far greater incidence of the disease in soldiers and others using particular latrines, who in other respects, as to water supply, etc., were under identical conditions with their fellows.

As to typhoid, there are numerous instances on record where it has been acquired either by direct inhalation of air from typhoid sewage, or indirectly by the contamination of water or milk by like exhalations from typhoid excreta.

We speak of scarlet fever as being conveyed by milk. But have you considered how this infection is carried to the milk? It may be by direct contamination from a case which is still infectious; but certainly in some cases it is because particles of desquamated epithelium, carried as dust in the air, lodge in the milk, and the contagium multiplies there. I think there cannot be the slightest doubt that it is in this way that outbreaks of diphtheria, typhoid, and scarlet fever sometimes occur.

There is another very elementary fact to which I may call your attention. We have a sort of idea that contagia must be borne as single naked particles, each bacterium separate, just as we might separate the dry spores of anthrax or other bacilli, and diffuse them in the air. This idea is undoubtedly erroneous.

Have you ever considered what is the size of common dust particles? I would advise you to try a little experiment. Make a thin smear of blood on a dry slide, to serve as a rough micrometer, leave the slide out for half an hour in your room, especially if the housemaid has been at work there, and then look at it under a microscope. You will find that the particles of dust which we speak of as being so very minute may, even when of some tolerably heavy substance, be large enough to be visible to the naked eye. They may be even as much as $\frac{1}{25}$ or $\frac{1}{50}$ in. Many of them may be $\frac{1}{100}$ in. in diameter. If you make a little calculation, based on your knowledge of the size of bacteria—take any of the common ones—you will find that in a particle of dust which is only $\frac{1}{100}$ in. in diameter you may have a million of some of the larger forms of infectious bacteria, or fifteen millions of some of the smaller. So that we are not dealing with such an infinitely minute quantity, judged from a bacteriological point of view. You will see that this would allow a large number to be embedded in a particle, and, whilst those on the surface might be injured by drying or in other ways, those which are in the centre of a mass of cells might remain there for a considerable time unaltered.

Let me remind you also of the extreme minuteness of the dose of some known contagia when experimentally intensified. A thousandth of a milligramme of some of the more virulent will kill some animals of small size. This allows, you will see, doses which are relatively enormous to be inhaled or swallowed. For instance, if you stay for five minutes in a room where you have a case of measles or whooping-cough, you may inhale enormous numbers of particles containing the contagium.

And I may point out that it is not the pure bacteria alone, such as we may get in cultures, which are inhaled, but that with them you may have in the particles substances which aid their lodgment, or increase their virulence.

I have no doubt that some of you will say that many contagious organisms will die outside the body, if left long enough in conditions in which they cannot grow, or that they will be killed by the bacteria of decomposition, or other agents. But I may point out that there are a number of contagia known to us which can exist for a very long time in dust. In smallpox, scarlet fever, diphtheria, tubercle, and anthrax, we have evidence that even in the less resistant forms they may, if dried, remain for months, or even years, alive, and capable of cultivation. Dried cultures of some of these may be virulent after many months. Cases have been known where contagion of scarlet fever was active after a lapse of eighteen months, and probably much longer. The spores of anthrax bacilli can, as we know from observation in wool-sorters' disease, remain virulent for two years or more.

And, further, we know that outside the body there may be conditions which intensify virulence, or which allow of actual growth of the contagium. Thus exposure of the excreta of typhoid and cholera to the air for a short time appears to increase the virulence of their contagia.

As regards tubercle, the observations of Dr. Ransome appear conclusive of the capacity for growth of tubercle bacilli on damp wall-paper, etc., especially if moistened by breath or by organic matter from excreta. And the recurrence of diphtheria in the same rooms, after long periods, points to a similar possibility, though it may be from the presence of dried bacilli. Indeed, the great capacity for growth of many pathogenic and pyogenic bacteria in suitable nutrient media suggests their *possible* persistence, under special conditions, to a far larger degree than we are generally inclined to allow.

It is, therefore, by far the safest plan to treat all contagia as being either actual or potential dust. If you leave discharges from the nose or mouth in diphtheria, or the urine or fæces in typhoid or cholera, on clothes or bedding, etc., and if they remain until they become sufficiently dry for particles to be detached by shaking or handling the articles, these fibres, hairs, etc., carrying the contagium, may be diffused as dust, even at a much later period. And this, of course, leads to the very important conclusion, that you should deal with the contagium *at once.*

But you may further urge that the dust is dry, and that, although some can withstand long drying, yet drying and exposure will kill most contagious organisms.

But is dust really dry? "Dry-as-dust" is proverbial. But in a damp climate, or during the rainy season in tropical climates, or where there is rapid evaporation from the ground at some periods of the day, dust is anything but dry.

I suppose I need hardly tell you that you may get dust, damp and grimy, adhering to your books and papers. And no one will doubt that in any atmosphere where persons are congregated, watery vapour from the breath is sufficient to keep up a considerable amount of moisture in the air.

If you are acquainted with the beautiful experiments of Mr. John Aitken on the effects of dust in causing condensation of moisture in the air, you will recollect that you may have any degree of saturation with watery vapour, so long as the air is dust-free. But, by the introduction of dust particles you get a shower, and you can estimate the number of dust particles in the air by the drops of water which fall on a plate. In other words, dust, so to speak, attracts water from the air; the water tends to accumulate round these dust particles, and therefore dust need not be dry.

Have you ever considered the *physics of dust?* It is a subject of great importance in relation both to the spread of infection and to cleaning and disinfection.

Dust, or "floating matter in the air," as Tyndall called it, tends, as you know, mostly to fall, and in cultivating bacteria in Petri dishes we make use of this fact. You may keep a cultivation absolutely sterile so long as dust is prevented from falling upon or being carried into it, by a glass cover which overlaps sufficiently. Of course the particles which fall most readily will be the heavier, such as those of stone and coal, whilst many of the more minute and lighter particles, such as of cotton, feather, etc., will float far more readily, and fall far more slowly. Such particles are necessarily carried about by the movements of the air.

We do not always remember that the air is never still. If you shut up a drawer or cupboard it is almost impossible to exclude dust from it. And the reason is, that every time the temperature rises or falls, the air moves out or in. Take a cupboard, for instance. When the temperature of the room rises, a certain quantity of air passes out of the cupboard; when it falls, the air is drawn in again, and with it necessarily particles of dust. The currents will be much greater in parts of the room where there are inlets or outlets, such as ventilators, or where the temperature varies most, *e.g.*, where you have windows exposed to rapid cooling and heating. In fact, the only hope of getting perfectly still air would be in a closed chamber where the atmosphere was of constant temperature and constant humidity. Therefore, dust passes *into* all drawers and cupboards. And it also passes *out*, not to an equal extent, doubtless, but it can pass out, or if not it may accumulate and be subsequently dislodged. So that any accumulation of dust may contain, if it has been exposed to it, either the contagium of a specific infection or pyogenic organisms or tubercle, and be a possible source of infection later.

If you look into this subject, you will find that there are certain places, such as the chinks in the floor and various other positions, in which dust especially tends to accumulate, and from which the common processes of so-called dusting and cleaning often stir it up and distribute it about in the atmosphere. Ventilators, also, are often agents for the accumulation of dust. And, remember, that the lighter the particles the greater will be their tendency to lodge in elevated and inaccessible places.

Turning now from these elementary matters, which I daresay many of you think trivial, but which are none the less of great practical importance, let us consider by what measures you may deal with any contagium in the light of known facts of bacteriology.

1. You may kill the contagium outright or totally remove it—that is *disinfection*, though this term usually means the killing only.

2. You may diminish its vitality or virulence, *e.g.* by exposing it to sunlight, which is one of the most powerful agents. Or you may induce such conditions as lessen its power of active germination and of disease-production by means of various chemical and physical agents—that is, in one sense, *antisepsis.*

3. You may *dilute* it to such an extent that the dose available will be insufficient under ordinary conditions to produce infection.

This is one of the most important effects of free ventilation in diminishing infection. We know that a certain dose of the virus is necessary in order to communicate contagion. We find in bacteriological experiments, that by diminishing and graduating the quantity, you can reduce the dose even of a highly virulent contagium to a point at which it is not lethal, and may be almost inert. When dealing with any case, therefore, one very important point (when you cannot otherwise diminish the risk) is to reduce the quantity in a given amount of air breathed below the point at which it is dangerous.

4. You may remove the susceptible person from its influence, or check the carriage of the contagium.

5. Or you may disregard the contagium altogether, and endeavour to antagonise it by increasing the normal power of resistance, or by introducing drugs which will antagonise it.

6. You may use preventive or protective inoculation, or antitoxic sera.

It is right to use all these means, adapting your measures to known facts in relation to each contagium.

And you will, of course, exclude the possibility of added infection, by sterilising milk and water.

Now, first, as regards infection through dust, how are we to deal with the dust, whether diffused in the atmosphere or in what I may call the potential form, on clothing or bedding or other articles?

It is questionable whether continuous moistening alone is a safe thing, unless you know exactly what contagion you are dealing with; because, if you moisten the dust and cause it to adhere to substances, it may not be so easily removed, and may possibly remain active or grow. You may remember that Koch especially insisted that in dealing with cholera it was not the safest thing to use much moisture, because the spirillum (or "comma bacillus") can remain actively living and multiplying when damp.

But, in order to apply any method of disinfection thoroughly, you must *moisten* the dust. There are almost no disinfectants which are active with dry bacteria. This is true of nearly all modes by which we can deal with them.

Well, then, first try to deal with contagion by free ventilation. Whilst doing this, a precaution too often neglected is to catch the dust particles, so that they may not pass out of the infected room into others. Hanging a sheet which is kept constantly moistened with some disinfectant—if need be, some kind of gauze, or else an ordinary sheet, kept moist with an active disinfectant, such as carbolic acid solution—will inevitably catch a large portion of the dust, and prevent it from going out of the room. The use of gauze curtains in the accesses to wards and other places where there are cases of infectious disease, is far too much neglected. It is one of the things overlooked, because people have not regarded infection as subject to common physical laws, and as carried by floating matter in the air.

Then, do not let dust accumulate, especially such as can retain the contagium. Hence the importance of constant attention to cleanliness in hospital wards. But do not allow dust to be stirred up by the common modes of "dusting," which, as you know, distribute the particles all over your papers and books. Keep it, at any rate, moist, if possible, during removal. You may use very simple means for this end.

Of course the most important thing is to check the contagion at its source, that is to say, to deal with it by dealing with the patient, who is the centre, the focus of the contagion. You can do this to a large extent by constant cleanliness, change of linen, and so on. But you must fix the contagium at once by moisture. It is safer to put all towels, clothes, bed linen, etc., into a tub of cold water at once than to leave them in a corner, waiting to be carried away and disinfected. I believe if this were constantly done, if at once all infected or possibly infected clothes were put into water, we should have far less spread of contagious diseases. The water ought, if possible, to contain some antiseptic, and boiling water is far better than cold.

But you must always remember that you have first of all to consider the comfort and life of the patient, and you cannot always entirely stop the contagion at its source. For instance, you cannot entirely check the carriage of contagion through the breath, though local disinfectant treatment may do much.

You will save much trouble if, as I said before, you attend to the matter of the possible necessity for disinfection *at once* when you are called to a case of infectious disease, and if you regulate the surroundings of the patient in view of this.

It is curious how these are neglected, *e.g.* the elementary details as to beds, bedding, and curtains.

As regards bedding, some kinds are peculiarly difficult to keep clean and to disinfect. Such things as straw palliasses, unless constantly renewed and burned, are to be especially avoided, unless you can insure constant purity of the straw, and burning it afterwards. If you had seen, as I saw not long ago, three straw mattresses in close contact with one another being disinfected by burning a little sulphur twice a day in their proximity, you would understand the necessity of attending to this matter.

It is far easier to prevent accumulation and spread of contagion if you begin early. It is much easier to *prevent* infection than to *dis*infect. Therefore, you should remove all needless articles from the room. Of course you know this, but one is astonished at the articles which are sometimes considered necessary in the sickroom, such as pictures, curtains, rugs, velvet mantelboards, and so on—things especially adapted to catch dust and difficult to disinfect.

Coming back to the subject of disinfection, let me remind you of some good old rules: "Everything which may abide the fire, ye shall cause it to pass through the fire," *i.e.*, if it is necessary. What can be disinfected by heat should be, whether by steaming or boiling. Where this is not applicable, soaking in disinfectant solutions must be employed.

In regard both to heat and disinfectants, do not forget the element of *time.*

A certain length of time is necessary to insure thorough disinfection. I have seen highly infectious fæcal matter rapidly mixed with corrosive sublimate solution and at once washed away by water, thus insuring that a large part of the bacteria were never touched by the disinfectant. And it is known that, in the case of tubercle bacilli in sputa, such measures may actually serve to retain the infection.

Of the disinfection of rooms and furniture I have already spoken. I know that some eminent authorities consider that it is impossible to do it thoroughly, and practically disregard it. And I agree that it is better to trust to ordinary cleaning and ventilation, if disinfection cannot be done efficiently.

I have been told by eminent authorities that it is too troublesome, that it requires care and thought, and is expensive.

I know nothing worth doing which does not require care and thought, even the cooking of a dinner. And, as regards expense, the cost of disinfection is trivial compared with that of a prolonged illness, the cost of nurses, etc., or even that readily spent for one dinner party.

In applying disinfectants, it is well to fix the dust by moisture. You will find that a spray charged with some disinfectant is a specially valuable method, and one now largely adopted in many places. The spray in the air carries down a large part of the dust, and covers the surface of the walls and floor with a coat of disinfectant. But I am doubtful whether any such spray alone —*i.e.* without subsequent cleansing and local disinfection—can be regarded as efficient.

As to the disinfectants to be used, it should not be necessary for me to give any details, as their action is well known, and should have been a part of your studies.[1]

It does not matter what disinfectant you use, if it is not too costly to get, or dangerous to use, and if it is practically active. Carbolic acid, corrosive sublimate, formalin, and so on, may be used as spray. There are apparent objections to corrosive sublimate. But it is an interesting fact that, in its use on a large scale in disinfecting rooms occupied by plague patients in India, it has been found that those who were constantly employed in carrying out the disinfection did not suffer, as might have been anticipated, from mercurial poisoning. Formalin has recently come into use, and it is said that its application, with the subsequent use of ammonia, is efficient and not offensive. But I am doubtful whether it has yet been sufficiently tested on a large scale.

All one would ask is, that whatever is used should be scientifically tested and shown to be active for the purpose for which it is employed, and under the conditions in which it can be used.

Besides this, you should be prepared to give instruction to others, to insure thoroughness of cleansing.

There are certain places where dust can especially accumulate, which are very commonly overlooked, such as the insides of drawers, tops of wardrobes, the backs and frames of chests of drawers, and the inside of shutters. I have often seen in rooms where disinfection and subsequent cleaning were supposed to have been carried out, that on opening the shutters the inside of the framework and the back of the shutters had never been touched. The backs of skirting boards, especially near the bed, where very often there are large accumulations of dust; mantel boards, the woodwork of water-closets and of parts containing cisterns; cupboards which have been locked, and are therefore supposed to be harmless, and especially ventilators also require attention. You may say that all these details should not occupy a highly educated gentleman in a respectable position. But I may point out that it is by attending to such small things that success in antiseptic surgery has been gained. If Lister had not condescended to such details as the treating of gauzes and plasters, we should not have had our present antiseptic surgery. You must condescend even to such humble matters, and, what is more, if you can teach a few able-bodied charwomen the necessity of these things, and train them to carry them out, you will do far more than a great number of sanitary inspectors and sanitary staffs.

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[1] With regard to sulphur fumigation, a Memorandum issued by the Local Government Board of England in 1892 (referred to on p. 386, note) may seem to give some authoritative support to its use. I am authorised by the Medical Officer of the Local Government Board to state that the Memorandum was "only the reissue of the pre-existing Memorandum, and that there had been no revision as regards disinfection for some years before that date."

Let me conclude by throwing out a suggestion. It would save much trouble and worry to busy medical men, if for each important infectious disease there were a separate code of directions—a definite simple statement of the more important facts and necessary precautions which could be given to the friends of the patient.

You will find some such directions in one or two books, and amongst the best directions of the kind are some privately drawn up by associations of the medical officers of schools. But I know of no sufficiently simple, recent, and condensed statement of the kind which is easily accessible.

I believe that no better work could be done by any sanitary authority than the issue of such a code to medical men, together with slips for distribution to their patients. Nor do I see why, on notification of a case of any infectious disease, the sanitary authority shall not send to the householder a slip containing directions applicable to that particular disease.

Until this is done, I would advise you, whilst your knowledge is fresh, to draw up such a statement for yourselves, and to keep it by you for reference.

The statement should include—

1. The common mode of infection.
2. The period of incubation, to the time of occurrence of the rash or other leading phenomenon.
3. The common and the possible modes of diffusion of the contagium, whether by the breath, sputa, fæces, etc., and how best to avoid and to prevent infection.
4. The capacity for existence of the contagium outside the body; the duration of infectiousness, and the conditions which may prolong its period.
5. The known facts as to the agents which most readily destroy the contagium.
6. Simple directions as to the points to be attended to in cleaning, dusting, dealing with clothes, and so on.
7. A careful statement of what can be done with various materials and common articles by way of disinfection, *e.g.*, what injures certain fabrics and what does not.

These matters may seem to you now to be trivial and elementary, to be commonly known or easily acquired. It may be so; but extensive inquiry has convinced me that attention to such simple matters would not only save much needless trouble to the medical practitioner, but would do much to check the spread of infectious disease.

CLINICAL RECORDS.

TWO CASES OF HYDATIDIFORM MOLE.

By JAMES OLIVER, M.D., F.R.S.E., F.L.S., *Physician to the Hospital for Women, Soho Square, London.*

CASE 1.—Ellen G., æt. 47, and married twenty-five years, has had ten children, but never a miscarry. The last child was born four years ago. During the last six months there has been daily a slight hæmorrhagic discharge from the vagina, but prior to this there had been complete amenorrhœa for five months. During the last four months she has complained frequently of pain all over the abdomen; sometimes it has been very severe, but it has never been like that of labour. The hæmorrhagic discharge, which has never been excessive, has always been liquid, and no pieces of membrane have ever been passed. There never has been any bladder symptom.

Physical signs.—The abdomen generally is slightly prominent. Palpation detects in its lower half a small softish and somewhat cystic swelling, located centrally, arising out of the pelvis and extending to within an inch of the umbilicus. Fluctuation cannot be elicited, and the tumour does not appear to contain any distinctly solid body. The cervix uteri occupies a fairly central position in the pelvis; it is soft, and the os is slightly open. The vaginal roof in front and to the right of the cervix is pushed down a little by a swelling which is continuous with the abdominal swelling. The body of the uterus, as such, cannot be differentiated, and the abdomino-pelvic tumour does not appear to be the uterus enlarged, as the tumour and the cervix uteri are evidently not structurally continuous. No sounds are heard on auscultating the tumour. The breasts present characteristic appearances of pregnancy, and colostrum can be obtained from them. The temperature is normal, and the general condition of the patient is good. After having been under observation a few days, labour pains were experienced, and within two hours after the starting of these a most typical example of hydatidiform degeneration of the whole chorionic structure was expelled. When the interior of the uterus was explored by the finger, curettage was deemed expedient, as some small portions were found adherent towards the left half of the fundus. The specimen weighed 3 lb. Convalescence was completed without an untoward symptom.

REMARKS.—The above is altogether a most remarkable case. The patient, who is 47 years of age, had given birth to ten full-time children, and had never had a miscarry. When the physical signs had been duly scrutinised, it was doubtful whether one should consider the five months' amenorrhœa as an indication of the climacteric or as an actual concomitant of the present derangement. A constant hæmorrhagic discharge from the vagina is occasionally *the symptom* which causes a patient to seek advice when an ovarian cyst develops after the menopause. The cervix uteri was soft, and the breasts were characteristic of pregnancy, but these were the only signs of this physiological state. The tumour did not appear to be the enlarged uterus, and nothing like a fœtus of a size commensurate with the clinical history and size of the abdomino-pelvic tumour was ever detected by palpation. The degeneration of the chorionic villi must have begun before the sixth week, as the placenta begins to be differentiated about this time. For five months there was complete amenorrhœa, and the pathological product of conception was retained *in utero* eleven months.

CASE 2.—Annie T., æt. 25, and married seven years, has had five children, but never a miscarry. The last child was born two years ago. For eleven weeks now there has been constantly a hæmorrhagic discharge from the vagina. It has never been excessive, but prior to its appearance three menstrual periods had been missed. During the six weeks preceding the onset of the constant hæmorrhagic discharge, patient had observed that the occurrence of sexual intercourse always induced a slight hæmorrhage from the vagina. For eight weeks she has complained more or less of abdominal pain, but this has never been like that of "labour." Sickness was not noted until the constant hæmorrhagic discharge made its appearance, but it has been a somewhat troublesome symptom ever since.

Physical signs.—The lower abdomen is rather prominent. It is occupied centrally by a globular swelling, which arises from the pelvis and extends almost to the umbilicus. This swelling is everywhere tender to the touch. The cervix uteri is very soft, and the abdominal tumour is the enlarged uterus. No sounds are heard on auscultating the tumour. The breasts are flabby, and no fluid can be squeezed from them. After being under observation two days, labour pains set in, and a typical hydatidiform mole was expelled piecemeal.

Eleven months after this event conception again took place. No anomalous symptom was noted during this pregnancy. The fœtus arrived at maturity, but died a few days after its birth.

REMARKS.—In this case the pathological product of conception was carried six months, and the size of the uterus was rather less than it should have been had the fœtus been alive. My experience of hydatidiform mole does not lead me to expect a uterus of a size greater than that of the supposed period of pregnancy. More or less abdominal pain is usually noted in association with this disorder. The feeling of the uterus may or may not be of diagnostic worth.

REVIEWS OF
BRITISH AND FOREIGN LITERATURE.

Atlas of External Diseases of the Eye. By MAITLAND RAMSAY, M.D., M.B., C.M., Ophthalmic Surgeon, Royal Infirmary, Glasgow. Glasgow: James Maclehose & Sons. 1898.

WE have had many atlases of the ophthalmoscopic diseases of the eye. Of late years drawings of the external diseases have been confined to text-books. Even there they have rarely appeared in such number as to be representative. In old atlases, again, the drawing, and especially the colouring, has left much to be desired. A really good modern atlas of the external diseases of the eye seems, therefore, to supply a want; all the more that, however interesting ophthalmoscopic changes may be, they can rarely be looked upon as of much practical importance to the general practitioner. They are relatively of infrequent occurrence, and even should they be diagnosed it is comparatively seldom that any treatment is likely to do much good. It is otherwise with the external diseases. They are constantly met with, and great harm may result from neglect or improper treatment.

Dr. Maitland Ramsay is to be congratulated on the completion of a work which must have given him great trouble to produce. A more beautiful and complete collection of coloured and uncoloured pictures of eye diseases does not exist. The process of photography which has been followed seems admirably adapted to give a representation which, even on the small scale of the size of the natural eye, is true to nature. Many of the photographs have been coloured. This has been done, too, with an absence generally of the usual exaggeration. By adopting the photograph as a basis, the proper size and prominence of every pathological change is assured. Apart from the judicious selection of representative types of disease, the author has thus only had to devote his attention to the colouring. This task he has performed in a manner which bears the stamp of conscientiousness throughout. As far as mere pictures go, the numerous drawings all show the conditions they are intended to depict, as well as could possibly be done. The only possible improvement one could suggest, would be the substitution of stereoscopic pictures of the same quality of excellence. But this might possibly prove impracticable in so exhaustive an atlas.

Dr. Ramsay has given an accompanying text to each plate. This text is mainly intended as a *résumé* of the characteristics of the various types of disease which are represented. In a few instances the actual case depicted is described. The various diseases illustrated are properly named; and wherever the nomenclature adopted by different writers varies, the several synonyms are also given. The etymology is also always stated. This is a good plan, as no doubt it helps to associate the disease in the mind of the student with its often rather cumbersome name. The corresponding names in French, German, and Italian are also noted. The necessity for this is perhaps not so apparent. Anyone who has sufficient knowledge of these languages, to benefit from reading medical works in them, would have little difficulty in recognising the names by which the diseases are known.

With regard to the text, it may be said that on the whole it is worthy of the pictures. There is nothing particularly novel in the descriptions given. Nor could there well be. It is sufficient that they should be true to nature, and that they serve admirably to draw attention to the points which call for observation. The book is altogether so good that it seems almost a pity that it did not lie in the plan of publication to add some slight indication of treatment, where any treatment is called for. It is usually a somewhat thankless task to devote so much time and labour on the production of an atlas as must have been required in the case of Dr. Ramsay's work. Where everything, however, is so satisfactory, and here it may be said that the publishers have succeeded admirably in all respects, there should be little fear that the author's efforts will be appreciated by the medical public, and that the atlas will prove of great value to the busy general practitioner.

G. A. Berry.

General Physiology: An Outline of the Science of Life. By Max Verworn, M.D., Ph.D., Professor of Physiology, Jena. Translated by Frederic S. Lee, Ph.D., Columbia University. London: Macmillan & Co. Ltd. 1899.

Professor Verworn's "Allgemeine Physiologie" is already well known to English physiologists as one of the most suggestive books ever written on that fascinating subject—Cell Physiology.

This is a translation from the second German edition, and one cannot but admire the accuracy and well-balanced judgment in selection of terms that characterise Professor Lee's work. Verworn's German is not the most easy to clothe in English dress, and yet it is only very occasionally that even a critical reader would wish a word or phrase altered. One dare hardly speak of the scope of such a work. It has practically no boundaries. Anatomy and physiology (human and comparative), physics, chemistry and philosophy, are all appealed to, in order that the treatment may not lack in breadth. The wealth of detail, especially in respect to elementary vital phenomena in the lower forms of animal life, is perhaps the most outstanding feature in the work. It is not a book that can be used as a guide to any student going up for examinations, the bewildering range of subjects prevents that; but it is one that should be carefully studied by anyone interested in the general problems of life, which, after all, can simply be discovered and understood after we have arrived at a fuller knowledge of the cell life.

The book is divided up into six chapters, the first one giving a general account of the aims and methods of physiological research. This is almost entirely historical, dealing with the development of modern physiology, from the period of Hippocrates to the present day. Living, as distinguished from lifeless, substance is treated of in the second chapter, both the morphological and the chemical composition of the cell being taken up. In such a chapter as this one, where individual selection must be exercised with care, many readers will wish that some part or other had been more fully treated at the expense of another portion. But the average reader could wish for nothing better. A clear exposition of the most modern views is given, and subjects are made to appear interesting which are apt to be regarded as the dry bones of cell physiology. One of the most interesting and original chapters in the book is that on elementary vital phenomena. He must be dull indeed whose interest flags while reading about the different ways in which energy may be introduced into the organism, and the different transformations of energy in the cell life. In such a work as this it is impossible to avoid some repetition, and that one meets with here and there in this chapter. It is a pity, however, that there is such a useless repetition of figures. For example, such figures as 118, 187, 261, and some others, are given elsewhere when their reappearance is quite unnecessary. Chapter v., on "Stimuli and their Actions," is perhaps the best in the book. Professor Verworn is a great authority on the phenomena of excitation and depression of the cell, and he has given a most fascinating description of the whole subject. The last chapter on "The Mechanism of Life" is almost as good as its forerunner, although here there is a more marked tendency to repeat what has been given previously. Perhaps, after all, this repetition only strikes one more after reading the other chapters; and it is a compliment to any author, when the reader has been so impressed by what has been given in the earlier part of the book, that a recurrence to anything that has been discussed before finds the subject still fresh in his memory.

Such a book as this, however, is worthy only of the warmest praise, and certainly carping criticism would be quite out of place. It is a book that compels the reader to think for himself. It would be impossible to find a better guide to lead one through the intricacies of the problems of the cell life.

T. H. MILROY.

Masters of Medicine—John Hunter, by STEPHEN PAGET; *William Harvey*, by D'ARCY POWER; *William Stokes*, by Sir WILLIAM STOKES; and *Sir Benjamin Brodie, Bart.*, by TIMOTHY HOLMES. London: T. FISHER UNWIN. 1897 and 1898.

IN some of the Continental schools there still exist chairs set apart for the teaching of medical history. It is to be regretted that the British medical curriculum is so full and varied, and the student's time so occupied, that there is no opportunity for his attending a course of lectures on a subject which could not fail to be as stimulating as it would undoubtedly be interesting and instructive. It is, however, gratifying to find such a series of works as those under consideration making their appearance.

The history of medicine is naturally intimately bound up with the lives of those men who have devoted themselves to the furtherance of medical and surgical science, have achieved distinction in their respective branches of a noble profession, and whose names will always be held in honour and reverence, while not a few have earned the right to be classed amongst the immortals.

"The immortal memory of WILLIAM HARVEY" has been treasured in many books, but a work such as that before us certainly fills a long-felt want. It is concise, well written, and appeals as much to the layman as to his professional brother. The great discoverer is brought vividly before us as a courteous and kindly, if somewhat irascible, little Englishman—energetic, ardent, and painstaking, with "the power innate in every great teacher of attaching to himself and enrolling in his work all sorts of unlikely people." But the book is much more than a mere account of Harvey's life and work. Graphic and quaint pictures are given of the old Lumleian lectures, and we are introduced to many notable physicians of the period. Here and there we have suitable allusions to the great struggle between King and Commonwealth with which Harvey was so intimately associated. The book is conveniently divided into periods, the most interesting perhaps being "The Zenith," in which we are favoured with Harvey's description of the Bass Rock and its noisy inhabitants, a description which might have been written at the present day, so faithfully does it picture the little-altered home of the solan goose.

In connection with the account of Charles' tour in Scotland, we would point out that it was the Dean and not the Bishop of St. Giles who was favoured by Jenny Geddes', and not, as stated, by Jenny Deans', kind attentions. This, however, by the way. Harvey's later years are well described, and his character summed up in a few well-written pages, in which he is compared with John Hunter, who did for pathology and surgery what Harvey did for medicine. The end of the book is devoted to a consideration of the great man's works on anatomy and his treatise on development, and it will be read with deep interest by all who have studied these kindred subjects. A fine portrait adorns the volume, which also contains a reproduction from a photograph of Harvey's *stemma*, or symbolic monument, discovered in the Jurist University of Padua, where Harvey formed one of the council of the English nation at the time he prosecuted his early studies in the great Italian school of the Middle Ages.

The first of the series is perhaps the most interesting of those which have appeared, and that even though it consists largely of JOHN HUNTER's letters to his friends and relatives. But such letters, especially those to his friend and pupil the great Jenner, at once give us an idea of this extraordinary man, who was at the same time surgeon, anatomist, zoologist, pathologist, physicist, and who threw fresh light on every branch of science he took up. They reveal his vehement energy and his insatiable "greed" for museum specimens, not always obtained in what would now be considered a legitimate manner. From their perusal we see him as he was—a man of rough exterior but warm-hearted, a mighty pioneer even in his last days, when ill health and trouble crowded upon him. It may be that the book is a little confused in its arrangement, but this is unavoidable in a work of its class, and no one can deny that every part of it is likely to attract the general reader as well as the man of science. It holds the attention from first to last, and not only brings John Hunter and his famous brother before us, but has about it an atmosphere of the days in which they lived, of London in the latter half of the eighteenth century, and of the study of anatomy in those times. How significant is this quotation from Jesse Foot: "Within these last ten years, five lecturers in anatomy are now fresh upon my memory, who have fallen the victims of putrid myasma in the prime of life."

And yet John Hunter, who scarcely ever took a real holiday, who dissected more bodies than any man then living, who spent his spare time in countless experiments on animals and plants, who had to practise (for, as he wrote to Lynn, "I must go and earn this damned guinea, or I shall be sure to want it to-morrow"), who lectured and wrote, rose early and went to bed late,—this sturdy Scotsman managed to live to the age of sixty-five, and left behind him a collection of priceless value, discoveries of all kinds, and a fame which a century has not dimmed. The history of such a master cannot fail to find fresh students when presented in so admirable a form. Sharp's engraving of the famous portrait by Sir Joshua Reynolds forms the frontispiece, and a suitable introduction is supplied by Sir James Paget.

The remaining two biographies, though not so engrossingly interesting, are yet well worth reading, especially that of the famous Irish physician, WILLIAM STOKES. Here, indeed, there is portrayed a perfect gentleman, a genial Christian doctor, of a type happily common in the profession. Edinburgh readers will peruse with interest the brief account of his early days, and his meeting in the Cowgate slums with the benevolent Alison, a teacher now forgotten in large measure, but whose memory was cherished by an older generation. Stokes had very high ideals and well did he live up to them, while he strove to impress his tenets upon his students. A touch of Irish humour pervades the book, which, let it suffice to say, is worthy of its distinguished author, who has been singularly happy in the grouping of his chapters and in the quotations from the physician's great works on the "Chest" and on the "Diseases of the Heart and Aorta."

Sir BENJAMIN BRODIE was a surgeon, a philosopher, and a psychologist. His career forms an instructive study of how a careful, persevering worker may rise to the top of his profession. It will, we fancy, be more read by the medical than the layman, and especially by those who have been connected with St. George's Hospital. There is, however, a hitherto unpublished account of the death of George IV., who met his end more calmly than might have been expected, while the many-sided activity of Brodie's life is sufficient to supply his biographer with material which he has woven into several chapters that will amply repay the time spent upon them. ANDREW BALFOUR.

Traitement des Dermatoses par la petite Chirurgie et les agents Physiques. Leçons faites à l'Hopital Broca-Pascal. L. Brocq, Médecin de l'Hopital Broca-Pascal. Recueillies par le Dr. H. Déhu, et revues par l'auteur. Paris: Georges Carré et C. Naud. 1898.

One of the best proofs of the strides which dermatology has made in the last decade is afforded by this volume. That it is possible to devote a closely printed treatise of 276 pages to the sole consideration of the minor surgical and other local procedures connected with this branch of medicine, must appear somewhat remarkable. Yet, if the methods are described in full detail, there is no padding, so to speak; it is from the fulness of his knowledge that the author draws; and a perusal conveys the impression that only from extensive experience could it have been written. Starting with local anæsthesia, all the plans for procuring this which have been suggested are analysed, their value estimated, and under what circumstances each should be employed, defined. Then scraping is taken up, the instruments required described and figured, how such are used, the accidents which accompany it indicated, and finally the diseases in which it is of service are detailed. Scarification—perhaps more extensively resorted to in France than in this country—follows, and an account of its history and mode of action is prefaced, ere a similar scheme of handling this operative measure is pursued. It is in the ulcerating form of lupus that it finds its happiest application, in saving islets of healthy tissue still existing, which would be ruthlessly swept away by the spoon, or more or less injured by caustics. Though it sometimes gives good results in lupus erythematosus, it is much less generally efficacious. In keloid it does more in arresting progressive advance, and in lessening bulk, than in actually curing. The same system of explaining the use and expediency of the thermo- and galvano-cautery is adopted. A second part of the work is devoted to the employment of electricity, a large space being allotted to electrolysis for the destruction of disfiguring hairs, of which Brocq has a wide and successful experience. He still adheres to the plan whereby the patient makes and breaks the circuit. This certainly avoids shock, but in other respects is inferior to that in which the control is in the hands of the operator. To our mind the small disadvantages are outweighed by the gain in time and accuracy. The question of the cosmetic importance of the operation at different periods of life is considered, and, while agreeing in the main with the author, we think that perhaps the feelings of females above 45, as regards personal appearance, are hardly sufficiently allowed for. We were under the impression that to a Frenchwoman, at least, the effort to preserve beauty only ended with life. In the treatment of tuberous angiomata Brocq recommends the employment of the positive pole as the active one, since he does not, as a rule, introduce both needles into the nævus. In flat capillary nævi, on the contrary, he uses both poles, but he admits that the results in such are not brilliant. Other uses of electricity, as the faradic current, Franklinisation, currents of high intensity and frequency, and the X-rays, are referred to at greater or less length, and the volume terminates with a brief account of the application of light and hot air. There is no work at present available which covers exactly the same ground, and it will therefore be found of great value to any one who desires to make himself acquainted with the best methods as described by a practised hand.

W. ALLAN JAMIESON.

The Essentials of Chemical Physiology. By W. D. HALLIBURTON, M.D., F.R.S. London: Longmans, Green & Co. 1899.

A THIRD edition of this well-known small text-book has just appeared. It mainly deals with the subject from the practical standpoint, although the theoretical side has not been neglected. It is an excellent little book, kept well up to date, and giving, in the form of lessons, the more important qualitative tests and quantitative methods employed in physiological chemistry. The division into an elementary and advanced course is hardly to be recommended in such a small book, especially when one notices that in the advanced section a number of subjects are referred to, which might quite as easily have been included in the more elementary section. There are a few points that are perhaps hardly explained sufficiently, for example, the rationale of Trommer's test. From reading the description of the test, one would imagine that the solvent of the cupric hydrate was caustic potash, and not the glucose. The inclusion of the protamins in the elementary course is presumably due to the fact that a knowledge of these bodies is required by those who wish to know the most recent work on the subject of the proteids. If they are to be discussed at all, especially in an elementary book, it would have been as well to have given Kossel's classification of the proteids, starting with the protamins as the essential constituents in every albuminous substance.

After reading carefully this text-book, one cannot help thinking that it might perhaps have been better if it had been made entirely practical, with a fuller description of methods and tests than is given here. But the author has made good use of his knowledge of the literature of the subject, as well as of his experience as a teacher, with the result that this last edition of his "Essentials" gives as good an account of the main subjects in physiological chemistry as anyone could desire.

T. H. MILROY.

Atlas of Legal Medicine. By Dr. E. VON HOFMANN. Translated by Drs. FREDERICK PETERSON and KELLY. London: The Rebman Publishing Company. 1898.

THIS volume, which constitutes the seventeenth of the series of Hand Atlases issued by Lehmann, is in no way inferior to those which have preceded it, and the name of Hofmann alone is a sufficient guarantee that the subjects illustrated and described in the text will form a valuable addition to medico-legal literature.

The work has a melancholy interest, inasmuch as it was practically completed during the last few weeks of the life of him who perhaps has done more than anyone else to advance our knowledge of forensic medicine.

In the preface Hofmann states that the Atlas is intended to serve as a supplement to a good text-book, in which it is impossible to adequately illustrate the various subjects treated, and therefore he has limited the descriptions of the plates so far as possible, at the same time choosing those subjects for illustration not usually portrayed in text-books. There are fifty-six coloured plates and 193 reproductions of photographs, taken almost wholly from preparations in the unique museum of the Medico-Legal Institute of Vienna, and dealing with, among other subjects, deformities and abnormalities of the genital organs; various conditions of the hymen and os uteri; abortion; infanticide; injuries to the skull; homicidal and suicidal wounds; gunshot injuries; hanging and strangulation; and the appearances presented by the stomach and internal organs in various forms of poisoning.

That most of the illustrations are excellent reproductions of the original preparations, we can testify from personal experience; and so far as illustrations can take the place of examination of actual specimens, the Atlas, which is exceedingly moderate in price, will be an assistance in the study of the subject. It only remains to be said that the translators have, to use their own expression, "Englished" the text well, a task by no means easy with such an incisive and epigrammatic style of description as that possessed by Hofmann.

HARVEY LITTLEJOHN.

Unripe Cataract. By WILLIAM A. M'KEOWN, M.D. London: H. K. Lewis.

THERE is doubtless, as the author remarks, a disposition on the part of surgeons to operate at a much less advanced stage in the development of cataract than once was the case—a tendency certainly due to the vastly increased safety of the procedure since the introduction of antiseptic and aseptic methods. Dr. M'Keown's name has long been known in connection with the washing out of the anterior chamber, a point in the technique of extraction of the lens which he believes to be of particular value in cases of what he objects to being named unripe cataract. His diatribes against the term seem to us to have little reason in them, since no one supposes the expression to have any very precise scientific signification, but to be merely a convenient way of saying that the whole of the lens substance has not lost its transparency. If he is determined to be so severely logical, he should discard the word "opaque" in this connection, since a lens never becomes so, but only intransparent. His meaning is that, in his opinion and according to his practice, no lens, in which the process has gone on so far that business is seriously hindered, need be considered unsuitable for operation. In order to obtain more complete, immediate, and safe removal of the cortical matter, he is accustomed to employ injection into the capsule before extraction, and washing out afterwards; his carefully constructed and devised apparatus for this purpose is described at length, and may well prove of value in cases of injury to the eye, even apart from its use in cataract. One very important piece of advice he gives among numerous others, namely, never inject any mercurial lotion into the eye. The book is a record of patient and careful work, rewarded by gratifying success in its performance.

W. G. SYM.

Transactions of the Association of American Physicians. 13th Session, Vol. XIII. 1898.

THE present volume contains no fewer than thirty-two separate articles, and quite a large number of them is of exceptional interest and value. Space will not permit of more than a mere glance at a few of them. Flexner describes a case in which the lungs showed consolidation necrosis and early cavity formation, due to an organism which he carefully describes and figures, known as *Streptothrix pseudo-tuberculosa.* He gives particulars and a beautiful plate of a case of perforating syphilitic ulcer of the stomach, and discusses carefully this rare disease. Gorham describes a new pathogenic chromogenic bacillus, which came from some rags obtained at a paper-mill. It appears to be closely related to the *Bacillus pyocyaneus*, but differs from it in several points. It caused extensive inflammatory œdema at the seat of inoculation. Other five of the articles are upon other forms of bacteria, *e.g. B. icteroides*, *Micrococcus subnormalis*, etc. Four separate articles treat of the allied subjects of antitoxic serum in pneumonia, tuberculosis, etc. Adler contributes an able and complete paper on cardiac syphilis. Fussel and Jepson give particulars of two cases of acute leukæmia and an analysis of the cases hitherto published, namely, fifty-six in all. They give the duration at from three and a half to sixty-three days; the average being thirty-nine days. Thayer and Councilman both give an able article on the kidney, the former on nephritis of malarial origin, and the latter on acute interstitial nephritis. Other interesting articles are albumosuria in medical practice (Fitz) and jaundice in typhoid fever (Da Costa).

R. F. C. LEITH.

Kirkes' Handbook of Physiology. By W. D. HALLIBURTON, M.D. Fifteenth Edition. London: John Murray. 1899.

THE fifteenth edition of the well-known text-book has just appeared. It is essentially a student's book, embracing as it does all the departments of physiology, including histology. This general character of the book has always been its distinctive feature, and although such an arrangement, where histology is included, can hardly be recommended from the scientific point of view, still it has the advantage of presenting in small compass what is required of every candidate for an examination in physiology in this country.

It has been well brought up to date by the author. The most important additions that have been introduced in this edition are the electrotonic changes in conductivity, recent methods for estimating the output of the heart, effect of gravity on the circulation, measurement of the blood pressure by Hill's sphygmometer, and Langley's recent experiments on the vagus and cervical sympathetic. There are many other additions, such as a short description of Beer's interesting work on the mode of accommodation in the fishes, amphibians, etc. Many of those are taken from papers read at the last Physiological Congress, held at Cambridge in August 1898. In such a comparatively small book, covering so immense a field, there must naturally be omissions and insufficient description of some subjects. These, however, are comparatively rare. There are certain things, nevertheless, which ought to have been corrected. The diagram of the spectroscope (Fig. 352), as it is given, could not possibly be understood by anyone. It would have been quite as easy to have given an accurate drawing. The same is true of Fig. 565, showing the course of the rays of light through the eye. The spectroscopic examination of the blood pigments has been brought well up to date, but it is unfortunate that the absorption bands of the blood pigments are so roughly mapped out.

What impresses the reader most, however, is the thorough way in which the little book has been kept in line with modern research. When one bears in mind the comparatively small space at the author's disposal, one cannot help admiring the fair and equal treatment of each portion of the subject.

It is, on the whole, a most reliable text-book, and can be strongly recommended to all those who wish to have the subject presented to them in moderate compass.

T. H. Milroy.

RECENT ADVANCES IN MEDICAL SCIENCE.

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MEDICINE.

UNDER THE CHARGE OF

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Phthisis Pulmonum.

To satisfactorily refer to even a tenth of the literature which has recently appeared regarding phthisis pulmonum and tuberculosis generally, would require several scores of pages. Some notes, however, may be of value at present concerning the subject.

Aufrecht (referred to *Schmidt's Jahrb.*, Leipzig, 1899, S. 38) still regards phthisis, as he has often stated in earlier papers, as commencing with a chronic broncho-pneumonia, which permits of tubercle bacilli finding a lodging, not with a primary tubercular growth. He lays great stress upon the value of a healthy condition of the blood, and so gives iron freely. J. E. Squire (*Am. Journ. Med. Sc.*, Philadelphia, Nov. 1898) read a paper in 1894 before the Royal Medical and Chirurgical Society of London, upon the influence of heredity in phthisis, showing that, in 275 families, of 1745 children 24·87 per cent. became tubercular, although the parents were free from it; 33·71 per cent. of 1182 children of phthisical parents developed that disease, or about 9 per cent. more. His present paper deals with 1000 families and 6450 children. Among these the parents were free from phthisis in 673 cases, the father phthisical in 148, the mother in 135, and both parents in 44; that is 327 with a history of the presence of the disease in the parents. The following table gives some of the data afforded :—

One Thousand Families.		Average No. of Children.	Deaths in Infancy per Cent.	Phthisical percentage (Infantile) Deaths.
Parents non-phthisical .	673	6·66	11·63	23·65
Father phthisical . .	148	6·25	13·62	31·15
Mother ,, . .	135	6·11	9·44	32·75
Both ,, . .	44	4·95	12·84	43·15
Total of last three .	327	6·02	11·74	33·16

The difference between the total death-rates is a little less than 10 per cent., although when both parents were affected the proportion rises to about 20 per cent. The infantile deaths show no great differences, nor does the average number of children to each family, with the exception of those whose father and mother both were or became phthisical. The moral of the whole affair lies in the comparatively slight difference between the frequency with which phthisis occurs among the offspring of phthisical and non-phthisical parents, leading to the conclusion that it is largely an infective disease and not a disease induced to any great extent by hereditary taint.

Rudio-Jicinsky (*New York Med. Journ.*, 18th Feb. 1899) discusses the data from twenty cases in which investigation for pulmonary tuberculosis was made by means of the X-rays, used by himself or by his colleagues. In each case the diagnosis arrived at by the ordinary methods is given, along with that made from the skiagraphic appearances. Five of the twenty cases were thought to be free from phthisis pulmonum after the usual mode of examination, while only one of these passed the test of the X-rays as non-tubercular. Two cases pronounced to be phthisis were regarded after being skiagraphed as free from it; one was seen to have peritonitis with effusion, the other clear, transparent lungs. As far as the exceedingly scanty and inadequate notes of the after-histories go, the fluoroscope proved to be a more accurate means of diagnosis than the customary physical methods.

An interesting series of figures, not dealt with in the paper itself, might be deduced from comparison of the mensuration and weight of the different patients, especially with regard to their family histories.

Thus, of the three cases described as non-tubercular, all of them males, one only had a satisfactory family history, and he showed the least power of full thoracic expansion and the smallest difference between the girth of his chest and waist. Of the phthisical patients, four males gave a good, and four a "not-good" family history (by not-good we presume a tubercular strain is indicated), while five females had a good and four a bad account to give of their progenitors. The average results may be given as follows:—

	Height.	Girth of Chest Inspiration > Expiration.	Girth of Chest on Inspiration > Girth of Waist.
NON-PHTHISICAL:	Ft. in.	In.	In.
Three males . .	5 9·2	2·66	6·56
PHTHISICAL:			
Family history—			
1. Good:			
Four males .	5 10	2·25	3·75
Five females .	5 3·5	2·6	2·4
2. Not good:			
Four males .	5 6	2·62	4·12
Four females .	5 4·7	2·12	3·00
TOTAL PHTHISICAL:			
Males	5 8	2·43	3·93
Females	5 3·8	2·38	2·66

One of the male phthisical patients with a bad history is given as being able to expand his chest 3 in., until it became 9 in. greater in girth than his waist on full inspiration, when the measurement of the chest reached 39 in. in a 5 ft. 8 in. man, weighing 10 st. 8 lb.

King (*Med. Rec.*, N. Y., 3rd December 1898), discussing the etiology of tuberculosis, as shown by 100 cases, is also forced to the conclusion that heredity is an insignificant causative factor. Of the 100 tubercular patients, one or both parents had been tubercular in twenty-eight; of 100 non-tubercular persons, taken as a contrast, twenty-six showed a similar history. In twenty-eight of the 100 infected, clear evidence of infection was obtained.

Fränkel (*Berl. klin. Wchnschr.*, 9th January 1899) deals with the prophylaxis of tuberculosis. Cornet, in 1899, postulated that expired air *never* contained tubercle bacilli or spores, and that the bacilli in the sputum, while it is still moist, cannot be disseminated through the air. Flügge, however, in 1897 demonstrated that the acts of coughing, sneezing, and even of speaking, cause bacilli to be ejected into the air, the fine drops of moisture present in it allowing them to retain their vitality, and their conveyance through considerable distances. Fränkel argues thus: as in the course of even ordinary conversation a phthisical patient may introduce bacilli into the air, and very many more when coughing or sneezing, destruction or sterilisation of the sputum is insufficient; while the best method of preventing the dissemination of the organisms is afforded by specially constructed face masks, formed so as to catch the drops of fluid expelled in speaking, coughing, etc. Hübener has shown that in patients wearing such a mask the colonies of *M. prodigiosus* upon agar plates placed a short distance in front of the mouth fell from an average of 527 without, to 10 with the mask, after ten minutes' speaking; from 94 to 0 after three or four acts of coughing; and from innumerable to 415 after one sneeze. Further particulars concerning the masks, their benefits and disadvantages, should be gathered from the original paper.

Minor contributes an article in favour of the hygienic treatment of phthisis, as against the medicinal, in the *New York Med. Journ.* for 14th January 1899; his text may be briefly stated in M'Lean's words (*Journ. Am. Med. Assoc.*, Chicago, February 1899), "If the phthisical patient would live, he must work for it." Weaver (*Journ. Am. Med. Assoc.*, Chicago, 14th January 1899) has got good results by a method of forcible alveolar expansion in the lungs, in which a deep inspiration is taken, the glottis closed, and straining efforts made; this is to be done every five to fifteen minutes. Other additional manœuvres that are practically immaterial may be directed. Of fifty-six cases treated on these lines, of nineteen in the first stage of phthisis, all recovered (at the time); of twenty-eight in the second stage, sixteen were cured, and three died; of nine in the third stage, six were improved temporarily.

SURGERY.

UNDER THE CHARGE OF

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THE LATER RESULTS OF GASTRO-ENTEROSTOMY.

MM. HARTMANN and Soupault contribute a series of articles on this subject to the *Rev. de chir.*, Paris, based upon twenty cases which have been under their immediate observation. Their results are more or less in harmony with those arrived at by other observers.

The most striking result of a successful gastro-enterostomy is the disappearance or amelioration of subjective symptoms referred to the stomach, *e.g.* pains, vomiting, etc. The appetite returns, and the patient is able to indulge in articles of food which would, prior to the operation, have provoked considerable suffering, especially in those the victims of pyloric obstruction, whether functional or organic. The general health improves, the febrile attacks disappear, and the body-weight increases. These phenomena are especially marked in cases of innocent stenosis of the pylorus; they are exceptional in malignant obstruction, and are scarcely to be expected, seeing the patient continues to suffer from the cancer which is left *in situ*. The average duration of life after gastro-enterostomy for pyloric carcinoma, amounts to seven months.

As regards the changes in the stomach which follow upon the operation, the authors have observed a general tendency of the organ to return to its normal dimensions. The degree varies in different cases. It depends very much on the condition of its musculature before operation, and this depends more upon the duration of the pyloric obstruction than upon its innocent or malignant character. In cancerous stricture, the normal dimensions of the stomach may be more rapidly recovered, because the obstruction is more rapidly developed. Adhesions which fix the stomach in an abnormal position may interfere with its contraction after operation. The procedure of inflating the stomach, which was employed to indicate its dimensions, was also of service in demonstrating the continence of the artificial opening into the intestine. The introduction into the stomach of even considerable quantities of gas does not force the opening into the bowel; it escapes more easily by the cardia and œsophagus. The continence of the new opening is not due to the formation of a sphincter. The explanation would appear to be that, in the resting condition of the stomach, the lips of the opening are approximated by the elasticity of the muscular tunic, and by the tension of the walls of the organ; when the stomach contracts, the orifice opens and is "forced" by the current of food.

Although the cessation of vomiting and the improvement of the general health indicate the sufficient passage of the gastric contents into the intestine, the motor functions of the stomach are never quite restored. The food lingers in the stomach, especially when the opening into the intestines is narrow or has not been made at the most dependent part of the organ.

The chemical changes within the stomach are usually considerably altered in patients who have undergone gastro-enterostomy. In malignant cases the liquid withdrawn during fasting is often neutral; in innocent stenosis of the pylorus the acidity is always diminished, and it may disappear altogether. The diminution is partly due to the presence of bile, partly to the fact that the food escapes more rapidly from the stomach, and therefore acts more feebly in exciting secretion, and partly also to the loss of function or atrophy of the glands of the stomach, resulting from the condition on account of which the gastro-enterostomy was performed, which atrophy continues to progress after the operation. There is further a delay in the production of the secreted elements of the juice.

The regurgitation of bile into the stomach is extremely common after gastro-enterostomy; it takes place through the artificial opening, apparently in an intermittent fashion; the maximum occurs about one and a half hours after a meal, when the biliary secretion is stimulated by the contact of the food with the mucous membrane of the intestine. To prevent this regurgitation of bile into the stomach, many surgeons make an anastomosis between the afferent and efferent segments of the loop which communicates with the stomach (gastro-enterostomy + entero-anastomosis). The authors hold, however, that no harm whatever results from the presence of even large quantities of bile in the stomach, basing this opinion on the successful results of cholecysto-gastrostomies both in the dog and in the human subject, in which all the bile has been made to flow into the stomach, without causing sickness or any discomfort, or any interference with gastric digestion. It is a very different matter, if the food regurgitates from the intestine into the stomach, along with the bile; the patient then suffers from the most persistent vomiting, and usually succumbs.

As regards the excretions after gastro-enterostomy, they are usually quite satisfactory. The constipation, which is so marked a feature in pyloric obstruction, usually disappears. The urinary secretion soon becomes normal in amount and quality, both in the innocent and malignant forms of stenosis of the pylorus.—*Rev. de chir.*, Paris, Feb. 1899.

DECEPTIVE REMISSION OF SYMPTOMS IN APPENDICITIS.

PROFESSOR DIEULAFOY of Paris gives a graphic description of the deceptive remission of symptoms in the course of acute appendicitis, under the title of the "accalmies traitresses de l'appendicité." He points out that the characteristic features of appendicitis, abdominal pain, vomiting, and fever may yield and improve, and the patient feels better. The improvement, however, may be only apparent, and it may coincide with the development of serious complications, *e.g.* gangrene of the appendix, peritoneal septicæmia, diffuse peritonitis. The apparent improvement or remission of symptoms differs from the genuine, in the persistence of certain symptoms; the belly may remain tympanitic, the muscular rigidity of the abdominal wall does not disappear, and the pulse is still rapid although the temperature may have fallen. There may be albumin or urobilin in the urine. The apparent improvement may be favoured by the injection of morphine, the application of ice, etc., which may mask the development of anxious symptoms. The period of its onset varies from the second to the sixth day of the disease. Inasmuch as the apparent remission of symptoms may coincide with the development of the most serious peritoneal complications, it is quite impossible to distinguish between appendicitis and peritonitis as two stages in the evolution of the disease; they really merge one into the other, so that one cannot say exactly when the peritonitis appears.

The apparent remission of symptoms in appendicitis may be the cause of death. The physician or surgeon in charge of the patient may be induced to defer or decline operative interference, in the hope that there will be a more favourable opportunity for this later on; very soon, however, urgent symptoms due to grave complications develop, surgical interference is thus out of the question, and the patient succumbs.

The practitioner must especially beware of these apparent improvements in the course of appendicitis, and must not be deceived into postponing calling in surgical assistance until it is too late.—*Presse méd.*, Paris, Feb. 8, 1899.

OPERATIVE TREATMENT OF HYDATID CYSTS.

M. POSADAS of Buenos Ayres, who has had considerable experience in the treatment of hydatids, is an advocate of the method in which the germinal living membrane or endocyst is removed, and immediate suture, without drainage, of the opening in the adventitious, fibrous, outer cyst. It is specially applicable to hydatids which are embedded in the viscera, or which are so closely adherent to important structures that their complete extirpation is impossible. The procedure recommended may be described in two stages: The first or preliminary operation consists in exposing the cyst and the organ in which it lies; this involves a laparotomy in the case of abdominal cysts, a thoracotomy for cysts in the lung and pleura, a temporary craniectomy for cysts within the skull. Successful asepsis is essential. The second stage concerns the cyst itself. Assuming that the cyst, as is so frequently the case, is embedded in the substance of the liver, it will be necessary to cut through, very carefully, any liver tissue overlying the cyst, and then incise the adventitious fibrous cyst. In the case of a very small cyst it may be possible to shell it out entire, without opening it. In large cysts, which are much more common, it is best to evacuate the hydatid fluid through a cannula in the first instance, then the opening in the cyst wall is enlarged, and the lining germinal membrane is thoroughly removed. Any fluid remaining in the cyst is soaked up with gauze. It will then be found that the walls of the emptied fibrous cyst come more or less into contact with each other. The wound in the fibrous cyst is then sutured with catgut, and finally the abdominal incision is closed, without having recourse to drainage. Sometimes it may be possible to resect a portion of the fibrous cyst, *i.e.* that projecting from the organ. Similarly, in a cyst of the neck or elsewhere, where complete extirpation is impossible, on account of dense adhesions to blood vessels or other structures, part of the cyst wall may be removed and the remaining adherent portion closed with sutures. In suppurating cysts, drainage must be had recourse to, as the above method is not applicable.

The author tabulates a series of forty-six cases treated by his method; there was one death on the third day after the operation, from septic peritonitis.—*Rev. de chir.*, Paris, March 1899.

THE RESULTS OF 360 LAPAROTOMIES FOR GALL STONES.

DR. HANS KEHR may be said to hold the record for these operations, so far as numbers are concerned. They include various operative procedures, from simple cholecystostomy to the more difficult drainage of the bile duct. The mortality of operations for gall stones, uncomplicated by malignant disease, amounted to 3·8 per cent.

Kehr recommends operation at the earliest possible stage of the disease, before the gall stones have made their way into the lower duct, because the operation for their removal is much easier and less dangerous. When there are signs of calculous obstruction of the common bile duct, it is not safe to defer operation, unless the passage of gall stones or diminution of the jaundice affords proof of the likelihood of recovery from the obstruction. The successful treatment of cholelithiasis by medical means is so very exceptional that it is not to be relied upon. Kehr agrees with those who believe that the chronic irritation by gall stones may play a part in the development of cancer.

Cholecystostomy is the favourite operation; although theoretically the retention of the gall bladder may allow of the formation of stones in the future, this has never been observed to take place. It is therefore to be preferred to the more radical operation of cholecystectomy, because the latter is difficult and dangerous. Ideal cholecystotomy, in which the opening in the gall bladder is closed by sutures after the evacuation of its contents, is only to be performed in exceptional cases, because thorough drainage of the bladder is the most certain means of getting rid of the existing catarrh and of avoiding leaving behind any concretions in its interior.—*Deutsche Med.-Ztg.*, Berlin, Feb. 20, 1899.

ARE COMPLETE CASTRATES CAPABLE OF PROCREATION?

WHILE it is a matter of common observation that an adult who has undergone double castration or lost the function of both testicles as the result of disease, may continue to discharge his marital functions without any impairment being observed by himself or his partner, it is generally and correctly assumed that he is incapable of procreation. Dr. Sturgis of New York ventilates the latter question in a recent paper, and arrives at the following conclusions:—In animals, for a varying period after complete castration, normal spermatozoa are found in the contents of the seminal vesicles. This period varies in different animals, being six days for the dog, seven days for the cat, and fourteen days for the guinea-pig. In man, clinical cases are recorded where fecundation of the female has occurred after coitus with the male who has been completely castrated, but in accepting the correctness of such statements we must remember the adage, that accidents may happen in the best regulated families. Still, if we are to assume the accuracy of a case recorded by Princeteau, spermatozoa may exist for a certain time in the seminal vesicles of a eunuch, and, arguing from what occurs in animals, this is quite probable. A complete castrate in man, as in the dog and cat, *may* be capable of procreation, provided the coitus occur within the first seven days after the castration.—*Med. News*, Phila., October 1898.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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FOUR CASES OF VESICO-VAGINAL FISTULA TREATED BY FREUND'S METHOD.

Romm (*Centralbl. f. Gynäk.*, Leipzig, 1897, No. 9) refers to the cases published by Freund in 1895, where the only alternative was the operation of kolpokleisis. He opened the posterior fornix, pulled down the fundus, and with a sharp spoon freshened its posterior wall, and sutured this to the rawed edge of the fistula on the anterior vaginal wall. An opening was then made into the fundus for the escape of menstrual discharge.

Romm points out the risk of ascending urinary infection after kolpokleisis, extending in some cases to the kidneys, and also refers to the uterine atrophy which follows Freund's operation as a consequence of continuous torsion of the blood vessels, with menstrual arrest resecting.

With Freund's operation, however, the bladder is completely closed, and the operation itself is not more serious than an ordinary vaginal cœliotomy.

Case 1.—Patient æt. 21, with complete laceration of the anterior vaginal wall extending to the urethra, allowing three fingers to pass into the bladder. Ordinary methods failing, the posterior uterine wall was freshened and sutured with a continuous catgut suture to the rawed edge of the fistula. Result was complete continence eight months later, but no recurrence of menstruation.

Case 2.—Patient æt. 42; large fistula in anterior vaginal wall as result of gangrenous colpitis. Freund's operation; escape of urine per vaginam on sixth day, from a small opening near the cervix, which was freshened and sutured; further escape on the eighth day. Opening again sutured; nine months later complete cure; no recurrence of menstruation.

Case 3.—Patient æt. 24; fistula with prolapse of vesical mucous membrane, allowing little finger to enter bladder. Treated in the ordinary way; four days later, escape of urine per vaginam. Freund's operation; complete cure.

Case 4.—Patient æt. 30; fistula in consequence of gangrenous colpitis. Ordinary methods failed. Freund's operation successful after closing a small opening close to the cervix.

Romm believes it is unnecessary to make an opening in the fundus for the escape of menstrual discharge, as suggested by Freund. In his first two cases there was no recurrence of menstruation, probably from the atrophic condition of the uterus, and in the fourth case menstruation only recurred twice, the fluid escaping through the bladder, and then ceased entirely. This publication is followed by the report of two cases by Kahn, successfully treated by Freund's method.

MISTAKEN DIAGNOSIS OF ABDOMINAL TUMOURS.

SCHRÖDER (*Centralbl. f. Gynäk.*, Leipzig, 1898, No. 41) reports two interesting cases:—

CASE 1.—Patient æt. 44; seventeen pregnancies—the last, four years before coming under observation. Seven years previously, patient noticed an abdominal swelling causing at first no inconvenience, but later requiring to be tapped, 10 litres of fluid being drawn off. When admitted to hospital the chief symptoms were dyspnœa, frequent micturition, and obstinate constipation. The whole abdomen was occupied by a tumour, partly solid, partly cystic. Liver dulness extended into that over the tumour; uterus retroflexed; appendages not palpable. Nodules felt through the anterior fornix, projecting from the lower edge of the abdominal tumour. Diagnosis was, double multilocular ovarian cyst.

On opening the abdomen the tumour was found to consist of multiple cysts, varying in size from a cherry to a child's head; on separating parietal adhesions the tumour was found to be a cyst of liver, which was with difficulty removed. There was marked cirrhosis of the solid portion of the liver. Death three hours after operation.

CASE 2.—Patient æt. 42; nine children; menstruation regular. Five years previous to her coming under observation, she noticed a swelling in the right inguinal region, which had rapidly increased. No jaundice. Liver dulness extended one finger's-breadth below the costal margin, and there was a tympanitic note between this and upper border of tumour, which was fixed and tender. Its lower border could not be defined. The diagnosis made was right-sided malignant ovarian or intestinal tumour. On opening the abdomen, the tumour was adherent to omentum, intestines, and abdominal wall, and was found not to be pelvic but connected with the liver and gall bladder. On microscopic examination, it was found to be a malignant tumour originating in the gall bladder. It was removed by a knife-shaped Paquelin, and patient recovered. Schröder remarks that in the first case cystic disease of the liver could not easily have been diagnosed from an ovarian cyst, as all the signs pointed to the latter. In the second case there was no jaundice or other sign of the gall bladder being affected with malignant disease.

COMBINED EXTRA- AND INTRA-UTERINE PREGNANCY.

C. J. MILLER (*N. Orleans Med. and Surg. Journ.*, October 1898) records a case of combined intra- and extra-uterine pregnancy. He was called to a coloured woman who was the subject of miscarriage. He found a 3½ months fœtus in the bed, and bimanually expressed the secundines. Four hours later he was summoned, and found the patient, who had been seized with intense abdominal pain, in a collapsed condition, with pulse 160 and shallow breathing. The abdomen was rigid, tender, and dull on percussion. No rupture of the uterus could be found. The patient died. On post mortem examination the left Fallopian tube was found to be ruptured, and among the blood clots in the pelvis was a fœtus about 3½ inches long.

RUPTURE OF VAGINA DURING PARTURITION.

J. W. DANIEL (*Ann. de gynéc. et pediat.*, Bruxelles, September 1898) says that while rupture of the vagina is usually associated with rupture of the uterus, the tear extending downwards, in a certain number of cases the injury is confined to the vagina. He reports a case of spontaneous rupture of the vagina in a primipara, æt. 25 years. The labour was natural, and no instruments were employed. There was no laceration of the perineum, and the placenta was born promptly and naturally. Shortly afterwards the patient became pale, with sighing respiration and weak pulse. The uterus was firmly contracted, but the patient was bleeding profusely. On vaginal examination, there was an irregular fleshy mass felt on the posterior vaginal wall, which extended below the middle of the vagina, but was unconnected with the uterus, and from this the hæmorrhage was proceeding. The rent extended to the rectal wall, but not into it. Hot vaginal douches checked the hæmorrhage, but it was found that the rent was too high to repair. The patient recovered.

Siebang (*München. med. Wchnschr.*, 1898, No. 43) reports another case of spontaneous rupture of the vagina during parturition, and calls attention to the fact that, while this rare accident is usually caused by operative interference, it may result independently. The patient had had eight normal confinements. She suffered from pendulous belly, but the labour was natural until the membranes ruptured, when she was seized with severe pain in the right side. A physician was summoned, who found her in a moribund condition. The abdominal tumour was irregular, and on the left side fœtal parts could be felt apparently free in the abdominal cavity. The head was in the pelvis. The doctor delivered with forceps, after which profuse bleeding occurred, and on introducing the hand a large tear in the fornix was found extending into the parametrium and communicating with the abdominal cavity, into which the placenta had escaped. The placenta was removed, and the wound firmly packed with gauze, but the patient died twenty minutes later.

The author believes that the pendulous belly prevented the head descending in the axis of the pelvis, and that this caused the vaginal rupture.

THERAPEUTICS.

UNDER THE CHARGE OF

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Ergot in Chronic Malacia.

Jacobi (*Med. News*, Phila., October 22, 1898) states that some cases of chronic intermittent fevers with enlarged spleen are benefited by ergot, after having resisted the action of quinine, arsenic, methylene blue, eucalyptus, and piperin. When enlargement of the spleen is comparatively recent, the contracting effect of ergot is noticed within a moderate time. It seems to control and lessen the feverish attacks, although the plasmodia do not disappear from the blood so rapidly as after quinine. A combination with quinine or arsenic is often advisable, and in acute cases quinine is preferable. Ergot, probably by its sudden contracting action on the spleen, and the consequent forcing of plasmodia-containing corpuscles into the circulation, sometimes brings on an attack of chills and fever.

Phosphorus in Rickets.

Lop has used phosphorus in the treatment of a large number of cases of rickets, but in many of them for too short a time to enable him to draw definite conclusions. In a large proportion of the cases, good results were not obtained until after many months. Seven presented very definite improvement after about twenty months' treatment, in five others the benefit was less marked. He gave the phosphorus in doses of $\frac{1}{60}$ gr. dissolved in cod-liver oil, or in some fatty vehicle. It sometimes produced gastro-intestinal derangement, and was then stopped for a few days.—*Rev. de thérap.*, Paris, November 1, 1898.

Treatment of Tetanus by Intracerebral Injections of Antitoxin.

Roux and Borrel were originally induced to try this method of treating tetanus, because the tetanic antitoxin, when injected into animals, remains in the blood, whereas the toxin has been absorbed by and remains in the nerve-cells. The antidote, therefore, does not come into contact with the poison. The antidotal serum is quite efficacious if it be injected subcutaneously along with the tetanus toxin, because the two then meet in the blood, but it proves powerless against the poison which has already reached the nerve-cells. This is why treatment so often fails both in man and animals. Experiments made on guinea-pigs seemed to bear out the truth of this view. Antitoxin introduced into the brain protects the upper part of the cord when the lower is already affected by the poison, but it does not cure the involved portion, and limited convulsions may take place still. They have also shown that if the medulla be already poisoned, as shown by impaired deglutition or respiration, death cannot be prevented. Out of twelve cases treated in this way in France, eight died and four recovered.

Rambaud (*New York Med. Journ.*, Dec. 17, 1898) reports two cases, both of which died, and Church one which recovered. In all, subcutaneous injections of the serum were given as well as intracerebral. The local effects of the injection on the cerebral tissue were very slight.

THE ACTION OF ALCOHOL AND OF WATER ON METABOLISM.

In spite of the important dietetic and medicinal uses of alcohol, its action on human metabolism is still somewhat imperfectly understood. It has been proved that the greater part of it—nearly the whole, in fact—is oxidised in the body, and that at the same time the consumption of oxygen and the excretion of carbonic acid are not increased. This means that it is oxidised, but at the same time protects other tissues from oxidation. It is still questionable whether these tissues are the albumins, or the fats and carbohydrates of the body. Experiments, as hitherto made, have not been successful in finally deciding this point. It is absolutely necessary that the nitrogenous intake and output should be carefully calculated, and this has very seldom been done, but the few experiments of this kind already made go to show that alcohol does not protect the albuminous tissues from oxidation. Rosemann (*Deutsche med. Wchnschr.*, Leipzig, 1898) has conducted an experiment on these lines. The diet consisted of 300 grms. flesh, 350 grms. bread, 100 grms. butter, 100 grms. sugar, and 30 grms. cocoa per day, equal to 15·5 grms. nitrogen. On the fifth and sixth days the amount of nitrogen excreted in the urine and fæces equalled the amount in the food. On the next three days 100 c.c. absolute alcohol, and on the the fourth day 150 c.c. were taken, diluted with water, and it was found that this had no influence whatever in lessening the excretion of nitrogen. The inference is, that alcohol does not lessen nitrogenous waste in man. A similar experiment made with water led to the conclusion that it increased the excretion of nitrogen.

PATHOLOGY AND BACTERIOLOGY.

UNDER THE CHARGE OF

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TOXINS, ANTITOXINS, AND TOXOIDS.

Plimmer (*Journ. Path. and Bacteriol.*, Edin. and London, Dec. 1898, p. 489) gives a critical summary of Ehrlich's recent important work on this subject, published by him in a paper entitled, "Die Werthlemessung des Diptherieheilserums und deren theoretische Grundlagen." It has long ago been suggested that the period of incubation represents a time of warfare between the invading germs and the forces of the body, and that when the disease manifests itself it represents a victory for the germs, the symptoms and signs of the disease being due to the toxins or poisonous substances produced by the bacteria. It is suggested that, as pathogenic bacteria are constantly gaining an entrance into our bodies, many passing malaises and fleeting localised aches and pains are probably due to the resulting battle between the germs and the body, in which the latter is victorious, and hence the disease never shows itself. Toxins are derived from albumins or their complex quaternary allies, probably by progressive hydration, under the influence of bacteria. If this decomposition of albumin be complete, alkaloidal toxins are formed. Those are met with under the name of *ptomaines* in the products of putrefaction. They are oleaginous or crystalline bases. If hydration be incomplete, which most frequently happens, much more complex modifications of the albumins are produced, of so slight a change that reagents are not delicate enough to detect them. They are, however, able to frustrate physiological action. They are called *toxic albumoses*. They are neutral, amorphous, and chemically near to peptones. If a bouillon culture of a pathogenic bacterium be filtered through a porcelain filter, the fluid will contain the toxins but not the bacteria. These toxins can produce all the main symptoms of a given disease, and they determine, moreover, the kind and course of the disease. On comparing the effects produced by these toxins with those of the poisonous alkaloids, for instance, two pronounced differences are seen. An alkaloid such as strychnine, when taken in a fatal dose, will act in so many minutes, while toxins take a much longer time, hours or days. Again, the toxins have a much greater toxicity. That of tetanus, for instance, is almost incredible, $\frac{1}{20.000}$ of a mgrm. being sufficient to kill a mouse of 15 grms. weight; and corresponding to this a dose of ·23 of a mgrm. would kill a man of ordinary weight. Of strychnine 30 to 100 mgrms. are necessary to kill a man. Again, toxins are very easily rendered innocuous by the action of certain agents, *e.g.*, tetanus toxin is destroyed by exposure for a few minutes to 65° C. Ehrlich heated tetanus bouillon with carbon bisulphide, and found that in a relatively quick time it became as good as non-poisonous (*i.e.* he gave 1 c.c. to a mouse without great harm). The mouse, moreover, attained immunity against tetanus toxin in about eight days, as compared with the very much longer period required by the ordinary toxin. He further showed that this modified tetanus toxin is able to enter into combination with antitoxins both *in vivo* and *in vitro*. Such modified toxins, *i.e.*, which have lost their poisonous properties, but yet retained their powers of combination with antitoxins, are called *Toxoids*. Since these toxoids must arise from the toxins, and possess their specific powers of combination, it follows that they must contain the characteristic combining groups in their integrity, and that their loss of toxicity has been brought about by some rearrangement of the atoms in the primary substance. Thus a great variety of toxoids can be produced from every toxin, through the action of toxic or chemical agents. Certain toxoids, *e.g.* of tetanus, possess immunising properties, but it is improbable that all toxoids do; a curious parallel is found in the vegetable toxalbumins—ricin, abrin, robin, and crotin. Robin produces an antitoxin which almost entirely corresponds with antiricin, and Ehrlich suggests that the much less poisonous robin may represent a naturally occurring toxoid of ricin. Again, Calmette has shown that the serum of an animal which has been immunised against snake poison can neutralise considerable quantities of scorpion poison, and hence the latter may be a toxoid of snake poison. The central nervous system (the motor ganglion cells) has an extraordinary affinity for tetanus toxin. If this be given to an animal in small quantities, it will all be found fixed in the central nervous system. This is true *in vitro* as well as *in vivo*, as proved by Wassermann and Takaki (*Berl. klin. Wchnschr.*, 1898, No. 1). They pounded a guinea-pig's brain into a fine pulp, and then added some tetanus toxin. The mixture was now centrifugalised, and it was found that there was practically no toxin in the supernatant fluid, it was all fixed in the nervous tissue at the bottom. Atom groups must be present in the ganglion cells, which have a maximal specific affinity for tetanus toxin. All functioning protoplasm may be conceived as consisting of a working nucleus and side chains joined to it, having different functions. One of these has the power of fixing the tetanus toxin, which thus becomes fast anchored to the cell, and keeps it under its influence. This explains the slowly beginning and long continuing disturbances of cell function met with in tetanus. The origin of the antitoxin can now be explained. Normally, these side chains, *i.e.* specifically fixing atom groups, probably possess definite physiological functions in addition to their property of fixing certain poisons (diphtheria, tetanus, snake poison, abrin, ricin, crotin). But if a given side chain be linked to a definite poison, it becomes no longer free for its physiological use, and hence the working nuclear protoplasm throws out another side chain to compensate for its loss. If this newly found chain be linked to a new dose of the poison, a second regeneration will become necessary, which in turn may become linked to a fresh dose of the poison, and so on, fresh regeneration taking place after each fresh dose of poison. In the course of typical immunisation, the cells are, so to speak, trained to produce these side chains in ever-increasing quantity. *Over compensation*, not *exact compensation*, is the rule, and by greatly increasing the doses of toxin such an overgrowth of side chains will be produced that they will be too much for the cell to carry, and will then be thrown out as a kind of excretion into the blood. According to this view, then, *antitoxins* represent the side chains of the cell which have been produced in excessive numbers and then thrown off. This antitoxin must possess a specific relation to the toxin. So far as tetanus is concerned, it would seem that the free side chains, the antitoxin, gain with the severance of the physiological link an increased relation to the tetanus toxin. This theory may hold good of other poisons, but probably not of all. The crystallisable poisons, poisonous alkaloids, glucosides, and other chemically well-defined substances have not the power of producing antitoxins. This is confined to the group of toxins, ferments, and toxalbumins. It is probable that the toxicity of these bodies consists in their power of anchoring their toxophorous groups to cells of certain organs which are important to life. If they lack this power they remain harmless, and perhaps this may explain many cases of natural immunity. An important practical suggestion in treatment arises naturally from the above reasoning. When a disease—*e.g.* diphtheria, tetanus, etc.—has commenced, there will be free toxin circulating in the blood, because the cells take a certain time to pick it up; hence the earlier the counteracting antitoxin is injected the better, and the later it is injected the greater must be the dose. Any excess of antitoxin will circulate in the blood and combine with later appearing toxin, for which it may be said to lie in wait. Further, Ehrlich thinks that it competes with the cell for a share of the toxin which has already become linked to that cell. The further problem regarding *toxoids* is very important practically, as the testing of the exact value of a toxin or antitoxin is influenced largely by their presence or absence. When a toxic bouillon is gradually added to a standard antitoxic serum (one immunity unit in 1 c.c.), two limit values are reached,—one, when the serum is completely neutralised (this Ehrlich calls Lo), and the other, when such additional quantity is added to the neutralised toxin as will cause the death of an animal within four days (this he calls L+). This additional quantity is the simple lethal dose (expressed by D). Hence it follows that D = L + minus Lo. But, experimentally, it is found that D has a higher value than this in the case of most toxins, and the difference is due to the presence of toxoids. The latter can increase the value of D only by possessing various affinities for antitoxin; and it has been practically shown that these must be of three kinds—(1) Protoxoids, *i.e.* those which have a greater affinity for antitoxins than toxins; (2) syntoxoids, those of an equal affinity; and (3) epitoxoids, *i.e.* those which have a less affinity. Hence, if antitoxic serum be added to a mixture of protoxoids and toxins, they will join with the toxoids first, and completely saturate them before saturating the toxins. Protoxoids, though non-poisonous in themselves, will cause death when added to a neutral toxin—antitoxin mixture, *i.e.* Lo—by uniting with the antitoxin and liberating the toxin. Ehrlich illustrates it thus: Suppose a diphtheria bouillon contains 99 parts protoxoids + 1 part toxin, neutralised by antitoxin, it becomes 99 parts protoxoid-antitoxin + 1 part toxin-antitoxin, *i.e.* Lo; now, add 1 part protoxoid, and we get 100 parts protoxoid antitoxin + 1 part toxin = L+. Syntoxoids, on the other hand, make no difference when added to Lo, nor do they influence in any way the value of D. Epitoxoids, however, have an important influence. If a neutral mixture of toxins and epitoxoids be taken, it may be expressed thus—99 parts toxin-antitoxin + 10 parts epitoxoid-antitoxin = Lo. Add to this 11 parts toxin, and we get 100 parts toxin-antitoxin + 1 part toxin (free) + 10 parts epitoxoid (free) = L + the lethal dose. Ehrlich makes the important deduction that it is not the presence of pro- and syntoxoids, but exclusively the presence of epitoxoids which is able to raise the value of D. These theories are all borne out by experiment. He has devised equations by means of which the exact determination of epitoxoids in a given toxin solution can be made, so that the true saturation capacity of the immunity unit for any given toxin, as well as the true relations of the various components of the toxin (pro- and syntoxoids, etc.), may be always accurately calculated.

THE CULTIVATION OF PROTEOSOMA (LABBE) IN GREY MOSQUITOES.

Ross (*Indian Med. Gaz.*, Calcutta, Nov. 1898, p. 401) gives an interesting account of further researches into this subject which he has recently made. He formerly described peculiar pigmented cells in two dappled-winged mosquitoes, and suggested that they were a stage of the parasite of malaria in these animals. He considers that his present experiments have given him conclusive evidence to this effect. He finds that out of 245 grey mosquitoes fed on birds with proteosoma, 178, or 72 per cent., contained pigmented cells. Out of forty-one grey mosquitoes fed on a man with crescents, five on a case of immature tertian parasites, 154 on birds with halteridium, twenty-five on a healthy sparrow, and twenty-four on birds with immature proteosoma, or a total of 249 insects altogether, not one contained a single pigmented cell. Further, that—(*a*) Ten mosquitoes fed on the sparrow with numerous proteosoma, contained 1009 pigmented cells, or an average of 101 each; (*b*) ten mosquitoes fed on the sparrow with moderate proteosoma contained 292 pigmented cells, or an average of twenty-nine each; (*c*) ten mosquitoes fed on the sparrow with no proteosoma, contained no pigmented cells. Full and interesting particulars are given of the proteosoma during the different stages of its growth. At first it seems to consist of about 20 grs. of pigment arranged within a small oval area, having a long diameter of about 8 μ. It lies especially beneath the muscle of the stomach of the mosquito. They continue to grow steadily, and in six or seven days reach a growth of about 60 or 70 μ, and are finally extruded from the organ into the general body cavity of the host. His experiments show that he could produce those parasites at will in grey mosquitoes, by feeding them on birds infected by proteosoma, a parasite very similar and closely related to the hæmamœbæ of malaria in man. Further, numerous control experiments, in which mosquitoes of the same species were fed on healthy men and birds, or on men and birds with other gymnosporidia, invariably yielded negative results. He deduces, then, that these parasites are a development in the mosquito of proteosoma in birds. They are probably coccidia.

DISEASES OF THE EAR, NOSE, AND THROAT.

UNDER THE CHARGE OF

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AND

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THE OCCURRENCE OF INTRACRANIAL COMPLICATIONS IN ACUTE SUPPURATIVE OTITIS MEDIA.

It is a well-known fact that intracranial suppurative lesions arise most frequently during the course of chronic suppuration in the middle ear. The relative infrequency with which acute purulent ear diseases have been followed by secondary infective lesions has often been commented upon. The possibility of the occurrence of serious intracranial complications during the acute stage must not be lost sight of, however, and recently more than one writer has drawn attention to the fact that such complications occur with greater frequency perhaps than has been generally recognised. In the chronic cases the partial destruction of the lining membrane of the tympanum and its neighbouring cavities, with caries of the subjacent bone, offers a pathway for invasion of the more deeply situated structures, such as does not exist in the acute conditions.

It would not be out of place to examine a few of these more recently recorded cases in some detail, and note some of the pathological conditions existing therein. Hugh E. Jones of Liverpool (*Journ. Laryngol.*, London, September 1898) has reported seven fatal cases of intracranial complication secondary to middle ear suppuration, three of which followed an acute otitis media. In two of the latter group a purulent meningitis supervened; in both an acute otitis had arisen ten days before admission to hospital; in each case the mastoid antrum was opened, and pus evacuated. In the third case, where the acute middle ear suppuration occurred after influenza, a communication was found between the antrum and middle fossa of the skull, in which an extradural abscess existed. Notwithstanding the improvement which followed the treatment of this complication, suppurative meningitis ensued, with a fatal termination.

A somewhat similar case to this last one is reported by M'Kernon of New York (*Arch. Otol.*, N. Y., June 1898). The patient, an adult male, was seen five weeks after the onset of a very acute otitis media. The presence of severe mastoid symptoms necessitated immediate opening of the antrum, which was found to contain a quantity of pus. A perforation was found in the roof of the tympanum, through which a probe could be passed into a subdural abscess, the cavity of which was limited by adhesions. In this instance complete recovery followed the operative interference. In Milligan's case (*Med. Chron.*, Manchester, January 1899) a large fistula led from the antrum into the posterior fossa of the skull, where an extradural abscess was found. Preysing (*Arch. Otol.*, N. Y., October 1898) reports two cases of extradural abscess presenting points of interest. In neither of the patients was there a history of any previous ear affection, and the acute attack in both was of short duration. In one, severe pain in and behind the right ear had commenced two weeks before admission to hospital, while in the other only eight days had elapsed since the onset of the ear condition. It may be noted, further, that in neither case was the tympanic cavity the seat of very active inflammation. The membrane in both was somewhat reddened and bulging, and, as the result of paracentesis, serous exudation only was evacuated, there being no trace of pus. There was swelling and tenderness over the mastoid process in each case. Without entering into details, it may be here stated that in the first patient no pus was found in the mastoid cells, but in the vicinity of the sigmoid sinus there were granulations and carious bone. During further chiselling of the bone, the posterior cerebral fossa was perforated, and a quantity of pus gushed out; the opening was enlarged, and the sinus was then seen to be covered with granulations, and actively pulsating. Owing to collapse of the patient at this stage, no further effort was made to expose the antrum. All the symptoms finally disappeared, the ear remained dry, and the patient was discharged cured at the end of six weeks.

In the second case, while raising the periosteum over the mastoid process, a quantity of pus mixed with blood escaped. The bone underneath was chiselled, and the dura mater was exposed covered with foul granulations. The centre of this area appeared to correspond to the point of exit of the mastoid vein. Here also the antrum was not opened into, as there was no extension of the diseased bone in that direction, and the tympanum had been found only to contain serum. The patient made an excellent recovery. A very valuable paper has been published by Grünert (*Arch. f. Ohrenh.*, Leipzig, Bd. xliii. Nos. 2 and 3), in which twenty cases of extradural abscess are dealt with, twelve of these having occurred in the course of acute ear disease. In the majority of them the middle ear inflammation healed, while in two the tympanic condition was never purulent. This latter fact, also observed in Preysing's two cases, is an interesting one. Grünert quotes Zaufal to the effect that pneumo-bacilli are found in those cases where rapid healing takes place. The extradural abscess is usually found in the vicinity of the lateral sinuses. Clinically, Grünert's cases were characterised by acute pain in the side of the head, and by the presence of œdema or abscess above and behind the mastoid, while the process itself remained free. In only one instance was there a slow pulse. In all, the eyes were normal.

It is interesting to observe that in M'Kernon's and Milligan's cases, and in one of those reported by Jones, the dura mater had been exposed by destruction of the intervening bony wall. Such a condition is met with as the result of a long-continued chronic suppuration, but in these instances the acute nature of the inflammation may have accounted for the rapid destruction of the bone. It must not be forgotten, however, that the tegmen tympani may be anatomically imperfectly developed, and may be the site of small perforations.

The occurrence of otogenous pyæmia in the course of acute otitis media suppurativa demands careful consideration. The following cases are of interest on that account, and may be studied with advantage. Laurens (*Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, Jan. 1899) reports two cases in which there was no apparent participation of the lateral sinus. In the first case, acute pain in both ears, followed by suppurative discharge, supervened upon an intranasal operation. Fifteen days after rupture of the tympanic membranes, it was found necessary to incise the right membrane. Four days later, delirium, nausea, vomiting, fever, intense occipital headache and rigors came on. There was pain on pressure over the right antrum, the surface of the mastoid being very slightly swollen, while its posterior border could not be quite so clearly defined as upon the left side; there was slight prominence of the posterior wall of the auditory meatus, and some reddening of the skin lining it, but otherwise the local appearances were not marked. There was nothing abnormal in the sterno-mastoid region, and palpation of the large vessels in the neck revealed nothing. The condition in the left ear had healed. The right mastoid antrum was opened and a quantity of pus evacuated; the sigmoid sinus was exposed, and its surface was found covered with fungous granulations. In order to obtain a more complete view of the sinus, the bony wall was removed still further, and then the exposed surface of the vein was seen to be blue in colour, pulsating, soft, and compressible. No further interference was deemed necessary. Symptoms of pyæmia, however, continued. There were great oscillations of temperature, gastro-intestinal symptoms with fœtid diarrhœa, some painful enlargement of the liver, and impairment in the percussion note over the left lung. Typhoid and tuberculosis were excluded. A second operation was performed upon the mastoid, but there was nothing to indicate any retention of pus. The symptoms continued more or less severe for three weeks after the first operation before signs of improvement began to show themselves. Complete recovery took place. The case is of interest on account of the long duration of the infection, from the absence of any signs of sinus thrombosis, and from the fact that it occurred in an acute affection of the ear.

The second case presented the following main features:—A child, æt. 8½, was seized with sickness, vomiting, and fever; there was no skin eruption. Three days later there was deafness and acute pain in the left ear, followed by discharge. The tympanic membrane was punctured. Fever and pain continued with the onset of rigors, followed by sweating. There was tenderness behind the mastoid, and the rapid formation of a red, painful swelling over the back region of one hand. There were no abdominal or thoracic symptoms, and the right ear was normal. The left mastoid cells and antrum were opened, but contained no pus. The sigmoid sinus was exposed, and the bone removed for a distance of 2 cms. It was of a blue colour, not pulsating, but easily indented. When the lumen was punctured with a syringe, blood was removed. The swelling on the dorsum of the hand increased, necessitating its being opened. There was no pus found. The child's condition remained grave for eight days after the operation, profuse diarrhœa supervening, but improvement then took place, and finally complete recovery.

Eulenstein's two cases of otogenous pyæmia (*Arch. Otol.*, N. Y., April 1898) also occurred during the course of an acute otitis media. He comments on the necessity, while opening the mastoid, of exposing the lateral sinus in all cases where there is high temperature, even though no signs of pyæmia exist. In one of his patients he had failed to carry this out, contenting himself with removing the granulations and pus from the antrum. A rigor occurred after the operation, collapse and death supervening on the following day. At the autopsy the outer surface of the sinus was found covered with granulations, while to its inner surface there was a thrombus attached, projecting into the lumen of the vessels, and with its central portion necrotic and purulent.

SOME POINTS IN THE DIAGNOSIS OF LATERAL SINUS THROMBOSIS.

THE diagnosis of infective sinus thrombosis frequently presents considerable difficulties; further, when the sinus lies exposed to view, it is often difficult even then to determine whether or not thrombosis has occurred. Where the ear suppuration is recognised, and repeated rigors have taken place with marked rise of temperature followed by remissions, and where metastatic abscesses have already formed, no great difficulty presents itself. In the earlier stages, when it is so essential to arrive at a correct conclusion and actively deal with it, diagnosis is not so easy. A valuable contribution in this connection is made by Whiting of New York (*Arch. Otol.*, N. Y., February 1898), in a paper which has a further intrinsic value, from the fact that three cases of infective sinus thrombosis were successfully operated upon by him. With regard to local manifestations of some significance, comment is made upon the presence of œdema in the mastoid region, extending upwards and backwards over the site of exit of the mastoid vein, and downwards to that part of the scalp which is drained by the occipital vein. This is associated with distinct tenderness. Œdema and puffiness of the eyelids, on the side corresponding to the lesion, has been observed. It is due to interference with the cavernous sinus and engorgement of the ophthalmic vein. Tenderness in the upper part of the posterior triangle of the neck may be present, and furnish considerable help in diagnosis. The tenderness in this region is due to an associated phlebitis of the deep cervical veins, in which the anterior and posterior condylar veins participate; the symptom becomes more constant, in proportion to the greater extension of the thrombus into the neck. In one of Whiting's cases, a diminished blood-flow through the external jugular vein upon the affected side was observed. Obstructive pressure with the finger was made across its course in the neck for a considerable period, but no turgescence of the vein above ensued. On the other hand, when similar pressure was applied to the external jugular upon the healthy side, it became engorged in a very pronounced way. Though the mastoid emissary vein connecting the lateral sinus with the posterior auricular or occipital vein—tributaries of the external jugular—is the largest and most constant of the series of emissary veins, it is somewhat difficult to realise how thrombosis in the lateral sinus can affect the blood-flow along the external jugular through this small connection. Voss (*Semaine méd.*, Paris, 23rd March 1898) reports upon the value of auscultation over the internal jugular vein in cases of suspected thrombosis of the sigmoid sinus. The sign consists in the absence, on the diseased side, of the blowing noise, which a certain amount of compression with the stethoscope normally provokes over the jugulars. This sign, which is best heard under normal circumstances near the base of the skull and along the anterior border of the sterno-mastoid muscle, is wanting on the side upon which the sinus is thrombosed. The author verified this fact by operation in five cases of middle ear suppuration, complicated with sinus thrombosis.

An ophthalmoscopic examination ought to be made in all cases of suspected intracranial complication, as no means should be neglected of making an early and correct diagnosis. Cases of sinus thrombosis are recorded where no statement is made as to the appearance of the fundus oculi. Gradenigo has recently drawn attention to the value of a systematic examination of this kind (*Ann. d. mal. de l'oreille, du larynx, etc.*, Paris, December 1898). According to his own researches, he has no hesitation in affirming that ocular lesions are sometimes the only indication of the commencement or existence of an endocranial complication. The figures which he quotes are compiled from a total of 635 cases, made up of 561 gleaned from literature, and an additional 74 observations of his own. Ophthalmoscopic examination was only made 172 times. Starting with the assumption that optic papillitis is never observed in affections limited to the middle ear and mastoid cells, he finds that papillitis was present in 31 or 59·6 per cent. of the cases of infective sinus thrombosis, whether simple or complicated with an extradural perisinus abscess. It would not be out of place to add here the further statistics which he has compiled :—

In thirty-nine cases of uncomplicated extradural abscess, the majority around the sinus, 16 or 41 per cent., showed papillitis. In thirty-four cases of cerebral abscess, simple or complicated with sinus thrombosis, 18 or 57·9 per cent. had papillitis. In twenty cases of cerebellar abscess, simple or complicated with sinus thrombosis, 12 or 50 per cent. had papillitis. In twenty-seven cases of lepto-meningitis, simple or complicated with sinus thrombosis, 13 or 48·9 per cent. had papillitis. That is to say, that out of 172 cases of endocranial complications of otitic origin, which were examined with the ophthalmoscope, 90 or 52·3 per cent. showed changes in the fundus of the eye, and they occurred most frequently in sinus thrombosis.

When the lateral sinus is exposed in operative interference, considerable doubt may exist in the surgeon's mind as to the exact condition of its interior. The value of pulsation as a diagnostic sign has been discussed. Pulsation may be transmitted from the adjacent brain. Preysing (*op. cit.*) found that unless a considerable portion of the bony support was removed, it was not visible, thus demonstrating to his mind that such pulsations are due to transmitted brain movements. He concluded that it was of no diagnostic value. In one of his cases the sinus was full of pus, and pulsated markedly. In Whiting's first case (*op. cit.*) pulsation was plainly seen and felt, and the sinus was found to contain a fibrinous clot, which had proved firm and resisting to the palpating finger. In his second case, on the other hand, pulsation was neither seen nor felt, and yet practically the same condition existed in the sinus as in the previous case.

The fallacy of puncture of the sinus is evident, as blood may be extracted, though clotting has taken place with the formation of a parietal thrombus. There is the further risk of infecting the sinus by this procedure. Microscopic examination of the fluid withdrawn by the syringe may assist the operator by the detection in it of pus corpuscles. The difficulty of detecting the presence of early thrombosis is illustrated in a case recorded by Jordan (*Arch. f. Ohrenh.*, Leipzig, Bd. xliv. S. 3). When the sigmoid sinus was examined, after the patient's death, there was no evidence to the naked eye of the formation of a thrombus. It was only after microscopic examination that it was detected, and it appeared to have existed for some time.

PUBLIC HEALTH.

UNDER THE CHARGE OF

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THE HOSPITAL SHIP "BAY STATE" IN THE SPANISH-AMERICAN WAR.

THE *Boston Med. and S. Jour.* (19th January 1899) gives an interesting account of the fitting up of the Massachusetts Volunteer Aid Hospital Ship *Bay State*, and the work it did during the late war. She is the first hospital ship in the world fitted out by an aid association, and authorised under the articles of the Geneva Convention. She is a steamship of 200 ft. long with a gross tonnage of 776 tons, and before reconstruction was a local trader. There are two wards—one fore and one aft—each 66 to 68 ft. long, with an average width of 24 ft., and having accommodation for fifty patients. There are two smaller wards to accommodate thirty-eight patients. In the operating room means are provided for asepsis and antisepsis. Near this is the laundry and a room for sterilising bedding, clothing, etc., by high-pressure steam. An ice room, capable of producing five tons per day, is provided, and also water-distilling plant to produce 3000 gals. pure water per day. Efficient ventilation of the wards is secured by an extraction fan 6 ft. in diameter. The beds, consisting of metal frames with wire mattress and hair mattress above, are detachable, so that they can be used as stretchers for carrying the patients on deck or ashore. The medical staff consisted of the surgeon-superintendent, who had the entire charge except as regards the navigation of the ship; three surgeons; and six female and six male nurses. Three trips were made—one to Santiago and two to Porto Rico, bringing 101 patients from the former and 235 from the latter. There were no surgical cases. From Santiago 80 per cent. of the cases had malaria, 8 per cent. enteric fever, and 18 per cent. dysentery; while from Porto Rico only 6·8 per cent. (including some doubtful cases) had malaria, 27 per cent. typhoid, and 9·8 per cent. dysentery. Diarrhœa, either alone or as a complication, was present in nearly one-half of the whole cases. Among the men from Santiago, out of fifty-six examinations of the blood, the crescentic form of the *Plasmodium malariæ* was found in forty-nine. Among the men from Porto Rico, 115 examinations of the blood by the method of Widal were made, with a positive result in sixteen. The *Amœba dysenteriæ*, though sought for, was found in none of the cases of dysentery. There were sixteen patients convalescent from yellow fever, all from Santiago. Out of the whole 336 cases, there were three deaths in transit to Boston, but a number died in the hospitals there. Great precautions were taken to prevent spread of disease by direct infection. Every patient on arrival on the ship was bathed with weak corrosive solution and provided with a clean set of pyjamas, and his own clothes were sent to the disinfecting room. After each trip the wards were disinfected with formaldehyde and thoroughly washed. In enteric cases, after each trip the bedding, bed-pans, etc., were disinfected by steam. The stools were disinfected by chloride of lime, and the closets daily flushed with corrosive solution.

Medical Officers for School Boards.

Dr. Schmidtmann (*Vrtljschr. f. gerichtl. Med.*, Berlin, Bd. xvi.) reports to the Minister of Education at Berlin on this subject, and discusses the experience of Wiesbaden. Here, in 1895, a medical inspection of the children attending the elementary schools (*Volks und Mittelschulen*) showed that out of 7000 examined, 25 per cent. suffered from some bodily defect or were in bad health, some from infectious diseases. In the interests of the children, as well as of the School Board, they appointed four medical men, to whom they entrusted the following duties:—To medically examine every child on entrance to school, unless it brought a medical certificate from the family doctor; in the case of every unhealthy child, to fill up a card, giving detailed results of physical examination, state of the organs of sight, hearing, and speech, together with suggestions to the teacher as to any special treatment at school; to visit the school fortnightly for at least one hour, to see any children specially pointed out by the teacher, *e.g.* cases of suspected infectious disease, and to observe the conditions of the schoolrooms as regards cleanliness, lighting, ventilation, etc. As regards the children, he must suggest nothing in the way of medical treatment. The honorarium is £30 per year. The results have been so good as regards the increased health of the children, that two additional appointments have been made. The parents now make no objection, and the teachers do not look on the medical man as interfering with, but rather helping, them. Schmidtmann recommends the adoption of this practice elsewhere, and considers that the Prussian educational system would be improved by this attention to the health of the children attending them. Its application should be restricted to the lower schools, where attendance is compulsory. It is the duty of the School Board to insure as far as possible that healthy children be not affected by others at school.

VACCINAL IMMUNITY.

MM. **BECLERE CAMBON** and **MENARD** (*Ann. de l'Inst. Pasteur*, Paris, Dec. 1898 and Feb. 1899) continue their investigations on this subject. They had already shown that the blood serum of a heifer vaccinated ten to fifteen days previously had a certain protective power against vaccination. If a heifer get a hypodermic injection of this serum immediately before inoculation with vaccine lymph, the characteristic eruption is not produced, nor is there any vaccinal virulence in the pustules that are formed. On the other hand, the injection of vaccine lymph must be made eight days before the vaccination to have any immunising effect. In their later communications they show that among heifers eleven or twelve days must elapse between injection with vaccine lymph and vaccination, to insure the *entire* disappearance of virulence from the vaccine, as determined by the non-production of a pustular eruption when it is used for vaccinating another animal. The immunising properties of the serum is not, therefore, due to the presence of living organisms, but to some chemical substance contained in it. Their method of investigation is as follows:—Two test-tubes, each containing a few c.c. of active vaccine lymph, are taken, and to the one is added the serum of the blood taken before vaccination, and to the other of that taken after vaccination. After forty-eight hours the serum is as carefully as possible pipetted off, and each of the samples of lymph used for vaccinating the same animal on different sides of the abdomen. The first of these gives the typical vesicles containing active lymph, while the second either shows no reaction, or only a slight inflammation, giving a little dried pus. Experiments on horses give similar results. As regards the human subject, nineteen persons were experimented on. One case, that of a woman 44 years of age, who had been vaccinated in infancy, but not revaccinated, may be taken. 20 c.c. of blood was drawn from the arm, and three days later she was vaccinated, the eruption being a typical one. Thirty-two days later another 20 c.c. of blood was drawn. The action of the two sera was tested as above described. The first had no anti-virulent action, showing that the effect of the vaccination had entirely disappeared; the second removed all virulence from the vaccine lymph. Similar results were got in three cases convalescent from smallpox. In one of these, which ran a usual course, and ended in recovery, the serum of blood drawn twenty days after the appearance of the eruption was strongly anti-virulent. The blood is anti-virulent, as a rule, on the twelfth day after vaccination. In smallpox cases the time varied. In some cases it was present, though not well marked, on sixth to eleventh day after appearance of eruption; in others, extremely well marked, in twentieth to twenty-seventh day. Immunity against vaccinal or variolar infection continues after the disappearance of anti-virulence from the blood. The time of disappearance of the anti-virulence varies. They found it present for twenty-five and even fifty years after vaccination or smallpox. The active part of the serum is evidently a globulin. Its power is not lost by the action of heat, light, or filtration through porcelain.

THE REMOVAL OF THE SEDIMENT FROM MILK.

EVERYONE is acquainted with the blackish sediment which is almost always seen at the bottom of vessels containing milk. This impurity is invariably got in the process of milking, and its amount is a good though somewhat rough measure of the uncleanly conditions of the byre, the cows, or the milking. Renk has shown that this dirty sediment consists largely of dried cow's excrement, and Flügge and others have shown that to it is due in large measure the presence of those spore-bearing, very resistant and peptonising micro-organisms associated with the causation of summer diarrhœa of children. In order to free milk from this sediment, Plaut (*Ztschr. f. Hyg.*, Leipzig, Bd. xxx. Heft 1) recommends that, immediately on delivery at the house, the milk be put into a cylindrical vessel having two side openings, one about the middle, and the other about 1 inch from the bottom, both of course closed with corks. After forty minutes' standing in a cool place, the upper portion of the milk, which contains most of the cream, is drawn off, and then the lower portion as far as the lower opening. This prevents the loss of cream which would occur if it were all drawn off at the lower opening. The two portions are then mixed. The milk left in the vessel contains little fat, and nearly all the sediment. The rest of the milk contains proportionately to the original milk a greater amount of fat, and is almost free from the peptonising organisms. He has found that milk, which even after boiling for ten minutes caused diarrhœa among the children fed on it, could be used quite safely after this treatment.

REPORTS OF THE MEDICAL SOCIETIES.

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Forfarshire Medical Association.—*Dundee, 7th April* 1899.—Dr. Macgillivray in the chair.—Dr. Macgillivray showed a patient with double upward coloboma, and a patient suffering from well-marked follicular conjunctivitis.—Two Röntgen ray photos of a compound comminuted fracture of the first phalanx of the right forefinger were shown by Dr. Pirie. The later photo showed a false metacarpo-phalangeal joint forming, and the patient had now fair movement. Dr. Pirie showed several other Röntgen ray photos taken by himself.—Professor Kynoch showed a papillomatous cyst of the ovary, removed by posterior colpotomy.—Dr. Foggie read notes of a case of acute yellow atrophy, occurring in a girl of 14. After gastric catarrh and jaundice for fourteen days, with occasional hæmatemesis, marked depression came on, and was succeeded by sudden and rapidly fatal cerebral symptoms, in which an initial delirium had given place to coma and tetanic convulsions. A petechial eruption was present both before and after the onset of the jaundice. No diminution in the liver dulness could be determined. Although a post mortem was not allowed, the diagnosis of acute yellow atrophy seemed warranted. He quoted the view, recently supported by W. Hunter, of the condition being a rare form of malignant jaundice of obscure infective nature, due to an organic poison, probably found in the intestine and acting on the liver after absorption; and he thought this view would assist in the explanation of the great clinical variation in the repeated cases.—Mr. Greig then gave a further communication on the two years' experience in the surgical out-patient room of the Royal Infirmary, and gave a summary of his cases and results.

OBITUARY.

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SIR WILLIAM ROBERTS, M.D., F.R.S.

We regret to observe the death of Sir **William Roberts**, which took place, after a long illness, in London on the 16th April. The son of Mr. David Roberts, who practised as a surgeon, Sir William was a native of Anglesea, where he was born in 1830. On leaving school he proceeded to University College, London, where he studied both in Arts and Medicine. After taking the degree of B.A., he became M.D. of the University of London in 1854, and at once commenced practice as a physician in Manchester. He passed successfully through the different grades of his profession until he became Physician to the Manchester Royal Infirmary and Professor of Medicine in Owens College. These appointments he resigned in 1889 in order to settle in London.

Sir William contributed largely to medical literature. The work with which his name will be most closely associated is that upon "Diseases of the Kidneys," that also on "Affections of the Digestive System." The deceased physician will be mourned by a very wide circle of friends.

JOHN FENTON EVANS, M.B.

Major Evans, I.M.S., Professor of Pathology in the Calcutta Medical College, fell a victim to the plague, after a week's illness, on 13th March. After graduating at Edinburgh in 1880, he was for five years attached to the Bristol School, but he entered the Indian Medical Service in 1886. He saw much active service in the field, and held several important posts in different stations, until his appointment, rather more than two years ago, to the office he held at his death. Much valuable work resulted from his labours, but a great deal of this is as yet unpublished. His early death is a great loss to scientific medicine as well as to the public service, and has caused widespread sorrow.

MEDICAL NEWS.

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THE NEW PROFESSORS.

But a few months have elapsed since we had the pleasure of mentioning the election of Professor **Muir** to the Chair of Pathology at Dundee, and it now falls to us to congratulate him on his transference to the larger field afforded by the University of Glasgow.

Mr. J. **Arthur Thomson**, who has been appointed Professor of Natural History in the University of Aberdeen, will leave a blank in Edinburgh which it will be difficult to fill. He goes to his new sphere with a great reputation for the excellence of his original work and the success of his teaching, while the charm of his diction has given him a world-wide celebrity.

To both of these gentlemen we desire to offer our most cordial wishes for their continued success.

ANALYTICAL REPORTS.

JOHANNIS LITHIA WATER.

(The Apollinaris Co. Ltd., 4 Stratford Place, London.)

SAMPLES of the "Johannis" lithia water from the springs of Zollhaus, Germany, have been sent us for comment. The waters supplied under this name fall to be placed in the category of those which are both natural and added to, while so free from any foreign flavour that they are able to be used for table waters. The Johannis Springs Company have availed themselves of an admirable suggestion, and added 1 gr. of lithium bicarbonate to each small bottle, and thus produced a natural lithiated water of decided therapeutic value. The "Johannis" water is strongly effervescent, almost tasteless, and an exceedingly pleasant example of a carbonated water, fit for consumption without admixture, or after the addition of spirits. It also possesses the advantage over alkaline waters, which contain salts of potassium, sodium, and calcium, in that it owes its alkalinity, apparent upon expulsion of the free carbonic acid, to bicarbonate of lithium. We here get what cannot be obtained in any other way, that is, a suitably constituted natural mineral water containing a salt of lithium in a definite and appreciable quantity, allowing of accurate dosage. It certainly is an undoubted advantage to be able to obtain a natural mineral water containing lithium bicarbonate in a proportion well suited for regular and continuous use by patients with a tendency to the over-production or under-excretion of uric acid compounds; the larger the amount drunk of it, the more the benefit which will accrue. It should prove of great service to gouty patients.

ELIXIR PHOSPHORI.

(O. E. Horn, Bournemouth.)

THE greater power of phosphorus in an uncombined form, when compared with the influence exerted upon the economy by phosphates or other phosphorus compounds, is well known in theory, but is generally not available in practice, owing to the difficulties attendant upon its proper pharmaceutical preparation. The Elixir of Phosphorus sent us by its deviser and manufacturer appears to surmount all the previous difficulties, as it contains that element, usually so intractable, in solution, in a free state, miscible with water, and compatible with both iron and quinine preparations. Its constitution is rather like a conundrum, for it has no chloroform or ether in it, "or any other known solvent." What the vehicle is which has been used to render phosphorus of a more sociable character, we cannot say, but the result is an excellent one. It is palatable, appears to be stable, and is indicated as a valuable preparation for exhibition in neuralgia, neurasthenia, and asthenic conditions generally, the more so as its ingestion is not followed by any unpleasant symptoms.

L'EAU D'OREZZA.

L'EAU D'OREZZA, from a spring in Corsica, is a carbonated ferruginous table water containing a proportionately large amount of ferrous carbonate in solution, about 128 mgrms. to the litre, ·0128 per cent., or about 2 grs. of the iron carbonate to each litre bottle. In addition to the iron salt, 2 litres of carbonic acid gas are reported to be contained in each litre of the water. The presence of so much of the protocarbonate of iron in this water (it is in greater proportion than any other we know of, Schwalbach, the next to it, containing 0·011 per cent.), as well as the absence of any unpleasantly marked chalybeate flavour, render it most suitable for ordinary consumption by patients requiring some extra *vis a fronte*. As it also contains a small amount of a salt of manganese, a metal believed by many to aid the pharmacological action of iron, it may very well be recommended for trial in cases of deficient metabolic or hæmic processes, and, in women, for disorders of menstruation.

As a sparkling water it aids the appetite and digestion, containing much easily assimilated iron, acts as a tonic; but, owing to its other constituents, possesses the great merit of not causing the constipation so often attendant upon the administration of iron salts in general.

NUCLEIN SOLUTION.

(Parke, Davis & Co.)

THE nuclein solution, prepared by Messrs. Parke, Davis & Co., contains 5 per cent. of nucleinic acid obtained from yeast. From an investigation into its composition, which we previously had occasion to carry out, the statements of the makers as to the proportion of nucleinic acid were confirmed *in toto*, and the solution as sent out by them affords the best and most effective means for the administration of an acid which is a fundamental component of the protoplasm of every nucleated cell, and which has proved of great service in the treatment of various diseases, such as diphtheria, phthisis, and debility, at the hands of different observers.

"TABLOID" FLUORESCIN.

FLUORESCIN is a most useful adjunct to the other resources of the ophthalmic surgeon in the matter of diagnosis. It has the property, when introduced into the conjunctival sac, of staining of a bright green colour any portion of the cornea which is deprived of its epithelium, the other parts of the cornea remaining untinted. It is thus very useful in cases where an abrasion of the cornea is suspected, or a superficial ulcer. But it has a further value, for when a more destructive form of loss of tissue is present, and it is decided to apply the cautery or other active agent to the advancing destructive area, fluorescin maps this area out in the most beautiful manner, enabling the surgeon to be precise in his application of the cautery to the whole ulcer and to the ulcer only. We have used the sample sent us by Messrs. Burroughs & Wellcome of their Tabloid Fluorescin, and found it to act as well as the solution, and as well as their tabloids of cocaine, which is no slight praise. We cordially recommend the tabloids to practitioners.

EUCASIN AND GUAJACETIN.

(Anglo-Continental Chemical Works Ltd., London.)

On a recent occasion, when the subject of artificial food-stuffs was dealt with among the Abstracts of Medical Progress, "Eucasin" was mentioned and its nature alluded to. We have just received a sample of this article of diet from the sole agents for it in Great Britain. "Eucasin" is a fine powder, formed by the action of ammonia on the casein of milk; it is in fact an alkali-casein, which is entirely soluble; it must be dissolved in cold water, and the pulp put into the hot liquid. It has an agreeable taste, and is by many regarded as one of the most valuable and assimilable in the class of artificially prepared foods to which it belongs; it also has the great merit of being cheap, compared with many of the other similar preparations. One of its benefits appears to arise from its power of increasing assimilation of the nitrogenous material in ordinary food, when it is given along with it, over and above the addition of nitrogenous substances supplied by itself. Salkowski has found that only 4 per cent. of the nitrogen in "Eucasin" is unabsorbed, a much smaller proportion than was found to be the case in some other preparations.

In combination with the second preparation sent us by the same firm, Guajacetin, "Eucasin" has yielded very promising results in the treatment of phthisis pulmonalis. "Eucasin" is now supplied in an improved form, with no smell, and with a still more agreeable taste than before, while Eucasin biscuits, cocoa, and chocolate are also prepared.

"Guajacetin" is derived from guaiacol, a carboxyl substitution derivative, but contains no actual guaiacol. It has given good results in the treatment of pulmonary diseases, and appears to have none of the irritating properties of creasote upon the gastric apparatus; in fact, it has been reported to have a considerable power in increasing the appetite. Eight grs. is the usual amount given, but 155 grs. may be safely taken in the twenty-four hours. "Guajacetin" can be obtained in the form of a white powder, or in tablets. As noted above, the combination of eucasin with it has been found to be very useful. Should further experience of "Guajacetin" prove it to be as effective in phthisis as creasote, where that drug is tolerated, and at the same time not to disturb digestion, it will certainly prove a boon to mankind.

MONTHLY NOTES ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,

MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

Meteorological Means for the East of Scotland District, for the Four Weeks ending April 1, 1899; and for the Year from January 1.

	Temperature.			Rainfall in Inches.		Sunshine. Hours.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees.		Actual.	Accumulated from Jan. 1.	Actual.	Accumulated from Jan. 1.	
		Above 42°.	Below 42°.					
Mean	39°·26	*[1] 69° [2] 133°	182° 531°	2·4	7·4	119	209	3°·0
Observed	39°·66	[1] 107° [2] 149°	182° 520°	2·6	8·4	134	241	3°·98
Difference	+0°·4	[1] +38° [2] +16°	± 0° −11°	+0·2	+1·0	+15	+32	+0°·98

Vital Statistics.

Mortality in Four Large Towns in the East of Scotland, during March, and since January 1. Actual Numbers and Rate per 10,000 per annum.

	Total.		Constitutional Diseases.		Circulatory System.		Nervous System.		Respiratory System.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	[1]1,270 [2]3,701	222·3 219·7	237 653	41·4 38·0	121 338	21·2 19·7	144 406	25·2 23·6	296 870	51·1 51·8
Actual Deaths and Rate per 10,000	[1]1,270 [2]3,849	222·3 228·3	253 717	44·2 45·7	104 342	18·2 19·9	145 396	25·3 23·0	255 826	44·5 48·1
Difference from Mean	[1]± 0 [2]+148	± 0 +8·6	+16 +64	+2·8 +3·7	−17 + 4	−3·0 +0·2	± 0 +10	+0·1 −0·6	−41 −44	−6·6 −2·6

	Digestive System.		Urinary System.		Pneumonia.		Phthisis.		Cancer.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	[1] 70 [2] 206	12·2 12·0	32 88	5·6 5·1	106 295	18·5 17·2	124 335	21·8 19·5	47 142	8·2 8·2
Actual Deaths and Rate per 10,000	[1] 72 [2] 227	12·5 13·2	35 89	6·1 5·1	111 355	19·4 20·6	129 366	22·6 21·3	50 160	8·7 9·3
Difference from Mean	[1] + 2 [2] +21	+0·3 +1·2	+3 +1	+0·5 ±0	+ 5 +60	+0·9 +3·4	+ 5 +31	+0·8 +1·8	+ 3 +18	+0·5 +1·1

* Note.—The figures [1] denotes the data for March, [2] for year to date.

The amount of light sunshine registered was much above the average, except during the second and fifth weeks. The warm third and cold fourth weeks give an increase of 27 per cent. and of 10 per cent.; the third showing 57 per cent. of the possible number of hours. The mean variability is above the average; in two of the weeks as much as 2°·4 and 2°·6, and in one 1°·3 below it.

To date, the warmth registered is still on the credit side, the cold below the usual figure, and 1 in. of rain in excess has fallen.

The total mortality for March was exactly that looked for, the deaths from circulatory and respiratory diseases being below the average, those from the other systems and diseases slightly above it. Influenza caused forty-five deaths, or 7·8 per 10,000. From the beginning of the year, the only death rates below the means are those under nervous and respiratory diseases; while the excess of deaths from pneumonia alone is only exceeded by that from constitutional diseases. The actual increase of deaths from pneumonia equals over 20 per cent. of the total calculated.

Although the mean temperature for the five weeks ending 1st April works out at a fraction over the mean, four of them taken together, the first three and the last, give a mean temperature 3° over the normal; the fourth week 10° below it. The third week of the series, ending 18th March, had a mean of 44°·9, or 6° above; the fifth week, of 42°·8, or 2° above; the mean of the fourth being as low as 30°·1, with a maximum of 48°, and a minimum for the East of Scotland district of only 3°. The maximum registered during the third and fifth weeks were 68° and 63°, the minimum 23° and 26° respectively. So there are differences of 14°·8 in mean and 20° in extreme temperatures, between the third and fourth weeks. At the same time the warm week was rainless; the cold week shows nearly double the usual precipitation. The cold snap was caused by the appearance of several small depressions passing over the North Sea to the east of our islands, causing northerly currents of air to flow over them.

Thus March, characterised by warmth, save for one week of unusual cold, a considerable excess of day-degrees over 42°, and an average number of these below 42°, a normal rainfall, much sun, and a markedly increased variability of temperature from day to day, shows an average death-rate, with diminished deaths from respiratory and circulatory diseases.

The year to date, of average temperature, increased rainfall and sunshine, shows an increase in mortality, most marked under constitutional diseases and pneumonia.

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THE TREATMENT OF RENAL DROPSY.

By NESTOR TIRARD, M.D. (Lond.), F.R.C.P., *Physician to King's College Hospital, and the Evelina Hospital for Sick Children, London.*

WHEN the British Medical Association met in Edinburgh, last July, I had the honour of introducing a discussion on the "Treatment of Chronic Renal Disease." During this discussion, the views expressed by various speakers indicated considerable divergence of opinion upon many matters that I had previously deemed to be generally agreed upon, and accordingly, as the time for reply at these meetings is necessarily limited, I have gladly availed myself of this opportunity of restating the general principles of treatment, which I consider advisable in the course of kidney disease.

In the following short course of papers I propose to deal with some of the more prominent symptoms of both acute and chronic kidney disease, and I have the less hesitation in taking this broad view of the subject, since the limits originally assigned to the discussion were not strictly adhered to by many of those who spoke; and, moreover, it seems to me that it may be serviceable to consider the treatment both for acute and chronic conditions. In the present paper I propose speaking chiefly of the treatment of dropsy, resulting from renal disease.

Renal dropsy occurs in two perfectly distinct forms: in the form associated with acute nephritis, where the œdema is general but often makes its appearance first about the face; and, secondly, in connection with chronic nephritis, where the œdema, although occasionally general, is more frequently of a type resembling that met with in connection with cardiac failure, or with respiratory troubles.

The dropsy of acute nephritis develops rapidly, and is associated with marked diminution in the daily excretion of urine and with well-known alterations in the urine, such as the appearance of blood, or the presence of lithates, and, when examined microscopically, the presence of free blood corpuscles, of blood casts, and of renal epithelium, which may be entangled so as to form epithelial casts. With these conditions patients may complain of sickness and of severe headache, while very commonly the diminished urinary excretion is associated with abnormal frequency of micturition, small quantities of urine being voided at a time with much discomfort.

The above indications form a fairly complete picture of the onset of an attack of acute nephritis, whether it results from cold, or whether it occurs as the sequel to an attack of scarlet fever; and the symptom which, in all probability, is the first to attract attention is the dropsy, which, in a few hours, completely alters the appearance of the patient, the face becoming puffy and pale, owing to the œdema in the subcutaneous tissues, the features being obliterated and the eyes almost closed. This alteration in the features is accompanied by general effusion in the subcutaneous tissues all over the body, so that the extremities and the trunk become swollen and disfigured, while movements are rendered difficult, owing to the infiltration of the subcutaneous structures round the joints.

Although the above account represents the first appearance of the dropsy of acute nephritis, after a few days the fluid tends to gravitate, so that, while the tissues over the chest and abdomen appear of more normal shape, there will be even greater distortion or deformity of the loins, with the development of the lumbar pad, which is, however, perhaps rather more characteristic of the dropsy of chronic nephritis.

To a great extent, the degree of swelling corresponds with the diminished excretion of urine; hence the continuance of renal dropsy is to be regarded with considerable distrust and anxiety, for although the dropsy does not, in itself, threaten life, yet as an indication of the probable extent of engorgement of the kidneys in acute nephritis, our efforts in dealing with this disease must primarily be directed to the relief of dropsy.

With acute nephritis there are three dangers to be kept well in mind. These are—(1) The immediate danger of suppression of urine; (2) the danger of interference with respiration and with the circulation; (3) the more remote danger of chronic renal changes, which will, in all probability, result from the accumulation of effused blood around the capillaries of the glomerulus and within the renal tubules.

In considering these three sources of danger, it will be found that the most satisfactory treatment is that by which the dropsy is reduced most rapidly; and indeed, though I have headed this paper "The Treatment of Renal Dropsy," it is necessary to remember that to a large extent, in the treatment of renal dropsy, we must include a consideration of the general management of acute and chronic nephritis.

The treatment of the dropsy of acute nephritis must include measures which will meet the threefold dangers above indicated; and since the most imminent danger is that resulting from suppression, this demands the first consideration.

To obviate the dangers of suppression, it is necessary to restore the interrupted functions of the kidney as early as possible, so as to favour the renewed elimination of nitrogenous waste. Since, however, the arrested action is associated with extreme engorgement of the renal vessels, any measures calculated to increase this engorgement must be deprecated; and our efforts must primarily be directed towards the relief of the results of the engorgement, and at the same time to the removal of the existing over-distension of the renal vessels.

The three main channels for the removal of fluid from the blood vessels are the skin, the intestine, and the kidneys; and their respective excretory functions can be stimulated by diaphoretics, by hydragogue purgatives, and by diuretics. The employment of diuretics is contra-indicated in the early stages of acute nephritis, since the majority of these remedies would cause an increase in the calibre of the renal vessels, and would thus promote the engorgement which it is desirable to relieve. Hence, so far as remedial measures are concerned, at the commencement of treatment we are almost entirely dependent upon diaphoretics and upon hydragogue purgatives.

I am well aware that some observers, and notably Sir William Roberts, advocate the early use of drugs intended to render the urine alkaline, hoping in this way to prevent the coagulation of blood or of albumin within the tubules. When such coagulation arises it undoubtedly adds largely to the risks of the subsequent development of chronic nephritis, and coagulation is certainly more likely to occur when the urine is highly acid. It has also been urged that not only remote, but also immediate dangers are diminished by this form of treatment, and Sir William Robert states that in no instance, where the urine has been rendered alkaline during the first week of the complaint, has he observed the more severe uræmic symptoms or secondary inflammations. To effect this object it is necessary to employ either potassium acetate or citrate, or the salts of sodium, which are converted into alkaline carbonates within the body. These compounds have, however, a powerful diuretic action, and I consider, therefore, that their use before the initial engorgement is relieved is not devoid of risk. They are valuable when an increase in the urine eliminated, and a decrease in the blood-red colour indicate that the primary engorgement has subsided, and that the circulation through the kidney is again tending to the normal condition; but, so long as the urine is blood-stained and scanty, I prefer to rely upon other measures for the relief of the more urgent symptoms.

By securing rapid action of the skin we are generally able to insure diminution of dropsy, and thus to afford relief from severe and persistent headache, which is so frequently a prominent symptom. The action of the skin may be favoured by baths, by drugs, and to some extent by copious draughts of liquids. The measures adopted must to a great extent be determined by the circumstances of the patient, and by the amount of nursing assistance which is available; but a little ingenuity will readily overcome difficulties, and permit the early employment of almost any desired form of treatment. Rapidity of action may often be secured by the use of the hot-air or vapour bath, and it is in this connection that the greatest amount of ingenuity will be required. In a well-appointed hospital it is easy to administer a vapour bath, since the necessary apparatus is always at hand. This consists of a large steam-kettle, which is placed at the foot of the bed, with its tube projecting a short distance through a wooden partition into the space at the foot of the bed. The bed-clothes are raised around the patient by a large cradle, similar to that employed in the surgical treatment of fractures of the lower extremity. Two of these cradles, if necessary, may be used so as to increase the space surrounding the patient, and the cradle is then covered with blankets which are closely fitted round the patient's neck, so that his body is completely surrounded with warm, aqueous vapour. It is necessary to fix the tube of the steam kettle in a position where it can by no possibility come into contact with the patient's limbs, since otherwise considerable damage may be done, the cutaneous sensibility of dropsical patients being so greatly reduced that severe blistering may result before there is any complaint of discomfort.

In cottages the vapour bath may be sometimes employed by seating the patient in a chair, under which a lamp with a large wick is allowed to burn, the patient being surrounded with blankets. This proceeding is, however, extremely hazardous, as there is danger of the lamp flaring and setting light to the woodwork, or to the bed-clothes. But, independently of this danger, this form of vapour bath is objectionable, since it requires a patient to be placed in an erect posture when, in all probability, he is in a condition of extreme weakness; and dropsical patients are notoriously heavy and difficult to move. The vapour bath, however, may, with a little ingenuity, be easily arranged, even in a cottage; the bed clothes may be temporarily raised sufficiently by low stools, such as are to be found in most country cottages, or, if these are not available, a few barrel hoops and a pole may be tied together so as to form a good imitation cradle. Yet even then, there is the difficulty of arranging the lamp with safety, and in general it is felt to be easier and safer to promote the action of the skin by the use of wet packs or by warm baths; of the two, the wet pack is far more easy to manage, since it involves no apparatus, and entails less movement of the patient.

The wet pack may be either hot or cold, and it does not appear to be of much importance which form is adopted. If the cold pack is employed, it is only really cold at the moment of its application, since, as a rule, the cold speedily excites a strong reaction, which serves to warm the sheet and to surround the patient with warm vapour. On the other hand, the hot pack may sometimes be applied injudiciously warm, and thus cause pain, or even damage to the skin by scalding, or it may produce a sense of cold as the temperature falls more nearly to that of the body; but, as in the case of the cold pack, the resulting vapour which envelopes the body will speedily be formed.

In applying the pack, the patient should be completely undressed, or should only wear a thin cotton night-dress, and the wet sheets should be wrapped round and then quickly covered with successive layers of blanket, so as to envelop the patient completely with the exception of the head, care being taken to leave no portion of the damp sheets projecting beyond the blankets. The time spent in the wet pack must, to some extent, vary according to its effects. Twenty minutes or half an hour is usually sufficient to induce copious perspiration, and when the action of the skin has been well started, continued diaphoresis will follow after the removal of the pack. When the sheets are removed the surface should be quickly dried with warm towels, and the patient should be again enveloped in warm blankets.

If, however, there are facilities for employing a warm bath, and if the patient is sufficiently well to be able to remove readily from the bed to the bath and back again, the bath may be used, the water being at a temperature which can be tolerated with comfort by the hand and arm of the nurse; but after the bath it is desirable to promote further diaphoresis by wrapping the patient in warm blankets, directly after he has been quickly dried.

When any one of the foregoing measures is being adopted, free perspiration may be encouraged by allowing the patient to drink copious draughts of water from time to time, while further action may sometimes be encouraged by the administration of full doses of liquor ammonii acetatis. It frequently happens, however, that during the first or even the second employment of any of the above measures, the skin does not act very satisfactorily, and the practitioner must then be guided by his judgment and by observation of the patient as to further treatment. In young adults, when the dropsy is the result of acute nephritis, it is ordinarily advisable to employ hydragogue purgatives, and to repeat the use of the hot bath, or other diaphoretic measure, some twelve or twenty-four hours later. On the other hand, when the subject affected by renal dropsy is a man of middle age or of advanced years, it will ordinarily be found that if the bath fails to afford immediate relief, it commonly produces much headache, and indeed it may have to be discontinued when the skin does not act, or acts but feebly. Since, however, there are great advantages in the free diaphoretic action of the skin, it is desirable in some cases to endeavour to stimulate diaphoresis by the hypodermic use of pilocarpine. It is sometimes found that relatively small doses of the pilocarpine nitrate will suffice to initiate diaphoresis, which can then be increased by the use of the wet pack or of the hot bath. This drug, however, requires to be employed very cautiously, since although by its use considerable diaphoresis may be produced, it also has the power of promoting excessive action of the salivary glands, and apparently also increased secretion from the bronchial mucous membrane. Therefore it is not to be employed with patients who are so far comatose that they are unable to expectorate, nor should it be used with those who are already suffering from œdema of the lungs, or when there are indications of cardiac weakness.

If diaphoretic measures afford little relief, or if the relief appears to be purchased at the cost of great suffering, it is well to continue the use of hydragogue purgatives, such as the compound jalap powder of the pharmacopœia, or of a mixture of jalap powder with scammony powder; and I have observed that the action of these hydragogue purgatives will frequently be followed by considerable diminution of dropsy, and by gradual increase in the amount of urine that is passed.

In the dropsy of acute nephritis the use of diaphoretic measures is scarcely as imperative as it is in cases of dropsy associated with subacute attacks occurring in the course of chronic nephritis. In acute nephritis, especially in the form resulting from scarlet fever, free purgation is generally followed by rapid diminution of dropsy.

Sometimes, in cases of renal dropsy, the use of mercurial preparations has been advocated, with the view of promoting free purgation, but although mercurial preparations are of great service in selected cases, the selection requires considerable discrimination. In my opinion, it is advisable, as a rule, to defer the use of mercurials until the urine shows signs of improvement, indicated more particularly by a diminution in the amount of blood; the reason of this postponement being that mercurials act not only as purgatives, but also as diuretics, and, for the reasons already given, it is undesirable to employ diuretics so long as free blood is to be found in the urine, since this indicates a condition of active engorgement which can only be increased by the use of stimulant diuretics.

In more chronic cases, however, when the dropsy persists after the urine has regained the customary pale tint characteristic of chronic kidney mischief, mercurials may be of considerable service. It is indeed in this class of cases that other diuretics may also be used, there being no objection at this time to the use of the alkaline diuretics previously mentioned; as, for example, the acetate or citrate of potassium, or of sodium, which, by their conversion into alkaline carbonates within the body, tend to diminish the acidity of the urine, or even to render it alkaline. These preparations may be used in subacute cases as soon as the urine becomes somewhat more copious and of a lighter tint, while in chronic cases they may be employed throughout the treatment.

Occasionally the risks of suppression are independent of any dropsical effusion, although in general the diminution of the daily excretion of urine bears a definite relation to the increase of the dropsy. In some severe cases of acute nephritis, however, symptoms of suppression may arise before any noticeable dropsical effusion has occurred, while, in cases of chronic nephritis, suppression may ensue in the course of other maladies, or subsequent to operations undertaken for the relief of other diseases. Occasionally symptoms of suppression will rapidly follow the injudicious employment of various drugs, and foremost amongst these must undoubtedly be placed morphine.

In speaking of the use of this drug, at the meeting in Edinburgh, I indicated that my distrust of morphine in connection with renal diseases at that time rested mainly upon the observations of others. Since then I have unfortunately witnessed most serious symptoms after the hypodermic injection of morphine, after a surgical operation on a patient who was already the subject of chronic nephritis. The dose of morphine employed was one which had frequently been used previously in this case with the view of alleviating pain, but after the operation, which was of a formidable character, the same dose apparently sufficed to cause arrest of secretion, with increasing headache and coma, which ultimately proved fatal, in spite of the successful employment of diaphoretic measures.

In some cases of acute nephritis the dropsical effusion may not only affect the subcutaneous tissues, but may also occur in connection with the submucous tissues and with the serous membranes, and in this way very formidable complications may arise, the most serious being that known as œdema of the glottis. With this form of œdema the breathing becomes obstructed, and the difficulty of respiration increases rapidly. As with other obstructive diseases of the larynx, the danger is extreme, and calls for the immediate employment of energetic measures. Relief may sometimes be afforded by scarifying the aryteno-epiglottidean folds, but this operation frequently fails to give permanent relief, and it becomes necessary either to intubate or to perform tracheotomy.

The performance of intubation demands some experience, as well as the possession of the appropriate instruments. Intubation is, however, gaining in favour, and the manual dexterity required is now possessed by many practitioners, who have been led to employ this method of relieving laryngeal obstruction in cases of diphtheria since the introduction of the antitoxin treatment. It must be admitted, however, that both intubation and tracheotomy, although theoretically they should suffice to overcome obstruction of the larynx, which is alone indicated under the term œdema of the glottis, yet may fail to save life, since the œdema is in many cases by no means confined to this region.

In a child recently under my care at the Evelina Hospital, each of these operations was performed in turn. The child was only 3 years of age, and the history of scarlet fever rested on an uncertain basis, the only fact in favour of this hypothetical commencement of the disease being, that one of the boy's sisters was at the same time in another hospital with acute nephritis. The mother had only noticed that the child was unwell three days before it was brought to the hospital, her attention being attracted by finding that the child's face was swollen all over, the eyes being nearly closed. On the following day the swelling became more diffused, the feet and legs being affected. The child appeared to be well nourished, but his face was puffy and pale, and there was general œdema extending over the body, the œdema being greater over the sacral area. On admission, slight impaired resonance at both bases of the lung was noticed, together with some few râles, and the sounds of the heart were somewhat altered. The cardiac impulse was in the nipple line, and the first sound was prolonged. The urine at this time was very scanty in amount, and contained both blood and albumin. A hot bath was given for ten minutes, but did not produce any sudorific action. The following day the dyspnœa increased, and appeared to be laryngeal in origin. There was considerable sucking in of the lower costal margin, and the child's lips were blue and the face pale. Accordingly, intubation was performed, and the immediate result was extremely satisfactory. The child went to sleep quietly, and his distress became much less, although the rate of respiration remained the same as before. The following day the tube was removed, but two and a half hours later the breathing became more and more difficult, and though the intubation tube was replaced, the distress continued. Accordingly, tracheotomy was performed, and although air passed freely through the tracheal opening, the general condition did not improve, and as the difficulty of respiration continued, a hot air bath was used. The skin then commenced acting slightly, but the pulse gradually became weaker, and the breathing more urgent and stridulous. In spite of the hypodermic introduction of ether, brandy, and strychnine, and in spite of the attempt to relieve the heart by bleeding, the pulse became weaker, and the child died.

Apart from the changes in the kidney, the chief features noticed subsequently were acute œdema of the epiglottis and aryteno-epiglottidean folds, extending thence down the trachea to the smaller bronchi, so that the whole of the lung was dark in colour, heavy and œdematous, and, on division and pressure, exuded considerable blood-stained fluid. In this child, undoubtedly, the œdema extended throughout the air passages, and although intubation and tracheotomy afforded some relief, this relief could not be expected to affect the œdema of the trachea and the finer divisions of the bronchi.

Œdema of the lungs is by no means uncommon with renal dropsy, even though there may have been no œdema of the glottis; indeed, in cases of advanced nephritis, œdema of the lung is a more frequent and more formidable complication. The œdema is sometimes replaced by passive effusion into the pleural cavity, so that the physical signs may resemble those of pleuritic effusion, although ordinarily there is no rise of temperature, and but little complaint of pain. These two complications, œdema and hydrothorax, add largely to the risks of renal dropsy, and a third similar complication connected with the pericardium is sometimes superadded.

The treatment of these complications involves some difficulty, since their passive character, to a great extent, hampers the use of those remedies ordinarily employed for similar respiratory or circulatory troubles. Practically, the œdema and hydrothorax must be regarded merely as forms of dropsy which are to be dealt with by diaphoretics, by hydragogue purgatives, and by diuretics, on the same principles which govern the treatment of dropsy of renal origin, while the group of so-called expectorant remedies will be of little or no service. It remains, however, to add that, in dealing with hydrothorax, the question of operative interference will necessarily arise, since it is easy to withdraw fluid from the thorax by means of an aspirator, and the relief which is thus afforded is often immediately apparent.

This mention of operative interference brings me to the consideration of the advisability of early operations in these cases. Certainly in hydrothorax, associated with renal dropsy, there is no advantage in postponing paracentesis, other than the desire to fall in with the natural reluctance of the patient to face any operative measure. But, on the other hand, when a pleuritic effusion, even of passive character, has been allowed to remain any length of time, there can be no doubt that absorption is rendered more difficult by alterations in the vitality of the epithelial lining of the pleural cavity, and also within the capillary walls.

I agree most cordially with the remarks made by Professor

Ewald concerning the desirability of removing this effusion early, and, if necessary, frequently. There is, however, some doubt in my mind whether it is possible to lay down any hard and fast rule as to the right time at which operative measures should be undertaken for the relief of dropsical effusion in the extremities. Certainly, so far as acute cases are concerned, when there is every probability that the capillary walls are intact, I prefer removing fluid, if possible, by medicinal agencies, or by diaphoretic measures, while reserving operative measures either for chronic cases, which show no signs of improvement with diaphoretics and purgatives, or for cases of acute renal dropsy, where the effusion is rapidly increasing and the skin fails to act.

Concerning the operative measures to be adopted, I prefer the employment of several of Southey's tubes, which have previously been rendered aseptic. These tubes may be used, as Ewald recommends, by being passed some distance into the subcutaneous tissues, as nearly as possible parallel to the skin, and their employment is not generally associated with any particular risk. On the other hand, more rapid subsidence of dropsical effusion may be secured, either by acupuncture over the lower extremities, or by making linear incisions over the malleoli. Both of these methods have certainly the advantage of simplicity and rapidity in their favour, but they involve the trouble of collecting the fluid which is poured out, by means of sponges or cloths placed around the limbs, and they also entail the inevitable discomfort of these damp surroundings, which may perhaps favour inflammatory action. It is rare after the use of Southey's tubes to find any indication of inflammation, but it is extremely common for incisions over the ankles, or the performance of acupuncture, to be followed by considerable erythema, or even by some sloughing of the edges of the wound.

Hitherto attention has been directed principally to the relief of dropsy, of hydrothorax, and of œdema of the lung. There is, however, in these cases, great risk of failure of the circulation, independently of dropsical effusion into the pericardium. Such failure, which is indicated by increasing weakness and irregularity of the pulse, has to be combated by the use of stimulants, which should be given by the mouth so long as consciousness is retained, and may be injected subcutaneonsly when the condition of coma renders swallowing difficult. In extreme cases it will be advisable to stimulate the heart's action still further by the hypodermic injection of strychnine and digitalin.

When increasing dulness over the cardiac region with diminished clearness of the sounds of the heart, indicates that there is much fluid accumulation within the pericardium, paracentesis of the pericardium must be considered. In these cases of passive effusion this operation involves very little risk. I have repeatedly ordered the pericardium to be tapped in children, without any ill result, even though on more than one occasion the aspirator has obviously entered the ventricle. Certainly, I feel that the risks which follow inaction in such circumstances are greater than those dependent upon operation.

In addition to other measures, some of the immediate risks due to acute nephritis, and also the more remote danger of the establishment of chronic nephritis, must be treated by modifications of diet. These modifications, which are necessary in every case of acute nephritis, whether dropsy is present or not, will be considered more conveniently in the course of a subsequent article. I reserve also, for future consideration, the treatment of the œdema which occurs towards the end of many cases of chronic nephritis, and appears to be dependent upon a gradual development of cardiac weakness. This form of renal dropsy is frequently but little affected by the measures already described.

AN OUTBREAK OF TYPHUS FEVER.

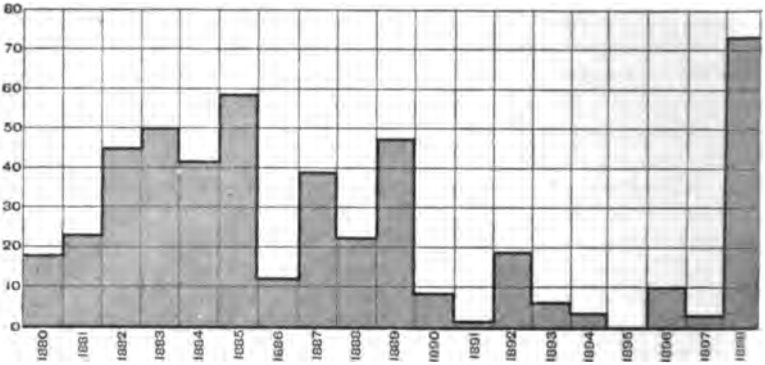
History—By HARVEY LITTLEJOHN, M.A., M.B., B.Sc., F.R.C.S.Ed.
Clinical Features—By CLAUDE B. KER, M.D., M.R.C.P.Ed.,
Superintendent, City Fever Hospital, Edinburgh.

HISTORY.—Murchison in his classical account of the continued fevers makes the remark that a complete history of typhus would be the history of Europe during the last three and a half centuries. With equal truth it may be said that no history of Edinburgh, of its development and of the conditions of life which existed up to a period within the memory of us all, could be written without frequent reference to this disease, which was at one time practically endemic in the Old Town; and annually desolated many homes,. not only amongst the poor but also in the ranks of our own profession. Owing, however, to the general increase of trade and the improved social condition of the working classes, which higher and more regular wages produced, as well as to the vast progress which has been made within recent years in all that affects the health of individuals and of communities, epidemics of typhus fever have practically disappeared, and even an outbreak such as we propose to describe must be accounted an exceptional event in the sanitary history of the city.

Accurate information in regard to the prevalence of the disease, previous to the introduction of the Edinburgh Notification Act of 1879, is impossible to obtain, but the following table taken from Murchison gives the number of patients admitted into the Royal Infirmary during ten years, 1862–1871; and when we remember how few of the actual number of cases of the disease could have been treated in that institution, an idea is afforded of how widespread the infection was a quarter of a century ago.

1862	14	1867	303
1863	74	1868	280
1864	212	1869	259
1865	447	1870	287
1866	847	1871	101

Since 1880, however, we have accurate records, and in the table below are given the number of cases and the mortality for each year, while the chart indicates even more clearly the history of the disease during the last nineteen years.



YEAR.	TYPHUS.		
	Cases.	Deaths.	Rate per cent.
1880	18	7	39·0
1881	23	14	60·8
1882	45	10	22·2
1883	50	16	32·0
1884	42	16	38·0
1885	58	10	17·2
1886	12	4	33·3
1887	38	11	28·9
1888	23	5	21·7
1889	46	9	19·5
1890	7	1	14·3
1891	1	...	...
1892	18	3	16·6
1893	6	1	16·6
1894	3	1	33·3
1895	...	...	...
1896	10	3	30·0
1897	3	1	33·3
1898	79	9	11·3
Totals	482	121	25·1

During the ten years, 1880–1889, there were 355 cases notified, and in no year was there ever less than ten cases, the average number being thirty-five. During the subsequent nine years, 127 cases have been notified, being an average of fourteen yearly; while, if we discard for the moment the seventy-nine cases which occurred last year, the average number of cases from 1890 to 1897 was only six, and only twice did the annual number of notifications reach double figures.

A still more remarkable contrast between the first period of ten years and the last of nine years is seen if we compare the death-rates of these two periods. From 1880 to 1889, 102 deaths occurred, equal to a death-rate of 28 per cent.; while in the subsequent nine years there were only nineteen deaths, giving a percentage of fourteen, or exactly half the death-rate for the ten years, 1880–1889. As a result of this gradual disappearance of typhus fever, the younger generation of medical men have little or no acquaintance with the characteristic features of the disease, and few of the students who have graduated in Edinburgh during the last twenty years can have had the opportunity of becoming practically conversant with its clinical aspects. That such want of knowledge may have disastrous results, is exemplified by the experience of last year's outbreak, and by many small outbreaks which have occurred recently in various parts of the country. For this reason, and also because the recent cases present some features of special interest, we venture to bring before you the following account.

In the first week of October four cases were sent into the City Hospital suffering from what was thought to be typhoid fever, although in one case the diagnosis was stated to be doubtful. All of the cases came from the neighbourhood of Simon Square, and resided within a hundred yards of each other. In the following week three cases were admitted to the Fever Hospital, two of them from public hospitals, and all resided in the same neighbourhood. One had been notified as a case of typhoid, another as scarlet fever, while in the third the suspicion of it being typhus was mentioned. Unfortunately, all of these patients had been ill for some time, and presented only fading rashes, but their whole appearance raised the suspicion that they were probably cases of typhus, and accordingly they were placed under observation in a separate ward. During the second week this suspicion became confirmed, owing partly to the localised distribution of the cases and to the negative results of Widal's reaction. Much valuable time had been lost through the failure of the medical attendants to recognise the disease, and all of the first seven cases were only received into the Fever Hospital when the disease was well advanced, while three of them had been sent in the first instance as patients to other hospitals and kept there for some days.

As a result of this delay, a much wider dissemination of infection occurred than might otherwise have taken place, and the number of cases increased during the remainder of October and in November. During December they again decreased, the patients during this month coming for the most part from already infected houses, and by the middle of January the disease had completely disappeared.

The following table shows the number of cases which occurred during each week :—

Week ending October	8	4 cases.
„ „ „	15	3 „
„ „ „	22	4 „
„ „ „	29	4 „
„ „ November	5	13 „
„ „ „	12	6 „
„ „ „	19	13 „
„ „ „	26	12 „
„ „ December	3	5 „
„ „ „	10	6 „
„ „ „	17	4 „
„ „ „	24	5 „
„ „ „	31	... „
„ „ January	7	2 „
„ „ „	14	1 „
		82

Origin and distribution of the cases.—A "de novo" origin of the disease may at the end of the nineteenth century be at once discarded, and if one fact was more clearly brought out by our investigation of the present cases than another, it was that practically the disease is only communicated by direct contagion, and that even persons who are residing in the same tenement with an infected family, and who are living under conditions most favourable to receive infection, namely, dirt, squalor, and intemperance, do not become infected unless they have been in close contact with the patient.

It is true that we are unable to point to the first case of the disease in the City or explain how it was introduced, but this is not to be wondered at, when we consider the kind of life led by the lowest class of the population, their wandering habits and chance acquaintanceships. The time during which the disease must have existed before its true nature was recognised was also a serious obstacle to the discovery of its origin. The presence of the disease in many towns and districts of Scotland about this time is, we hold, sufficient ground for maintaining that the infection was almost certainly introduced from outside into the city, without being compelled to fall back upon a "de novo" theory.

The disease had probably existed for several weeks before it was recognised in October. The grounds for this presumption are the simultaneous occurrence of several cases at the beginning of October, and also the information which was elicited in our investigations, that one or two cases of severe "influenza," which confined the patients to bed and left them very weak, had occurred in the families which furnished the first true cases of typhus. It appeared also that a man residing in one of the worst parts of the infected area died at the commencement of September of "pneumonia," and that a wake which lasted for two days was held in connection with the body. The house where this occurred was small and of the most wretched description, and, according to the account of some of those present at the wake, the room was continually crowded. There is reason for suspecting that the man died of typhus fever, but whether this is so or not, it is a curious coincidence that at least three cases of so-called influenza occurred shortly afterwards amongst those who had been present at the wake, and later on three others were struck down with undoubted typhus.

As regards the *distribution* of the cases in the city, with a few exceptions they occurred within a radius of a quarter of a mile of Simon Square, and amongst families who were in constant intercommunication.

The cases which occurred outside of this area were for the most part clearly traced to have received the infection from persons residing within it, but in one or two instances, notably a series of four cases in one family residing in Portobello, no direct communication could be discovered. The man was a hawker, and could give no very clear account of his movements or those of his family, so that the possibility of the disease being connected with the Edinburgh cases could not be excluded.

Age incidence.—The following table shows the ages of those attacked, and the remarkably large proportion of children affected must strike any one acquainted with previous statistics. Children under 10 years of age constitute over a quarter of the total cases, and are more numerous than the patients at the two age periods which usually supply the greatest number of attacks, namely, 15 to 20, and 20 to 25:—

Under 5 years	7
5 to 10 "	15
10 to 15 "	6
15 to 20 "	8
20 to 25 "	11
25 to 30 "	8
30 to 40 "	13
40 to 50 "	10
50 and upwards	4
Total	82

As regards *sex*, the cases were pretty equally divided, 38 males and 44 females being attacked.

Mortality.—Ten cases, or 12 per cent., ended fatally. This is a very low death-rate compared with the mortality given in connection with former epidemics, and I think it must be attributed, in part at any rate, to the fact that all the cases were treated in hospital, with the exception of two which were too ill to remove, and afterwards died. No death occurred under 15 years of age; indeed, the predominating factor in regard to prognosis was not so much the age of the patient, as the existence of a previous life of intemperance.

Infectiousness.—Typhus is very infectious under certain favourable conditions, but its "striking" distance is very short. This point was repeatedly brought before us by the fact that a patient in a crowded tenement was not a source of danger to other families, so long as members of these households did not actually enter the room where the patient was lying. In all the cases where the source of infection was traced, the patient was found to have been in actual contact with a previous case, while, without exception, families living in a common stair, in which there was an infected house, escaped the disease so long as no direct communication with the patient took place. Members of the patient's family might mix with other families in the stair or outside the house, apparently with impunity. This is an interesting fact, but it must not be used as an argument against the isolation of all cases of the disease in hospital, since in the class of the population amongst which typhus fever occurs, visits from relations and friends to the infected house are constantly taking place, and are difficult to prevent.

An illustration of this was afforded in a striking manner by what occurred to the following family:—

One of the first cases in October was a woman who lived along with her husband and family of six children in Paul Street. Little information could be obtained as to the existence of any friends or relations, who might have visited the infected house, and whom it was therefore important to keep under observation. The required information was, however, soon forthcoming by other means, as the following family tree indicates, and the dissemination of infection to which this patient gave rise was the means of discovering the intricacies of a family relationship, of which even the members themselves had lost count.

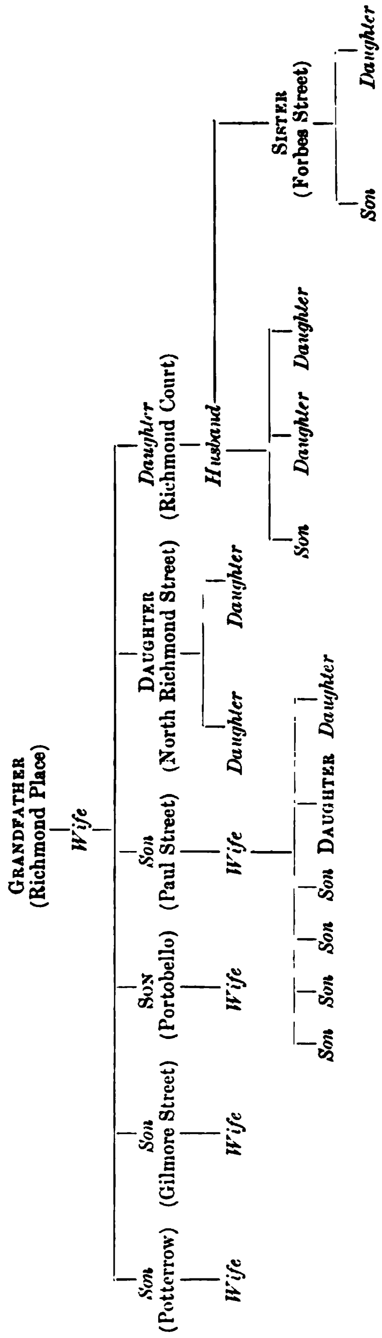
The wife infected her husband, and five of their children subsequently suffered from the disease; the husband's mother, living in Richmond Place, the families of his three brothers and of two sisters, all residing in different streets—one of the former lived as far off as Portobello—were all infected, as well as the family of a sister-in-law.

In all, twenty-two members of the family suffered from typhus fever, and seven died. They resided in eight different tenement blocks, each crowded with inhabitants, and yet in one instance only did another family living in the same tenement become infected, and in this case it was fully proved that this was the only family in the tenement which had entered the infected house, and was on friendly terms with the inmates.

Dirty houses, deficient cubic space, and want of ventilation greatly increase the infectiousness of typhus; while, under the opposite conditions, the risk of anyone taking the disease from a patient is much reduced. In support of this proposition, it was observed that the great majority of the cases occurred amongst people who were living under the most insanitary conditions of overcrowding and uncleanliness, and that the fever was much more frequently spread to others from these cases than it was from the cases which occurred in clean and well-ventilated houses. The experience of the fever hospital also bears this out. Notwithstanding the fact that seventy-eight cases were treated there, not one of the staff, who came into contact with the cases, caught the disease, a result which is at variance with the usual experience of hospitals where typhus fever is treated, and which must, we think, be ascribed to the great care bestowed upon the personal cleanliness of the patients, and to the free ventilation maintained in the wards.

That "fomites" may convey the infection must be admitted, although they are not nearly so active in this direction as is commonly supposed. This was shown by the apparent absence of danger to those who mixed with inmates of infected houses in tenement buildings, and the non-conveyance of infection by persons residing in infected houses, to those with whom they came in contact at their work and elsewhere. That fomites, however, may convey infection, was proved by the case of one of the inspectors of the health department, who contracted the disease from a mattress which he cut up, preparatory to burning it, and which had been taken from a very dirty house, from which a patient had been removed. This inspector had not been exposed to infection previously, and he sickened with the fever thirteen days afterwards.

Preventive measures.—The means which ought to be adopted to prevent the dissemination of the disease are—(1) Isolation of the patient in hospital.—This is essential under all circumstances. (2) All persons living in the same house, and all friends or neighbours who have been in close communication with the patient, should be removed into quarantine for fifteen days.—This is the best means of stamping out the disease quickly, and as a rule, amongst the class of people with which one has to deal, little objection is offered to this procedure, provided some small compensation is given to cover the house rent, and an arrangement is effected whereby the men do not lose their employment during their detention. If removal to quarantine is not carried out, then all per-



Italics denote cases of typhus fever.

sons who have been exposed to infection should be frequently visited at their homes, so as to discover as soon as possible any symptoms of illness appearing amongst them. In many instances where the people could not be taken into quarantine they were removed for a few hours to hospital, while their houses were disinfected, and they themselves received a bath, and had their clothes passed through the steam disinfector. (3) Disinfection.—This should be effected by removing all textile articles from the house and disinfecting them by steam—or, in the case of old mattresses, burning them—thoroughly fumigating the rooms with sulphur, or, better still, spraying them with formalin or perchloride of mercury solution; and, lastly, by the plentiful use of soap and water, fresh air, and the whitewashing of the ceilings, stairs, etc.

The infective agent of typhus does not appear to be difficult to destroy, and even free exposure to fresh air alone seems to have a great effect in inhibiting the spread of infection. In none of the houses which were disinfected, according to the above method, was there a recurrence of the disease.

(To be continued.)

THE HYGIENICS OF MILK.

By W. Leslie Mackenzie, M.A., M.D., *Medical Officer of Health, Leith.*

(Concluded from page 378.)

The five diseases I have named—tuberculosis, anthrax, diphtheria, typhoid fever, and scarlet fever—have, each of them, important relations both to the milk-supply and to the housing of cows; but the relations vary in urgency.

Tuberculosis is a problem by itself; it will afterwards be dealt with by itself; here I shall use it merely as an indicator of sterilisation or pasteurisation. Anthrax is relatively uncommon. It is drastically dealt with under the Anthrax Orders of the Department of Agriculture. When a medical officer of health has to deal with an outbreak, he finds, as I found in the counties of Wigtown and Kirkcudbright, that his functions are mainly "consulting" and "advising." But the orders are so managed as to make commercial interest run hand in hand with hygienic requirement. As an everyday matter, anthrax gives little or no trouble. The only practical point likely to create difficulty is—when to resume the sale of milk from an infected byre. This depends—(*a*) On the previous disinfection, which must be very thorough; (*b*) on the incubation period of anthrax. This period has not been defined in the orders; but, on careful collation of authorities and cases, I have found that, for external anthrax or malignant pustule, the "incubation period" (if, strictly, it has such a period at all) is from sixteen to twenty-four hours; for internal anthrax (intestinal or respiratory), from one to four days, sometimes five days. In actual practice, if five days have elapsed without the occurrence of a fresh case, the likelihood of any recurrence is small. Provided isolation and disinfection have been thorough, the sale of milk from non-affected cows may safely be resumed seven to ten days after the last possible exposure. The sterilisation of anthrax-infected milk is a matter of academic interest only. In practice, the milk is well boiled and thrown away where no animals have access to it.

These two diseases—one from its frequency, the other from its infrequency—occupy a very different platform from diphtheria, typhoid fever, and scarlet fever. These three, as everyday experience proves, are among the commonest of the milk infections. And when I say "these three," I include their congeners—sore throat and diarrhœa. Now diphtheria, typhoid, and scarlet fever are practically endemic in most of our towns. Consequently, if only for them alone, the hygienic care of milk must be placed among the highest of social urgencies. The local authorities have drastic powers of dealing with these infections in the person; but the powers for dealing with infected milk supplies are somewhat cumbrous in operation. Cumbrous though they be, the powers are great and, ultimately, sure. But here, as at so many other points, hygienics runs up against economic conditions that require other than hygienic forces to change them. Fortunately, however, all the five diseases named, besides others not named, may equally be dealt with in the processes now to be described—the cleansing and sterilising of milk.

5. *Methods of cleaning milk.*—How shall we clean dirty milk, which is most milk, or prevent its pollution? My question is intended to be strictly practical. I am not here concerned with the abstract laboratarian; I am concerned with the purveyor and the consumer of milk. Within the limits of their reasonable demands, there are many ways of preventing the pollution of milk; there are some ways of cleaning. Some of these ways are for the purveyor, some are for the consumer; but both purveyor and consumer must submit themselves to the economic conditions of commerce in a perishable article. And these conditions are very severe. For the most part, the purveyor is not his own master; and again, for the most part, the consumer is at the purveyor's mercy.

What can be done on the purveyor's side? Let us think of the cowkeeper; we deal with the shops afterwards. The cowkeeper can occupy only improved and approved byres. Good air-space, good drainage, ventilation, warming, are all entirely practicable, but they are not the practice—except in the paper regulations and in a few well-regulated localities. Let us assume that these conditions are realised. What more? From what I have said earlier in this article, the inevitable pollutions of milk demand more. Let us follow the milk to the creamery and study what there happens. The sequence of the actual processes will demonstrate what can be done on the large scale; what may be done in individual farms; what may be aimed at in the poorest. Then I shall return to the individual consumer, who, if he is in the first instance at the mercy of the cow-keeper, is, in the last resort, his own absolute master.

(*a*) *The creamery methods.*—If you take the train at Dumfries and travel westward to Stranraer, you will pass through the two counties that make up modern Galloway. These two counties contain some 600 or 700 dairy farms. The cows on the farms will vary in number from ten to 200. In the middle of Wigtonshire, just on the convexity of Luce Bay, lies the hamlet of Dunragit. One large creamery is there. If you drive down the western wing of the county, you will reach the hamlet of Sandhead. Another creamery is there. If you inquire at Stranraer, you will find another there. If you travel by rail down the east wing of the county, you will find another at the village of Sorbie. If you come back into the Stewartry of Kirkcudbright, you will find another at Valleyfield, down by the picturesque Tarff River. And I believe there is now a creamery at Lockerbie, in Dumfries county. Ireland, I believe, has been the British leader in these developments, Mr. Plunket, M.P., being the guiding mind. The train you have travelled by is a noisy one; the noise at the stations is from the large milk-cans, coming and going. Galloway is one of the Scottish leaders in the milk business. Dumfries and Ayr, no doubt, do relatively a large milk business; perhaps as large as Galloway. I speak of the Galloway creameries, because I have made myself familiar with them. For many of the details of the present practice, I am indebted to Mr. M'Cracken, Manager of the United Creameries. Some details are not strictly relevant to my subject, but they serve to fill in the picture of a concrete case.

From 7 A.M. to 10 A.M., farmers from a multitude of farms bring their milk. They are under very stringent contracts. They must make known, under penalty of breach, any case of infectious illness among their workers. They may have their milk thrown back on their hands, if it is too poor in butter-fat. They get, perhaps, a lower, but a strictly calculated, price for their milk—the price being proportional to quality. In this way, the consumers of creamery milk are under both hygienic and commercial protection. On arrival, the milk-can is swung in by a hoist and tipped into a 70-gal. can, which rests on the weighing-machine. This has a beam graduated in gallons, 10¼ lb. being allowed to the gallon. To prevent souring, warm milk is kept separate from cold. From each weighing-machine a sample is taken and poured into a stoppered bottle. This is repeated daily for a week. At the week's end a sample is drawn from the bottle and tested. The test gives the average quality of the week's milk from each farm. The farmers are paid strictly in proportion to the quantity of butter-fat contained in their milk. After being thus weighed and sampled, the milk, on raising of the plug, passes to large storage vats, which hold from 600 to 800 gals. each. From the storage vats it passes to the heater. This is a steam-jacketed vat, in which the temperature of the milk is raised to between 80° F. and 90° F. The object of the heating is not to pasteurise, but to prepare for separation, which takes place more rapidly and completely at certain temperatures. Ninety degrees is preferable for separation, but is unsuitable for milk that is to be made into cheese. In the heater, the milk, by simple zigzagging movements, under the force of gravity, is thoroughly mixed. This is an important point hygienically; for, although a single gallon of infected milk may infect a whole vat, the chance of concentrated infection is lessened and the uniform quality of the milk is better sustained. Where the milk is pasteurised, the danger of infection is partially eliminated. From the mixing and heating vat the milk passes to the separators. In these centrifugal machines, three things happen—the cream, by its lower specific gravity, keeps to the inner (central) layer; the non-fatty constituents, by their higher specific gravity, keep to the outer layer; the solid particles of dirt and probably some of the bacteria are flung against the metal, and stick there. The cream is shed off by one tube to the butter quarter or to the bottling quarter. On its way to the butter quarter it passes over a refrigator, which cools it down to 50° F. Thence it passes to a storage vat; thence ultimately to the churn. Some of the cream is thicker than the rest. This goes to the bottling quarter. It is cooled down to near freezing point and bottled in small sterile jars. It is then distributed as "double" cream. The separated milk passes to a "scalder" or "pasteuriser," and then passes over a refrigerator. It is then received in sterile cans, and, the same day, sent off by rail. The "scalding" or "pasteurising" is a matter that the creameries do not seem to have perfectly mastered yet. (I refer to the point later.) Thus we have followed the milk from the cart to the railway; the cream, to its bottle; the butter and the cheese we are not concerned with. By the application in a very simple way of a few simple physical principles, the milk has been cleaned of many pollutions; not impossibly, it has been made almost absolutely safe, if consumed within thirty-six hours of treatment. Do you doubt the cleaning? Then look at the lining of the separators, which have now stopped from their mad, steady whirl of 4000 or 6000 revolutions a minute. The lining I have already described—dirt, mucus, débris of every sort. Certainly, the milk has been cleaned. Is it safe? I shall answer this under the head—"Pasteurisation."

(*b*) *The farm methods.*—In all this, which part is impracticable for any dairy? Not the heaters, nor the mixers, nor the separators;

for all these are now manufactured for very small dairies. Not the pasteurisers or scalders; for they can be had of any capacity, down to pints. Not the refrigerators; for ice-making machines may be had for a few pounds. Consequently, many individual farms have adopted the most precise creamery methods. But even a minor installation of heaters, separators, and refrigerators demands a good deal more capital than the petty farmer, perhaps a dairy middleman working on a narrow margin, can command. As a result, two things happen: when the farmer has not enough capital to institute the later processes, he either sends his milk to market unpurified, or sends it to a creamery. If the first, he usually commands a better price; if the second, he must often maintain a better standard, to command any price at all. But, as the success of the creamery shows, the constancy of a local market may more than make up for the better prices of a distant market, and so the milk goes to the creamery. He has only a commercial interest in the milk; but the economic forces, on the whole, work in favour of the creamery treatment. When to these local forces one adds an increased demand for cleaned and manufactured milk, the farm methods and the creamery methods will tend more and more to approximate. In America, as the proceedings at the Wisconsin Agricultural Experiment Station demonstrate, the creameries operate in yet another way; the milk is brought to them to be mixed, pasteurised, and refrigerated; an equivalent of manufactured milk is then returned to the farmer, for him to dispose of as he pleases. The creameries are under the skilled supervision of the State. This is a hint for further development. Whether the development takes place soon or late, depends largely on the individual consumer.

(*c*) *The consumer's possible methods.*—The consumer, I have said, is in the last resort his own absolute master. And so he is, if he but take the pains. In large institutions the preparation of milk is not difficult to arrange for; but, owing to the stress laid on general cookery appliances, the cooking of milk usually takes a secondary place. But wherever there is a steam-boiler, or a steam-jacketed vat, or sufficient heating appliances to heat a few gallons of water, there milk can be "pasteurised" or "sterilised" at will. In large hospitals, or asylums or schools, the dairy ought to be at least as important as the laundry. Every large institution where young children or other delicate persons are the principal inmates, ought to have an installation of milk-cleansing appliances—heater, separator, pasteuriser, refrigerator. Such installation makes the institution independent. In the individual home we have to face a much more difficult problem. This "home," which, even in the moderately comfortable classes, is essentially a compound of bed-room, bath-room, restaurant, and wash-house, is rarely an ideal place for the cleansing of milk. Neither is the domestic servant always ideally clean, even when she is an ideal cleanser. The cleansing of domestic milk is a problem of many variables. Yet the theoretical solution is simplicity itself; the practical difficulty is—how shall we so reduce the ordinary domestic work-load as to make the detailed treatment of milk an easy part of the routine? Further on, I sketch the simple methods of dealing with milk; but my personal experience does not lead me to expect much from the application of individual knowledge in private life. On the average, the private consumer takes what is put before him; he will cease to drink unpurified milk only when there is none to be had. Meanwhile, occasional scares from scarlet fever, typhoid, diphtheria, and tuberculosis will operate on his mind to make him less intolerant of improved methods of cleaning.

(*d*) *Separation of milk and cream.*—Incidentally, separation of milk from cream is a method of cleaning. It is proper to add that separation is not essential to the treatment of milk for pasteurising or sterilising purposes. On the other hand, milk is bought "sweet" that, next morning, it may yield its cream. Cream and milk are, as a rule, consumed in separation. With improved distribution, cream and milk (as already in great measure) will be bought separately. From what has been already said, the hygienic superiority of separated and pasteurised milk and cream is hardly open to question. The food-values remain practically the same whether the milk is separated by "creaming" at home or by the centrifuges at the creamery. If the consumers prefer whole milk, the housekeeper need have no difficulty in remixing the milk and the cream. And, if demanded, the creameries can equally supply whole milk.

6. *The methods of preventing pollution.*—Can nothing be done short of this elaborate treatment of milk? A great deal can be done; but, as I have shown, it is not done. To neutralise most of the polluting agencies is really a matter of extreme simplicity; but the intellectual conviction of simplicity does not necessarily generate the practical belief. If the milker's hands, clothes, and hair were always clean; if the "bedding" and dry-feeding of the cows always followed and never preceded milking; if the cows were regularly groomed; if the flanks and udders were, at every milking, wiped with a moist cloth; if the first few squirts—the fore-milk—were sent into the air, not into the pail; if the byre floor were moistened; if the pails were well scalded; if the milk were forthwith placed in a hand separator, so freed of its adventitious impurities, and then cooled from its natural temperature of about 90° F. to a temperature of 56° F. or lower;—if all these conditions, or most of them, were systematically fulfilled, the germ-content of the milk would, at the end of twelve hours, be as low as in pasteurised milk; the necessity for any process of cleaning would be extremely small, and the milk would not only be less liable to develop disagreeable qualities, but it would keep longer and go farther. That this is all so has been shown

experimentally. For instance, Professor H. L. Russell of Wisconsin[1] states that "Backhaus found that 95·6 per cent. of impurities were removed by centrifugal separation." Again, quoting Harrison's results (p. 38), he finds that "the number of bacteria deposited per minute, on a surface equal to that of a twelve-inch milk pail," may vary from 42,750 during bedding to 483 an hour after bedding. In a cow whose "udder was thoroughly carded, and then moistened," the bacteria per c.c. were 330, the fore-milk being rejected and the milk itself "received in steamed pails." The milk of a "mixed herd taken under usual conditions was 15,500 bacteria for the same volume." These facts are enough to indicate what can be done when a rough approximation to aseptic treatment is aimed at. Is it hopeless to expect further developments? Must we accept the traditional dirty habits of the dairyman and maid? A few years ago, a milking-machine, based on the principle of aspiration, was experimented with a good deal in Galloway and elsewhere. The hopes were great, but I have not observed the apparatus in the trade catalogues; and though a well-arranged system of aspiration would reduce pollutions from many to few, probably the milker's and the cow's preference for personal handling will long keep aspiration within narrow limits in dairy practice. Where methods are so preposterously simple, education may hope to achieve something. For the present, however, we must fall back on the methods applicable "after the fact," and that is why the "creamery" methods must become more and more general. The reactive effects on the practice of the byres will tend to improve the dirty proceedings so popular there.

7. *Sterilisation and pasteurisation.*—If, then, we mean our milk to keep well, and, in times of epidemic disease, to be safe, we must sterilise it or pasteurise it. Anyone who doubts the value or the need of this will have his eyes opened when he faces a milk epidemic of scarlet fever or typhoid; he will become convinced when he finds the "milk-scalders" escaping where the "raw-milkers" fall; and when he finds, in family after family, the infant, whose milk is sterilised, passed over when the others of the family are struck, he will begin to give directions. It is well to have some very clear directions to give. Very frequently milk scarlet-fever strikes adults, who demand more loudly than children some method of self-defence. In an epidemic, to say "boil the milk" may pass, and the boiled milk will, for a time, be tolerated. But only for a very short time. Consequently, some other mode of sterilising is necessary. But, first, let us fix a standard. Tuberculosis offers a severe standard.

In his investigations for the Tuberculosis Commission,[2] Professor Sims Woodhead found that, for complete sterilisation, tubercular milk must be raised to from 88° C. (190°·4 F.) to 92° C. (197°·6

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[1] "Dairy Bacteriology," 4th edition, p. 36.

[2] "Report," pt. iii. pp. 176 *et seq.*

F.) As soon as the milk reached this temperature, the danger was eliminated. But he also found that milk kept for twenty-five minutes at 60° C. (140° F.) was also sterile. If the milk was kept at 70° C. (158° F.) for twenty minutes, the result was the same. There are some slight discrepancies in his results; but these figures have been confirmed by others and may be taken as a standard. The milk tested was known to be tubercular; it was tested by the severest tests for tubercle, namely, inoculation and feeding.

For the fermentation germs, and presumably for the majority of milk germs, including the major infections, a temperature of 70° C. (158° F.) for twenty minutes is more than enough. If, therefore, simple methods can be devised for securing this temperature, no excuse remains for not adopting them. But why not "boil"? First, because boiled milk is not palatable; second, because, according to many, boiling interferes with the nutritive, and, particularly with the antiscorbutic, properties of milk. For infants fed entirely, or chiefly, on milk, this is important; for children whose food is only one-fourth or one-fifth milk, it is less important. But sterilisation at the lower temperature avoids at once the alteration in taste, and, in the opinion of competent clinical observers, the alteration in nutritive value. Experiment shows that 70° C. (158° F.) is just the point where the mawkish taste begins to appear. Sterilisation, or rather partial sterilisation, at these lower temperatures (154°–156° F.) is "pasteurisation." Dr. Ed. von Freudenreich,[1] after analysis of researches, concludes:—"It therefore follows from these researches that pasteurisation for twenty to thirty minutes, at a temperature of 154° to 156° F., kills with certainty all the disease germs that are liable to be found in milk. Such a milk, therefore, fulfils all the requirements of hygiene." The vast majority of non-pathogenic milk germs yield to the same treatment. For practical purposes, therefore, pasteurisation is all that, except in special cases, need be aimed at.

8. *Methods of pasteurisation.*—There are many specially designed appliances for pasteurising. The aim of them all is the same—to obtain and maintain for a definite time in the whole body of the milk a temperature of 145°–168° F. But this physical problem is not so simple as it appears, and the ordinary creameries do not seem to have satisfactorily solved it. "Moreover," says Professor Russell,[2] "the true limit must also be known. This requires the treatment of a *definite quantity* of material for a *definite length of time* at a *definite temperature.*" And "the temperature should be under absolute control."

(*a*) *Pasteurising on the large scale.*—For the creameries, pasteurising is now a matter of detail. The conditions enumerated are realised more or less in several forms of apparatus.

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[1] "Dairy Bacteriology," Eng. translation, p. 100.

[2] *Op. cit.*, p. 112.

Of these, some are designed for continuous heating and cooling. The milk runs in a very thin layer over a corrugated surface, heated by water or steam; then it runs over a similar surface, cooled by water or iced water. To be entirely reliable, the heat must keep near Professor Sims Woodhead's maximum. Others are so arranged that steam at a given pressure circulates through the milk in small pipes, which are used as mechanical stirrers of the milk. This is a good method, but the milk proteids are apt to get over-scalded, and then the disagreeable flavour ensues. Others, again, are simple tanks for hot water; into the tank are placed suitable small vessels; the subdivision of the milk makes it easy to secure a uniform temperature for a definite time. Others, again, are arranged to take in quantities of air-tight bottles, which are heated by steam or water. Yet others combine separation and heating. Obviously, the demand for pasteurised milk is increasing, for the number of machines in the market is great. And doubtless many of these machines, if they do not achieve everything that is claimed for them, yet achieve a great deal. Where their use is under skilled supervision, they may be trusted to improve the hygienic qualities of milk; where they are part of a slovenly dairy, they are probably worse than useless.

The "pasteurising vat" devised by Professor H. L. Russell seems one of the most simple, and, from the bacteriological results, it seems also to be very effective. "It consists of a long, narrow vat, surrounded by a water chamber, on the bottom of which is placed a row of perforated steam-pipes. To facilitate the heating of the milk, both the milk and water reservoirs are supplied with agitators having a to-and-fro movement. The milk-stirrers are perforated so as to give an up-and-down movement, thus preventing the cream from rising to the top of the vat during the operation. The milk is withdrawn from the vat by means of a stopcock that is placed inside of the water chamber. The cock has a circular bore, so that, when open, the outlet tube presents a continuous passage that can be easily and thoroughly cleaned. Placing the stopcock within the water chamber prevents the accumulation of unpasteurised milk in the outlet tube, at a point not reached by the heated water, and where it would contaminate the whole vat when the milk is withdrawn.[1] With this apparatus, "an extended bacteriological examination of milk and cream pasteurised on a commercial scale," showed that "over 99·8 per cent. of the bacterial life in raw milk or cream was destroyed by the heat employed, *i.e.* 155° F., for twenty minutes' duration." As with all such machines, the milk has to be properly selected. The clean byres have the advantage here as everywhere.

(*b*) *Pasteurising on the farms.*—Some time ago the secretary of a milk association said to me, "If you can give us a method of pasteurising milk rapidly, in time for the morning market, the farmers will jump at it." Does anyone seriously contend that mechanical difficulties now stand in the way? For my part, I do not believe it. Neither do I believe that much reform will come from the purveyors of milk; reforms are responses to the consumer's demands, and the methods of pasteurising will be forced on the individual dairies by the stress of competition. The creameries are setting the pace; the consumer, backed by his doctor, will do the rest.

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[1] "Dairy Bacteriology," p. 113.

(c) *Pasteurising in institutions.*—As I have already indicated, wherever there is a cooking-boiler or pot—and that is in every large institution—pasteurisation may be done simply by placing milk-vessels in boiling water for half an hour. The size of the vessels must be regulated; the temperature must be tested until the proper time is hit, and then a time rule can be laid down. Details as to stirring and cooling must not be neglected. The manufacturers of apparatus are now placing on the market simple machines, easily cleaned, and easily controlled. But where expense is an obstacle, milk-cans and a boiler are all the essential apparatus. A little intelligent care on the part of the cook will secure that the milk remains palatable. In all this there is no serious "house-keeping difficulty" whatever.

(d) *Pasteurising in the household.*—It is easier to pasteurise milk well than to make good tea. Yet the normal domestic servant is regarded as equal to the making of tea, but not equal to the pasteurising of milk. What could be nearer the bed-rock of simplicity than this? Take a clean bottle; fill it with fresh milk; cork it. Take a clean pail or pan; place in the bottom an inverted pie-tin, with holes, in order to keep the bottles off the bottom of the pail; place the bottles in the pail or pan. Then fill the pail, or pan, with warm water up to the same level as the milk in the bottles. Heat the whole until the water shows, by the thermometer, 160° F. Remove from the fire or flame. Cover over "with a cloth or tin-cover, and allow the whole to stand for half an hour. Remove bottles of milk and cool them as rapidly as possible without danger to bottles, and store in a refrigerator." These directions are taken from Professor Russell's book; but the same precise conditions can be realised in many simple ways. If a domestic servant is equal to making tea, she is equal to heating water up to 160° F., and then letting it cool again. Or, if that is beyond her capacity, she can at least boil water, remove it from the fire, put a milk-vessel in it, and cover the whole over half an hour. One or two simple experiments will show that, with almost no trouble, a few pints of milk can, for immediate consumption, be made as good as sterile, and that without endangering the palatability. Those that prefer well-designed appliances may now have several. The principal practical point is, that the users should be serious-minded enough to use them—not under the impulse of a scare, but—under the intelligent conviction that our present milk-supplies are, for the less resistant population, seldom entirely free from danger.

9. *The distribution of milk.*—Already I have dealt with the byre, with the factory, and with the consumer. There remains the distributor. The distribution of milk is as difficult a problem as the preparation of it before distribution. Our current methods are of the crudest. Recently the structure of the carts and of the cans has shown some regard to the cleanly handling of a delicate liquid; but there is yet a vast amount to improve. Then, consider for a moment the ordinary town milk-shop. Exceptions, handsome exceptions, there are, doubtless; I speak of the ordinary shop. It is placed in a busy thoroughfare. It is every hour of the day frequented by the people of the locality. They come from every grade, and in every variety of dress. The door opens and shuts every five minutes—now to receive supplies, now to serve an urchin. "A penny's-worth o' skum milk," he says. "We have no skim milk," replies the shop-maid. "Weel, then, gie's sweet." He comes from a poor home; his hands, not long ago, have been assisting at the cleansing of a street gully; but he takes the milk home in an open jug, placing it, perhaps, on the ground in order to have another turn at the gully. As he left the shop a gust of wind blew in some dust. The milk-vessels on the counter were open to receive it. In some cases, it is true, the vessels are covered; in a few cases, they are kept in a glass cupboard; but any milk-seller of experience will tell you that milk shut off from the air is less pleasant to drink. He is, of course, thinking of raw milk, as it comes to him from the byre—half-cool and but roughly sieved. Probably he is right. As milk absorbs odours very readily, so it may part with them sooner on exposure. It is certain that in butter-making free exposure to the air during the churning dissipates certain disagreeable odours. Anyhow, a glass case would protect from dust without preventing aeration. But to return to the shop's environment. Once a day the cleaning cart comes round. Like as not, the ash-bucket is tilted into the cart and pitched down just opposite the door. The dust is naturally borne where its chance of lighting on the milk is greatest. Meanwhile, look further in. That door goes, by a short passage, to a living-room behind—a kitchen, bedroom, scullery, and work-room all in one. That is the inviolable freeman's house, which is his castle. The short passage is sometimes reduced to two doors, separated by three or four feet. That is, "indirect" as opposed to "direct" communication. The "short passage" is, in fact, a legal subterfuge to evade the Dairies Order. From and to the shop the children run all day. Perhaps groceries are sold; perhaps only eggs and butter; much more frequently, confections; rarely is the shop devoted exclusively to milk, milk products, and accessories like eggs. Then the shop goods must be dusted; they must be arranged from time to time; they may, in

many shops, stand until the dust covers them; and then every swing of skirt or cloak or shawl, every current of air, sets something more floating milk-wards. Perhaps, again, mangling and milk-selling go together. Perhaps the means of scalding the plates and pails are inadequate. To describe every variety of combination legally permitted is impossible. What I have said is enough to demonstrate that if the dirt of the byre is enormous, the possible dirt of the milk-shop is little less. And if, to the inevitable dirt of the cleanest town environment, one adds the dirt of infrequently washed hands, uncut nails, shed skin, fouled sleeves, and the innumerable abominations of pent-up life in single rooms of town houses, one cannot but stand amazed at the capacity of the civilised palate to feed on polluted supplies. It is necessary to add—clothes that go for months unwashed; beds unaired; blankets washed once a year; adults and children that have never had a bath of the whole body. In places like these, a surgeon would exercise the most stringent care in his endeavour to secure asepsis, even for a minor operation; a major operation he would not tackle. But the same surgeon, if bacteriology has not cured him of his milk, often as not permits his kitchen staff to supply his children with milk from the very shop where he would not operate.

Here, then, is a serious problem. The hygienic solution is simple enough. Let milk-shops be constructed on the same lines as an aseptic operating theatre; let the principles of the laboratory be applied to the protection of the milk; let the shops be devoted exclusively to the sale of milk and milk products; let there be no communication with living-rooms; let there be air; let there be light. To fit up a shop with impervious walls, shelves, counters, etc., is an affair of every day. To provide protecting cases for the milk, to separate old milk from new, to reduce the need for manipulation to a minimum—these are easy problems, and, now and again, they are solved. But there is still a large residuum. How shall we deal with it?

10. *The municipalisation of the milk trade.*—Theoretically, the municipalities, that is, the public health local authorities, have a fairly adequate control of the purveyal, preparation, and distribution of milk. They can make regulations for registering milk-sellers; for inspecting dairies, cowsheds, milk-shops; for securing good air-space in byres, good ventilation, good drainage, good water, cleanliness; for prohibiting the work of infected dairy attendants; for stopping milk supplies that may be considered by the medical officer of health to be infected, and for all the many "propria" that flow from these powers. In towns, no byre can be occupied without a licence, and the licence need not be granted until the magistrates are satisfied that the byre conforms to their conditions. Further, certain municipalities have the power to examine byres beyond their bounds, if milk

from such byres is sold within the municipal boundary. In all byres, the attendants, the premises, and the cows, may be exhaustively examined by local authority officers. All these special powers are reinforced by the large general powers of the Public Health Acts, by the powers of the Food and Drugs Acts, and by the detailed Police Act powers for insuring the cleansing of premises. Legally, it is obvious, the powers are extensive and drastic. Yet, for all that, such milk-shops as I have described may be found in any considerable town. How is this?

Limits of inspection.—Certain things regulations can do: they can lay down general conditions for wholesome milk-shops; they can secure partial isolation of the shop from living rooms; they can direct the construction of washable shelves, counters, cupboards, floors, and walls; they can insist on good lighting, ventilation, drainage, and general facilities for cleaning. So for byres. But certain things regulations cannot do: they cannot make unclean people wash often; they cannot restrain the chance raids of unclean customers; they cannot keep doors shut, or lay street dust in the street, or tie the winds of March. Much they can secure by systematic inspection, even when structures are poor. They can tone up the sensibility to dirt. An inspector can advise, direct, forbid, threaten, prosecute. But, at its best, inspection is regulative, not productive. It cannot alter the habits of a people, the structure of a society, the economic conditions of supply. These all it must take as it finds them, and guide them towards a better model in detail. As with water, with gas, with electricity, with sewerage, with the slaughtering of cattle, with hospital isolation, so with milk; experience is slowly proving that municipal control will remain a feeble and ineffective compromise until the municipalities have a direct commercial interest in the purveying, or preparation, or distribution of milk. Already one observes signs of a tendency in this direction. The city of Edinburgh has resolved not to accept a hospital contract for milk unless the contractor agrees to have his cows subjected to the tuberculin test. If the municipalities extend such conditions, they will at once raise the standard of milk production and cleansing. If they made cubic space, refrigeration of milk, pasteurisation, and the like, also conditions of contract, they would soon create a supply. And if, on failing to get such conditions accepted, they instituted, for hospital purposes, municipal byres, municipal creameries, municipal pasteurisers, and municipal bottling, they would, in a short time, be flooded with applications for putting consumers' milk through their machines. As with the disinfection of clothes, as with public wash-houses, so with public milk-factories—their presence standardises popular ideas and stimulates to better production. I do not urge the insane proposal that the municipalities should forthwith acquire the dairies, milk-shops, and byres. What I urge as a reasonable possibility within the range of ordinary municipal practice is that, as with lodging-houses, working-men's houses, wash-houses, and the like, the municipalities might, by a piece of concrete practice, create the model for a vast industry. If, in the course of time, the demand for clean milk presses on the resources of the municipal installations, these may be extended by many methods. The possible methods I leave aside, meanwhile; I content myself with bringing the control of milk into line with the control of other common goods. The Public Creamery will have for its correlate the Municipal Milk-shop.

11. *The economic question.*—Against the adequacy of inspection there is a greater force than any I have indicated. A milk-seller's licence is of great commercial value. The large dairyman must rely on the small milk-shop. By selling milk, thousands of poor people eke out, to a living return, the profits of a minor trade in groceries, or confections, or bread. To drive sections of a people over the line that separates poverty from pauperism is no great gain, on the long run, even for the public health. And, undoubtedly, in many instances that would be the result of drastic inspection in terms of the legal letter. Then, in the milk trade, as in every trade, competition is keen. Hygienic restrictions make markets difficult. "Compensation" continually rouses the dairyman's mind and threatens the local authority. Cows feed; milk goes wrong; cows and milk are alike perishable stock. The trade is a trade in perishables. Every day must dispose of the day's produce. These are some of the economic reasons why inspection has its limits, why regulations remain on paper, why the baffled student of hygienics looks round for new organic growths that shall displace the products of degeneracy.

As with so many other economic problems, the solution comes like a thief in the night. While the retail milk-shops are springing up in legions, one striving to kill the other, the county creameries and their methods are gaining steadily. As the ancient pedlar went down before the modern merchant, so the present defective preparation of milk will yield to the less defective, the present less perfect organisation to the more perfect. Instead of merely inspecting miserable milk-shops into decency, the local authorities will do well to encourage improved methods of manufacture. And, as I have shown, a possible beginning lies there ready to their hand.

12. *Conclusion.*—In this condensed sketch I have put in prominence only the practical problems. I have shown the conditions that hamper cleanly production, the methods of overcoming the resultant evils, the defences the consumer has always in his power, and the lines of development open to the municipal organisations. Many by-problems remain; but the solution of them is rapidly coming within the range of practical hygienics.

ERASION *VERSUS* EXCISION IN CHILDHOOD; OR, THE REMOTE EFFECTS OF EXCISING OR ERASING TUBERCULOUS JOINTS IN CHILDREN.

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OPERATIONS during childhood possess features of interest quite distinct from those connected with adult life; and whether these be considered in relation to the immediate effects of an operation, or to its remote influences, they are of such a distinctive and important character as to demand separate consideration. The innumerable changes wrought during the process of development upon every part of the system will of necessity entail, at some more or less distant period, indications of previous interruptions with normal growth. In no part of the body is this better seen than in the case of the joints. The functional activity of these regions is such that any interference with them during the process of growth must show itself ultimately in a very marked degree, both in impairment of movement and in development. Inasmuch as the joint proper is dependent upon the action of muscles attached to other regions, these latter also suffer to an extent in proportion to the normal part they are accustomed to play under ordinary circumstances. What I propose to consider in these remarks is the effect observed in a limb after either excision or erasion of a joint, judged by the time which has intervened between the operation and a subsequent investigation of the parts. With the help of my assistant, Dr. Macdonald, I have been enabled to ascertain the results of several of my cases operated upon as far back as 1887. The number turns out to be a comparatively small one—twenty-six—but, I think, sufficiently large to prove instructive, and admit of certain conclusions being drawn. From the number of operations of this kind which I have performed in the Victoria Infirmary, during the last ten years, I had hoped for fuller statistics; but the impossibility of tracing some cases, and the death of others, renders it very difficult to present a long series, the result of one man's work. I might have extended my list by drawing upon reported cases, or upon the work of some of my colleagues in Glasgow. It seemed to me, however, that more value, as a rule, attaches to the operative results obtained by the same surgeon, than to those the collective results of several men's labour. The large class of excisions of the hip-joint I have not included in my table for discussion, for the simple reason that we cannot erase that joint; and there is, therefore, no data for comparison. On the other hand, I have included excisions of the shoulder-joint, because I believe, under certain very favourable conditions, this might be erased. It will be seen, therefore, that the cases for consideration limit themselves almost exclusively to the elbow, knee, and ankle, where it is possible either to excise or erase. I should add, before referring in detail to the table of cases, that in every instance the operation was performed for tuberculosis; and, further, that I have made no selection whatever. With only one exception, I have recorded in this list every case that I have been able to lay my hands on. The exception was that of an erasion which, unfortunately, became septic either at or immediately after the operation. It did not seem to me fair, in discussing the relative merits of the two operations, to allow an accident of this kind to weigh either for or against any conclusions which might be drawn. I will now proceed to take up each series of cases, and discuss them in the order in which they appear in the table.

Table of Twenty-six Cases of Excisions and Erasions of Tubercular Shoulder- Elbow- Knee- and Ankle- Joints in Children, showing Results after intervals of from 1½ *to* 13 *Years.*

No.	Name and Age (at Date of Operation)	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
			SERIES I.—EXCISION OF SHOULDER-JOINT.					
			Years.			In.		
1	George S., æt. 13.	April 5, 1888.	11	R.	.. ..	2½	"Free movements at the shoulder-joint; limited elevation, unable to reach a right angle."	"He has a perfectly useful arm, and can lift a half hundred-weight."—(Report of Dr. William Russell, New Mains.)
2	John L., æt. 9.	May 6, 1893.	5 11/12	L.	Arm 3 in., forearm 1 in. shorter. Circumferential measurements also much less.	4	Free movements at the joint; arm could be raised to a right angle with the body.	Hand diminished in size in all dimensions.
3	Patrick T., æt. 4.	Jan. 17, 1896.	3¼	R.	Arm 1 in. shorter; but forearm equal.	1	Free movements at shoulder; but can only raise limb to 45°.	Slight diminution in size of hand. In this case the excision was performed by a deltoid flap, which appears to have unduly impaired the action of that muscle.
			SERIES II.—*A*. EXCISION OF ELBOW.					
4	Maggie C., æt. 11.	Mar. 3, 1886.	13	L.	Arm 1¾ in., forearm 2½ in. shorter.	4¼	Perfect flexion and extension of false joint.	Hand diminished in all dimensions. Upper part of thorax, 1½ in. less on left side than right. Excision cicatrices only. Very serviceable limb.

Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
		Years.			In.		
Robert L., æt. 14.	Sept. 10, 1888.	10 7/12	R.	"Arm 1½ in., forearm 2½ in. shorter. Arm 3 in. and forearm 1½ in. less in circumference."	4	"Incomplete extension. Flexion to a right angle with arm."	"Hand smaller than left . . . discharging sinus for two years after operation. . . . Very useful limb, but becomes exhausted after much use."—(From Dr. M'Laren, Uddingston.)
Hugh S., æt. 14.	Mar. 6, 1894.	5 1/12	R.	Arm 1½ in., forearm 1½ in. shorter.	3	Bony ankylosis at an obtuse angle.	The limb a very serviceable one. Numerous cicatrices exist around the joint.
James P., æt. 13.	June 20, 1894.	4 10/12	R.	Arm ½ in., forearm 1½ in. shorter. Circumferential measurements respectively 1¾ and 1¼ in. less.	2	Complete flexion and extension of false joint.	Hand ½ in. shorter, and proportionately small in other dimensions. Very few cicatrices present. A very serviceable limb.
Francis H., æt. 16.	May 7, 1895.	3 11/12	L.	Arm 1 in., forearm 1¾ in. shorter.	2¾	Complete extension, but flexion limited to bending the forearm a little above the right angle.	Marked diminution of hand in all dimensions. Few cicatrices, apparently those connected only with the incisions for excision.
John H., æt. 7.	Nov. 1, 1895.	3½	L.	Arm 1 in., forearm 1½ in. shorter.	2½	Bony ankylosis at an obtuse angle.	Numerous and extensive cicatrices exist around the joint.

B. ERASION OF ELBOW.

Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
Sarah S., æt. 10.	June 25, 1895.	3 10/12	L.	No shortening of either arm or forearm. Circumferential measurements of arm and forearm less.	0	Firm ankylosis at an obtuse angle.	Hand same in length, but slightly diminished in breadth (½ in.).
George T., æt. 7.	Oct. 23, 1895.	3 6/12	R.	No shortening of either arm or forearm.	0	Flexion and extension to 60°; 30° to each side of the right angle.	A perfectly serviceable limb.
Rachel D., æt. 7.	Jan. 28, 1896.	3 3/12	L.	No shortening of either arm or forearm.	0	Flexion and extension to 20°; 10° to each side of an obtuse angle of about 130°.	A perfectly serviceable limb. Girl strong and healthy-looking.
Thomas M., æt. 10.	Oct. 26, 1896.	2½	L.	No shortening of either arm or forearm.	0	Firm ankylosis at an open angle.	Both hands equal in all dimensions.
Mary M., æt. 3.	Feb. 21, 1897.	2 2/12	R.	"No shortening of either arm or forearm."	0	"Firm ankylosis; limited amount of pronation and supination."	(Report of Dr. William Grant, High Blantyre.)

SERIES III.—*A*. EXCISION OF KNEE-JOINT.

Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
Wm. M'I., æt. 9.	Mar. 15, 1890.	9	L.	.. ..	2	Parts firmly ankylosed; limb perfectly straight.	. ..

No.	Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
			Years.			In.		
16	James M'G., æt. 6½.	June 24, 1893.	5¾	L.	.. ..	2	Firm ankylosis, but slightly flexed.	The limb a very serviceable one.
17	Kate M'F., æt. 8.	Mar. 31, 1896.	3	L.	.. .	2	Firm ankylosis; leg perfectly straight.	Foot markedly diminished in all dimensions.
18	Hugh B., æt. 3.	Dec. 9, 1896.	2¼	R.	.. ..	1½	Firm ankylosis, but with slight hyper-extension.	.. .

B. ERASION OF KNEE-JOINT.

No.	Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
19	Lizzie S., æt. 9.	Sept. 1, 1896.	$2\frac{7}{12}$	R.	Both legs measure the same in length.	0	Firm ankylosis, with slight flexion.	Girl active, in good health. The tendency to flexion has, however, to be checked by the constant use of a plaster splint.
20	Lily N., æt. 3.	June 25, 1897.	1¾	R.	Both legs measure the same in length.	0	Firm ankylosis, with slight flexion.	Child very active, and in excellent health; needs plaster splint to prevent further flexion.
21	Willie R., æt. 12.	Jan. 11, 1898.	$1\frac{4}{12}$	L.	Both legs measure the same in length.	0	Firmly ankylosed, and quite straight.	The disease was very extensive; the boy very tubercular. His right elbow was erased on March 24, 1899. Healed under two dressings.

SERIES IV.—*A.* EXCISION OF ANKLE-JOINT.

No.	Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
22	John D., æt. 8.	April 10, 1887.	12	L.	Thigh 1 in., leg 3 in. shorter.	4	Fibrous ankylosis, with fairly free flexion, and extension at the false joint.	The foot much smaller in all dimensions.

B. ERASION OF ANKLE-JOINT.

No.	Name and Age (at Date of Operation).	Date of Operation.	Period elapsed since Operation.	Right or Left Side.	Relative Measurements.	Total Shortening of Limb.	Functional Activity.	Remarks.
23	Ellen M'G., æt. 6.	Mar. 22, 1895.	4	L.	Both legs measured the same in length.	0	Fibrous ankylosis, with free flexion, and extension at the ankle-joint.	In walking only a very slight limp was observable.
24	Kate M., æt. 8.	June 18, 1895.	3¾?	R.	Both legs the same in length.	0	Fibrous ankylosis, with free flexion, and extension at the ankle-joint.	With the exception of the line of incision in front of the ankle, the result was so perfect that no difference could be detected between the two feet either as to appearance or function.
25	George R., æt. 5.	Nov. 19, 1895.	3¼	L.	Both legs the same in length.	0	Fibrous ankylosis, with free flexion, and extension at the joint.	The child walked so well that the operated-upon foot was indistinguishable from the sound one.
26	Martha C., æt. 9.	April 6, 1897.	2	..	"No difference in length of leg."	0	"Slight stiffness at joint."	"No difference observable in walking. A complete success in every way."—(Report of Dr. E. Corbett, Barrhead.)

The first series comprises three cases of excision of the shoulder-joint. They are too few in number to admit of any special remarks regarding the amount of shortening that might be expected. But there is one striking feature which has come out very prominently in many of the other cases, and that is the diminution in growth which takes place in parts of the limb quite unconnected with the actual seat of disease and operation. This I shall refer to later; but it will be noted in Case 2, for instance, how both the forearm and hand were diminished in size as compared with the same parts on the sound limb. Functional impairment after excision of the shoulder is limited to the raising of the arm from the side; there are free movements in all other directions. This limitation in elevation of the arm doubtless arises from the altered mechanical conditions resulting from the removal of the head of the humerus. The arm of the lever being shortened between the insertion of the deltoid (the power) and the glenoid cavity (the seat of the fulcrum), a greater power is required to elevate the limb. But this diminution of the distance has lessened the contractile range of the deltoid, and hence, while required to do more, it can only do less. Added also to this diminution of power, when in reality more is required, the muscles which acted on the tuberosities have been detached, and are hence useless for any purposes of elevation. These few considerations are given to show that if it were possible in any case to erase the joint instead of excising it, better results might be obtained in every way; freedom of movement might be got almost equal to that so successfully acquired in the case of erased ankle-joints.

Passing to the second series of cases, those of excisions and erasions of the elbow-joint, we have some very interesting facts elicited. It must be borne in mind, in the first place, that by excision of a joint we take away the articular surfaces of the humerus, ulna, and radius, irrespective of their being diseased or not; while in erasion we remove the disease, and that alone, interfering as little as possible with any other parts of the joint which appear free from involvement.

The comparison of these two operations shows that in the case of excision we may, though by no means necessarily, get very free flexion and extension, associated with much shortening of the limb; while in the case of erasion we almost always get more or less ankylosis, either quite firm, or admitting of some comparatively slight flexion and extension (see Cases 11 and 12), but there is little or no shortening of the limb.

It should be noted in the case of the excisions, that where ankylosis and not free movement was the result of the operation, there existed around the joint numerous cicatrices (Cases 5, 6, and 9), while, where there was free movement, there were simply the cicatrices of the operation incisions (4, 7, and 8). The inference naturally is, that where numerous sinuses exist before the operation, and especially after it—where, in fact, several subsequent scrapings may have to be performed—then we may expect anything between comparatively limited movement and firm ankylosis. On the other hand, where previously no open wounds exist, and none follow the operation, the most perfect functional activity of the false joints may be looked for.

Unfortunately, it was not noted whether similar considerations affected the question of movement in the case of the erasions. But the probabilities are, that the same would hold; that where discharging sinuses previously existed, or lingered after operation, firm ankylosis would be the rule; while movement to variable extents—never, however, to the same degree as in the case of excision—would follow the opposite conditions. The nature of the joint—a "hinge"—with normally no lateral action, renders it probable that any movement will be the exception rather than the rule.

In the third series, that of excisions and erasions of the knee-joint, there are also interesting comparisons. The operations themselves are virtually the same as those upon the elbow, with, however, functionally opposite results; for while in excision of the knee we aim at getting bony ankylosis, in the elbow we hope for movement; in erasion, we may get some movement at the knee, but we generally get firm ankylosis at the elbow. In other respects the results are fairly similar, for in excising we may, and probably will get, much shortening; while in erasing little or no alteration in the length of the limb appears to follow. There is another important point of difference between these two operations upon the knee, which, from the effect it has upon the after-treatment, must be carefully noted. In a successful excision, with firm bony ankylosis, the limb remains perfectly straight. In an equally successful erasion, there is a constant tendency, so long as growth is proceeding, for the knee to become flexed.

In the fourth series, that of excisions and erasions of the ankle-joint, the solitary instance of the case of excision renders it difficult to make any very positive comparisons. This one case, however, is probably pretty typical of what might be expected in others, and may certainly be taken in itself as representing as good a result as may ever be expected. Although the joint was excised, and bony surfaces brought together, yet the union has only been fibrous, with a very considerable amount of flexion and extension at the false joint. The shortening of the limb, however, was very marked—4 in.—necessitating a raised sole, and a considerable limp in walking. The foot also had failed to develop to anything like the extent of the sound one.

In the cases of erasion some of the very best results are seen of operations upon joints. Without exception the limbs were not shortened, and the movements of the feet so good that in walking it was frequently impossible to distinguish the affected side from the unaffected.

So far I have attempted to deal with facts; now let me venture upon a few conclusions warranted by them. In the first place, it must be understood that we are dealing with a disease the very nature of which it is at all times difficult to gauge. While it may seem to be purely local in its manifestation, it may equally possess a far-reaching constitutional basis. For this reason, therefore, it is difficult to speak, except in the most general way. What operations should be performed, when and how, are questions that can only be answered in the face of each individual case. There is one guiding principle, however, which touches every case, and that, that our operation must have as its primary object the total removal of the disease. To effect this, however, is frequently to disregard all conventional and classical methods of making skin incisions for the excision or erasion of a joint. Cuts must be freely made in every and any direction, in order to get thoroughly at the infected parts. Indeed, it is only by these free incisions that secreted pockets of tubercular material are discovered. And, further, it may be stated as a practical and important matter of detail, that diseased tissues should be *cut* away, and not scraped away, as much as possible.

As regards, then, the knee and elbow, my practice is to make my incisions for the opening of the joint just as seems likely to give me the best exposure; and I do not hesitate to extend these or make fresh ones if there is the slightest indication that by so doing I can better cut away the disease. In erasions, after all the diseased tissues have been methodically excised, any pockets or spaces left are stuffed with sterilised gauze, all bleeding vessels having, of course, been previously secured. The skin incisions are stitched up to variable extents, and the limb fixed by simple splints in the required position—in the case of the knee, straight; in that of the elbow, at an obtuse angle. If the temperature keeps normal, and there is freedom from pain, nothing should be touched for a week, ten days, or even a fortnight, when the stuffing is removed, and properly applied pressure substituted.

In the case of erasion of the ankle-joint, somewhat more definite lines may be indicated regarding the steps of the operation. In all the cases in Series IV. (*B*) the joint was exposed by an anterior incision, extending across the articulation from one malleolus to the other; other incisions were made afterwards to expose sinuses or pockets extending usually up the leg. After dividing the skin and subcutaneous tissues, the tendons are picked up and secured by stitches in such a way that after division, and at the conclusion of the operation, they can be easily reunited. Attention to this comparatively small detail will save a good deal of trouble in seeking for the retracted tendons, when they are subsequently required for reunion.

A study of these cases collectively brings out the remarkable fact, that in not a single instance was the growth of the limb interfered with by erasion, while variable degrees of shortening followed in every case of excision. Another striking fact, not altogether easy of explanation, was the diminution in size of other parts of the limb, other than those directly associated with the diseased or operated-upon region. Unfortunately, this fact was not noted in every instance, but in such as were examined it was found that the want of development showed itself in the cases of excision and not erasion. The natural explanation, and possibly after all the most likely, would be functional inactivity; that parts grow in proportion to the function they have to fulfil. Yet it is strange to note that in both the cases of excision of the elbow (Cases 4 and 7), where there was good flexion and extension, the hand of the affected side was markedly diminished in size; while in the only two cases of erasion in which it was noted (Cases 10 and 13), where there was complete ankylosis at the joint, the hands were practically the same in size on both sides. If functional inactivity be the true cause of this want of development, then it would serve to indicate that an erased ankylosed elbow gave a more serviceable and useful limb on the whole than an excised movable one: that a patient is able to make much more use of her hands in the former case than she can do in the latter.

Attempting, in conclusion, to express an opinion as to the best operation to perform upon tuberculous joints in children, as based on my own experience, and such of it as is shown in the accompanying table, I should unhesitatingly advocate erasion in every joint where it can be executed; and the younger the child the more imperative the practice. In the case of the shoulder, I see no reason why this joint should not be erased, by turning the head well out of the socket. But the disease must be limited, otherwise it is impossible to get room enough to cut it out without excising the head. With the elbow, care must be taken to fix the limb at the time of the operation in the position required, should firm ankylosis occur, that is, at an open angle of about 130°.

As regards the knee, prolonged care is required to prevent subsequent flexion. This is best combated by the comparatively frequent re-application of a plaster splint. The firmness of the ankylosis seems to determine chiefly the tendency or not to flexion. So long, however, as the tendency to flexion lasts, so long must the limb be kept forcibly straight.

Lastly, with the ankle, the only precaution necessary is to prevent the patient making too soon use of it. Freedom of movement will come readily enough when the child once begins to get about; but to allow it to attempt to walk before the deep parts are soundly healed will be only too likely to light up some fresh foci of disease.

I wish, in conclusion, to thank those medical men, whose names are quoted in the tables, for the reports of cases kindly forwarded to me; and also to express my indebtedness for other useful information about cases from Dr. Donald Macphail of Whifflet, Dr. James Alexander of Galston, Dr. James Clark of Beith, and Dr. J. Reid Foulds of Clydebank.

PUERPERAL HYPERPYREXIA, WITH AN ILLUSTRATIVE CASE.[1]

By J. Lamond Lackie, M.D., F.R.C.P.Ed.

Hyperpyrexia is well recognised as one of the most serious of complications occurring in the course of many diseases, the mortality, indeed, being as high as 84 per cent. From Bryant's most exhaustive paper on the subject, one finds that brain and spinal cord lesions were responsible for 29 per cent. of the cases; that pyæmia and septicæmia were the cause of 17 per cent.; that in 7 per cent. the complication arose in the course of acute rheumatism; while pneumonia, infectious fevers, ulcerative endocarditis, tuberculosis, heat stroke, etc., were the cause of the remaining of the hundred cases recorded.

One is bound to distinguish between those cases in which the hyperpyrexia is of what is known as the pro-agonistic or prolethal type of Wunderlich, and those cases in which the high temperature occurred some time before the death of the patient, and was in itself the direct cause of a fatal issue, or from which the patient recovered.

With the pro-agonistic type of hyperpyrexia most must be familiar, and in obstetric practice it is probably met with most commonly just before death from eclampsia. But, as occurring in the course of a puerperium from any cause, and as being the direct cause of death, or as being the most important symptom, hyperpyrexia must be extremely rare. Pye Smith of Sheffield records the case of a young married lady who, after her first normal labour, had for three weeks a temperature from 104° to 106°·2, usually in the morning after a sleepless night. She was of a nervous temperament. Wunderlich says, that after parturition the temperature may reach 107°·6, and in a few days fall to normal without any apparent cause. A case is recorded in which the temperature rose to 110°·2 at death, on the fifth day following labour. This case, however, was obviously of the prolethal type, and is comparable to those cases of, for example, fatal eclampsia, in which an exceedingly high temperature occurs just before death.

Beyond these, I can find no record of hyperpyrexia occurring in the course of a puerperium, and I therefore venture to report the following case to the Society, in which exceedingly high temperatures occurred, but the patient ultimately recovered.

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[1] Read before the Edinburgh Obstetrical Society, 10th May 1899.

Mrs. O., æt. 36, with five children, each being delivered with great difficulty by forceps, owing to a contracted brim, went into labour for the sixth time. When called, and on examination, one found the os the size of a five-shilling piece, and it was noted that, as the result of her previous confinements, the cervix had been so torn that to the left and the front the tear extended right up to the fornix and even beyond it. I afterwards ascertained that the patient had been warned that, owing to the condition of the cervix, further pregnancies, or rather labours, would be dangerous, since the state of the uterus already so closely bordered on rupture. In due course the os dilated fully, and the membranes ruptured, but the head did not descend. As time went on the pains became more and more feeble, until the patient had ultimately complete inertia uteri. Forceps were therefore applied, and in thirty minutes the child was born dead. The mother did perfectly well and had no abnormal symptom till the end of the fourth day, when she suddenly had a severe rigor. I was sent for, and found that the temperature was 105°. As the vaginal discharge was fœtid, an intra-uterine antiseptic douche was administered and 15 grs. of antipyrin given by the mouth. In spite of these measures it was found two hours later that the temperature was 109°; the patient was delirious, and the pulse very feeble and so rapid that it could not be counted. The excessively high temperature being evidently the cause of the patient's alarming condition, ice was procured, and as much of the surface of the body as possible was covered with it, while the temperature was taken per rectum every ten minutes. This gradually fell to 103°, when the use of ice was suspended, but, as is the rule in such cases, the temperature still fell till it reached 99°·5. The patient now seemed much better, talked quite rationally, and expressed herself as feeling quite well. For twenty hours she continued apparently fairly well, the temperature ranging from 100° to 101°. Suddenly she had another rigor, the temperature rose to 110°, and similar treatment as on the night before was resorted to with satisfactory results. This time the patient was quite comatose and apparently dying, but the treatment by ice restored her to consciousness, and all alarming symptoms passed away.

Meanwhile intra-uterine antiseptic douches were being administered every four hours, and per vaginam nothing abnormal could be ascertained beyond the excessive tear in the cervix.

Exactly twenty-four hours from the commencement of the previous attack of hyperpyrexia, I visited the patient, when I found her temperature was 100°, and her pulse 100, but she had a certain amount of restlessness, and especially a wandering fitful movement of the eyes, while she expressed herself as feeling perfectly well. From previous experience of hyperpyrexia, I ventured to tell the nurse that very soon she would have a very high temperature again, and promised to return in half an hour. I left the house, but had not gone five hundred yards when I was overtaken by a messenger summoning me back. A rigor had occurred and the temperature was 110°; ten minutes later it was 112°; the patient was absolutely comatose; no pulse could be felt at the wrist; breathing was stertorous, and one expected a fatal issue at any moment. The patient was packed in ice; 20 grs. of antipyrin were administered per rectum, and an antiseptic douche was given, when there came away a large slough and a large quantity of foul-smelling discharge. The temperature gradually fell to 104°, when antipyretic treatment was suspended, and as the patient was then conscious, stimulants were freely administered both by the mouth and subcutaneously. The temperature continued to fall until the ordinary clinical thermometer recorded nothing at all; but gradually the pulse returned, and became hour by hour stronger, until there were no immediate signs of danger. On the following day the temperature was nearly normal and the general condition was good. The nurse, however, reported that the patient passed no urine per urethram, and that all seemed to escape per vaginam. On examination, a large cervico-vesical fistula was found, so large that it admitted two fingers. As the result of systematic douching the temperature gradually fell, and the patient from that time had no further symptoms and made an excellent recovery.

The fistula was later operated on successfully, and now Mrs. O. is quite well.

This, then, was a case in which the dose of ptomaine poisoning from autogenetic infection was so great that on three successive nights the patient had marked hyperpyrexia. It was a case of severe sapræmia, there being no evidence of either septicæmia or pyæmia. These two latter, without any reference to the puerperium at all, are responsible for 17 per cent. of the cases of temperature above 106°, but I can find no record of a case in which an absorption such as occurs in sapræmia was the cause of hyperpyrexia. The symptoms set in on the fourth day and continued from then until the slough separated, resulting in the formation of the cervico-vesical fistula. With regard to the symptoms, I should like to draw special attention to that one which I had an opportunity of observing on the third day, and which indicated the threatening onset of hyperpyrexia: it was the peculiar restlessness, especially of the eyes, and the deceptive expression of "well-being" on the part of the patient. It has frequently been noted in rheumatism, and in three cases of hyperpyrexia from this cause, occurring at different times, that I had the privilege of watching, while resident in the Royal Infirmary, it was a most striking symptom. The patients announced quite suddenly that they were perfectly well, and that their joint pains had suddenly disappeared, while the chart certainly indicated a temperature in one case distinctly lower than that previously recorded. They had, however, the special restlessness and appearance of doubt as to their surroundings to which I have referred, and within half an hour all had temperatures above 106°. I take it that the condition was exactly comparable to the state of my puerperal patient, when from similar symptoms I was able to predict a third attack of hyperpyrexia.

It has been noted that hyperpyrexia is most liable to occur in the hotter months of the year; thus 20 per cent. of Bryant's cases were observed in August, and 51 per cent. in the four months, June to September, leaving only 49 per cent. for the remaining eight months. My case, however, occurred in December, so that the season of the year cannot be said to have had any influence in the production of the complication.

The presence of a separating slough, and that alone, accounts for the condition, though why in this particular case a sapræmia should have caused a temperature of even, on one occasion, 112° one cannot determine.

With regard to the treatment of the case, the cold bath is the most strongly advocated for hyperpyrexia. Of this I had had experience in four cases, the three in the infirmary to which I have already referred, and one of pneumonia following influenza in private practice. These cases all succumbed. It so happened that to carry out the bath treatment in the case which I have now recorded was at the moment impossible, and therefore simply packing in ice was resorted to, with happily a good result. By the bath treatment, more gradual lowering of the temperature can be insured; but it involves much more movement of the patient, and probably throws a greater strain on the heart and circulation, than when the patient is not disturbed by the transference from bed to bath, and *vice versâ*. At all events, it is well to remember that in all cases of hyperpyrexia the temperature should be reduced by active measures to only a point midway between the highest recorded and that to which it is desirable it should fall. Experience has shown that after antipyretic treatment is suspended the temperature continues to fall just as much after as during the application of ice or the use of the cold bath.

The case which I have described in some detail must be at least uncommon, and therefore I hope I am justified in bringing it before the notice of the Society.

ASTHMA SEXUALIS AND ITS OCCURRENCE IN THE INSANE.

By F. GRAHAM CROOKSHANK, M.D. (Lond.), *Assistant Medical Officer, Northampton County Asylum.*

EVEN those who most insistently urge the "central," "neurotic" element in the pathology of asthma, attach considerable importance to the existence of causes of peripheral irritation. And this peripheral irritation is, as everyone knows, most often from mucous membrane—bronchial, nasal, or gastro-intestinal. But perhaps more often than is admitted in the text-books, attacks of asthma are precipitated by morbid states of the genito-urinary mucous membrane. To this fact attention has been drawn in an admirable essay by Peyer, translated by the New Sydenham Society, and published in 1894. It has of course been known since ancient times that physiological sympathy exists between the reproductive organs and certain nasal structures. And, indeed, at a recent meeting of the British Medical Association, speakers were not wanting who insisted on their structural as well as their pathological similarities. Dr. Goodhart also has urged the value of that hypothesis which assumes the immediate cause of an asthma to lie in the erectile properties of bronchial tissues. Peyer's point of view is, however, somewhat different. He finds asthmas, whether diaphragmatic or bronchial, to be frequently set up by morbid hyperæmias and catarrhs of the genito-urinary tract; such catarrhs and congestions, in fact, as may be set up in women by fibroids and subinvolution, and in either sex by masturbation. Most often Peyer has found these asthmatic attacks—attacks of "asthma sexualis" as he says—to be associated with perversities of sexual function and practice. It should be pointed out, however, that it is to the resulting catarrhs and hyperæmias that Peyer attaches etiological importance, and not to the practices that originate them.

Since reading Peyer's paper, I have constantly borne his observations in mind, as they seem, from a therapeutical point of view, of no little importance. And I have been surprised to find how often, in certain groups of cases, carefully directed questions have revealed the existence of the etiological factors on which Peyer insists.

The most striking case that I have seen, and it is one not without parallel in Peyer's experience, I met with several years ago, when in residence in a London Hospital.

CASE 1.—One morning, about 2 A.M., being urgently summoned to the surgery, I found there a sickly and pallid youth, greatly excited, and distressed by a violent attack of asthma. He declared it was his first attack; and, the thoracic organs appearing free from complicating disorder, was being dismissed with what seemed appropriate remedies, when he mentioned his wife's alarm. A very few questions elicited the facts that he was recently married, that the asthma had followed on a sexual failure, and that the failure was directly attributable to previous excess.

The subject is not altogether a pleasant one, but from my experience I believe that "asthma sexualis" is to be suspected when asthma occurs, in the absence of bronchial, cardiac, gastro-intestinal, or nasal excitation—(*a*) In young neurasthenic men, given to sexual perversions or excess; (*b*) middle-aged women, the subjects of uterine catarrh from fibroids and other causes, particularly when single; (*c*) young women and girls who have given way to sexual irregularities.

But there is one other point not without interest. When, in the insane, asthma is met with in such clinical conjunction that the case is deserving of being called one of asthmatic insanity, it often happens that there is present a very strong sexual nisus.

Three cases, all of which are at present under my observation, tend to bear out this proposition.

CASE 2.—The first case is that of a burly ex-soldier, who, like so many of the asthmatic insane, labours under delusions of persecution. Like many others, too, his delusions are only vivid when he is most asthmatic. But his spells of asthma are invariably preceded by periods of strong sexual excitement, during which he writes inflammatory letters to the female staff of the asylum, and employs every stratagem to catch sight of the female patients. At the end of these periods, he gives way to uncontrolled masturbation, and the consequent asthma revives the delusions that "enemies" are "conspiring" to deprive him of virility.

CASES 3 and 4.—The second case is that of a man who, before admission, was a notorious pæderast. Even now he is a paroxysmal masturbator, and would, if indeed surreptitiously he does not, indulge with a lad of 21, like himself insane, asthmatic, and a masturbator. Both these vicious and degraded organisms pay the penalty of their perverted lust in attacks of asthma. In fact, the attendants are unwilling to recognise the call for medical treatment, so convinced are they that the asthma is a consequence of vice.

No doubt the nervous tissue of these unhappy creatures is unstable, but I have no doubt that their indulgences stand in direct relation to their asthma, in the way Peyer has indicated. The relation of the asthma to the insanity is less obvious.

The whole subject of asthma sexualis, though nauseous to a degree, is of considerable importance. Peyer has achieved excellent results with the treatment suggested in individual cases by the particular local conditions. In none of my dozen or fifteen cases has general treatment been of the slightest use. Unless, however, attention is drawn to this particular cause of asthma, and pertinent questions be asked, these cases are sure to be overlooked in practice. It is, then, with the desire that others may pay attention to Peyer's unduly neglected observations that these notes have been written.

One more point may be alluded to. Most text-books make mention of the fact that asthma in the previously healthy often first occurs at night. In my first case, the reason for the nocturnal incidence of the attack is sufficiently obvious. May it not be so in others?

CHLOROFORM SICKNESS.[1]

By T. A. Ross, M.B., C.M.

During the last fifty years there has been a large quantity of literature on the subjects of anæsthesia and chloroform; but, so far as I can make out, very little attention has been paid to the question of chloroform sickness. In the text-books it is all but ignored. Yet the condition is one of great importance—so great that Lister, writing in the year 1863, says: "In the special case of ovariotomy, the disturbance thus occasioned (*i.e.* by sickness) is often a cause of death." He further points out that chloroform is contra-indicated in cataract, where any tendency to vomiting would seriously interfere with the success of the operation. The literature then being scanty, I have had to fall back mainly on observations which I was able to make while acting as house-surgeon to Dr. P. Hume Maclaren, to whose kindness in allowing me to publish the results of these I am much indebted.

Sickness due to chloroform may be divided into two distinct classes—one sickness during the administration of the anæsthetic, the other coming on after the patient has fully recovered consciousness. Those cases occurring before consciousness has been fully regained, though after the administration has been stopped, belong to the first class. This sickness often takes place just when the patient has been put into bed, or before he leaves the theatre; it is curiously rare for it to occur in the cold corridors.

The first class is treated of in all the text-books. It is usually stated that it is due to the patient being allowed to come out of the anæsthetic; that it is the first act of returning consciousness, analogous to the vomiting which so often precedes consciousness after cerebral concussion; hence it is that sickness after the administration, before the patient is fully conscious, belongs to this group. The proof that returning consciousness is associated with the sickness, lies in the fact that we never see sickness on the table when the patient is kept properly under the anæsthetic. In connection with this a particular class of cases becomes interesting. Among the cases of which I have notes are ten where operations were performed on the mouth. As is well known, such cases cannot be kept fully anæsthetised for any length of time; they are alternately in a state of deep anæsthesia and semiconsciousness. Such a condition ought to be one where sickness is almost constant; and yet not one of these was sick on the table, and any sickness from which any of them suffered afterwards was merely the vomiting of a little blood. We may possibly explain this by reference to a reflex inhibitory action on the part of the vagus, which supplies the stomach and also the posterior third of the tongue and the pharynx. In these operations the throat is constantly being swabbed out, so that the vagus is well stimulated. A very similar reflex may be brought about to inhibit the act of sneezing. The fifth nerve supplies the mucous membrane of the nose, and also the skin of the nose. If the skin of the nose be squeezed when the desire to sneeze is felt, the act can be inhibited. In midwifery practice, too, deep anæsthesia is seldom produced, and yet sickness is rare. I shall try to show later that the hypertrophy of the heart which occurs in pregnancy may have something to do with this.

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[1] **Read before the Royal Medical Society, Edinburgh, 6th January 1899.**

The act of vomiting is preceded by great pallor; the pulse tension falls, and the heart beats feebly. The duration of the sickness is brief. The patients recover almost at once, irrespective of treatment. We notice again that those cases which are sick immediately after the administration has been stopped, also recover almost at once; and if there is to be further sickness in such a case, it does not begin till consciousness has been fully regained. The danger in this class of sickness is that foreign matters may be sucked into the air passages, and cause asphyxia, or be followed by septic pneumonia. The first of these is rare; the second may follow the inhalation of a tiny particle, and it may be difficult to prevent this occurring. Its frequency is probably greater than is usually supposed. It is not only after operations on the mouth and throat that patients die of septic pneumonia. It would be interesting to know how many such were sick on the table. I have seen at least two cases of the kind.

Let us now turn to the subject of post-chloroform sickness. In this I include cases occurring within twenty-four hours of the administration, and after the patient has recovered consciousness. Certain people are much more prone to this sickness than others. They are very sick after every administration; and seemingly the more protracted the administration, the worse the vomiting. A case which I believe proved fatal from vomiting shows both these points. D. S. suffered from tubercular kidney. In the summer of 1897 several nephritic abscesses were opened under chloroform. After the administration he was very sick, the vomiting lasting for about three days. After a short stay in hospital he went home. He returned to hospital in May 1898. As there were still signs of tubercular kidney, it was decided to perform nephrectomy. The operation proved to be very long and tedious. After coming out of the anæsthetic, the patient began to vomit. Various means were tried to check this, but the patient continued to vomit, till he died on the sixth day after the operation. There was no suppression of urine, and nothing to account for the condition except chloroform. The other kidney was healthy.

In order to prevent such accidents, what is usually done is to so prepare the patient that he will have nothing to vomit.

Though this method is well known, it may be as well to describe it. The patient's bowels are evacuated, in order to prevent defæcation on the table. At about 6 A.M. he gets a light breakfast, of a small cup of tea and a fragment of toast. Five or six hours later the anæsthetic is administered; that is, he is not anæsthetised until the stomach is empty. Now, out of seventy-one cases prepared in this way twenty-five were sick; this is a percentage of 35·2 sick. Out of all the cases, 137 in number, of which I have notes, the percentage of cases sick was 35·7. This does not point to any benefit derived from this method of treatment. Indeed, the following figures would seem to show that it is actually hurtful. I have notes of cases chloroformed without any preparation; they were mostly hernias, accidents, and so forth. Out of eighteen only four were sick. This gives a percentage of 22·2 sick. The conclusion is that the time-honoured method of preparation causes about 13 per cent. more to be sick than if no preparation were made. It is difficult to make out when this method arose. Sir James Simpson does not mention it, and yet he tells us that there was "no vomiting or sickness in any of the cases." In this connection the following case is of interest. A young man was admitted to the wards suffering from bladder symptoms. For the purpose of physical examination, he was put under chloroform after the ordinary preparation. He suffered very severely from subsequent vomiting, which continued for nearly thirty-six hours. In about a fortnight it became necessary to perform the operation of suprapubic cystotomy. As the patient had suffered so much from sickness on the previous occasion, it was decided to use cocaine. A meal was given just before the operation, so as to diminish the shock. In the course of the operation the patient begged for chloroform, as the pain was more than he could bear. Chloroform was therefore given—the administration lasting about twenty minutes. This patient had no sickness, though, according to existing theories, he should have been very sick indeed. All this seems to point out that chloroform administered on an empty stomach is more likely to cause sickness than if the patient has recently had a meal.

Vomiting, occurring either during or after the administration, is associated with circulatory depression, as shown by pallor and lowered pulse tension. A well-known handbook states that the faintness is caused by the vomiting; but it may always be noted that in these cases the pallor and lowered tension precede the vomiting, and are therefore not caused by it. Indeed, the administration of brandy—a circulatory stimulant—will check vomiting when it has arisen. Now, patients who have been starved for several hours are, of all, the most prone to syncope, and also, as I have endeavoured to show, to vomiting. Hence I believe that chloroform sickness is in part due to circulatory depression. The reasons in brief are—(1) Circulatory depression precedes vomiting; (2) when the operation is prolonged, *i.e.* where there is more circulatory depression, there is more vomiting; (3) in midwifery where the heart is hypertrophied, and where shock is usually little compared to that after operation, there is little tendency to sickness; (4) an empty stomach augments depression and is more commonly accompanied by sickness; (5) a circulatory stimulant (brandy) diminishes sickness.

That another cause is present also is probable. This is, that chloroform acts on the stomach wall as a local irritant and causes emesis. Every patient at some period of the administration swallows some of the chloroform vapour. If the stomach be empty the local effect will be greater. Again, the longer the administration the more chloroform will find its way into the stomach, and the effect will be increased. Further, as will be shown later, the application of local anæsthetics and of lavage to the stomach wall tends to check vomiting; these measures counteract or remove the local irritant. In view of these considerations, it would seem to be right to give the patient his breakfast before administering the anæsthetic. It would not be wise to allow him to distend his stomach, but it does not seem to matter how near the administration the meal is given, provided that the anæsthetist will not let him be sick on the table.

Certain measures may be taken medicinally to prevent the onset of sickness. First, as already mentioned, we may exhibit local anæsthetics. Dr. Maclaren suggested a capsule of cocaine, $\frac{1}{10}$ gr.; menthol, $\frac{1}{8}$ gr.; and acid carbolic, $\frac{1}{4}$ gr. This was given as soon as the patient came out of anæsthesia. Sixteen patients were treated in this way. Of these, four were sick. This equals 25 per cent. These patients had been prepared in the ordinary way. As already mentioned, in those similarly prepared, who did not get these capsules, the percentage of cases sick was 35·2. This is a gain of 10 per cent. Cocaine alone in capsule was tried also, but the results were not good. Sixteen patients were given $\frac{1}{8}$ gr.; ten were sick. This equals a percentage of 62·5 sick. The preparation Pepsencia (Fairchild) has been recommended by some surgeons as a preventive against chloroform sickness. The result, as we found it, happens to be the same as that obtained when cocaine alone was used. Sixteen got it. Ten were sick. It is recommended in doses of 2 drms. to 4 drms, ten minutes before the administration of chloroform, and again as soon as the patient can swallow.

If sickness has come on, in many cases it will soon stop; but if it has gone on for a few hours, something must be done, as the patient soon becomes worn out. First, on the ground that there is in the stomach a local irritant, we get the indication to remove this, to practise lavage of the stomach, as recommended by Greig Smith. This may be done by the stomach tube, but much more easily by giving the patient two large breakfast cups of weak tea, without sugar or cream. Almost at once the patient will vomit a large quantity of fluid, and will then often turn round and go to sleep. This seems to be a very good plan for stopping chloroform sickness. It always succeeds. It is liked by the patients. The remedy can be got anywhere.

A second indication is the exhibition of stimulants, on the ground that vomiting is due partly to faintness. Brandy in teaspoonful doses has often a very beneficial effect. I think, however, that it is only temporary, lasting two or three hours.

Morphia is recommended in many books. Possibly its effect is deceptive. A full dose of morphia will of course stop vomiting, because morphia checks peristaltic movement. But when the effect of the drug passes off, the condition of vomiting is apt to be re-established. Indeed, it is common for patients after operation, to whom morphia had been given for pain, and who had not been sick prior to its exhibition, to vomit on awakening from the morphia.

There is a class of cases where the patient continues for some days to be sick once or twice a day. It will be found that an evacuating enema will often check this. Sinapisms over the epigastrium are in all cases useful. Creasote in minim doses was tried in four cases. It did not appear to be of advantage in any.

CLINICAL RECORDS.

CASE OF LOCOMOTOR ATAXIA, WITH CANCRUM ORIS AS A FATAL COMPLICATION. POST-MORTEM EXAMINATION.

By R. Barclay Ness, M.A., M.B., F.F.P.S.G. *Professor of Materia Medica and Therapeutics, Anderson's College Medical School, Glasgow.*

Case 1.—Mrs. T., a widow, æt. 48, was admitted, on my recommendation, to the Glasgow Royal Infirmary, under the care of Dr. Middleton, on the 28th August 1896. The following facts regarding the case are taken from the Ward Journal. The patient, who was a tall spare woman with grey hair, stated that her illness began in January 1896, when she first complained of shooting-pains in the legs, extending from the hips to the ankles. Five months later she complained also of pain in the lumbar region, and at a later date of a feeling of constriction round the waist. She experienced also a peculiar feeling in the legs, consisting partly of a sense of weakness and stiffness about the knee-joints, partly of a sense of numbness in the same parts, but extending also to the feet. Here the feeling of numbness was most marked, and gave rise to the usual feeling, as if she were walking on soft carpet, when in reality she was walking on a hard wooden floor. Accompanying the numbness there was present also a prickling sensation "of pins and needles," but this was not so persistent. With the development of the above symptoms, she began to experience some difficulty in walking in the dark and in turning quickly. This unsteadiness gradually became more marked. Though she had no actual pain accompanying micturition, she had difficulty in evacuating the bladder, requiring time and much straining before the flow of urine would begin. There had been no pain connected with the action of the bowels, though there was a tendency to constipation. It was noticed, however, during her residence in the hospital, that when a mild purgative was given, like the liquid extract of cascara sagrada, in small doses, there was a tendency to undue looseness of the bowels produced. Previous to her admission into the hospital she had had no gastric disturbance of any kind, nor had there been any disorder of the special senses. In her walk she was very unsteady, being quite unable to keep in a straight line. The difficulty in walking was apparently not due to any want of muscular power, as the heels were brought to the ground with considerable force. Equilibrium was lost immediately on putting the feet together and closing the eyes. The examination of the muscles showed that there was no undue wasting in any particular part, though the limbs and the body generally were rather thin. Analgesia and anæsthesia were present in both legs below the knees, the analgesia being very marked. The muscular sense, on the other hand, was unimpaired, as tested by the patient's appreciation of weight, and the position of the legs when the eyes were closed. The knee-jerks were completely abolished. The ankle-clonus was not present. The plantar reflexes were much exaggerated, the tickling of the feet causing also a tingling sensation in the legs and often a sudden spasmodic movement of the whole limb. The abdominal reflexes and jaw-jerk were well marked. No abnormalities were noted in the hands, beyond a little tremor. The grasp of each was equal, registering 80 by the dynamometer. The examination of the eyes showed the action of the extrinsic muscles of the eye to be practically normal. There was just the slightest degree of nystagmus detected. The pupils were much contracted and almost insensible to the stimulus of light, while they contracted very slightly during accommodation for near objects. Ophthalmoscopic examination was made at a later date (September 16th), when the eyes were under the influence of homatropine. The optic discs were normal in their appearance, the only point noted being the slightly oval shape of both discs in a longitudinal direction, when seen both by direct and indirect methods. The fundus and vessels of both eyes were normal in appearance.

The other facts bearing on the condition of the patient on her admission may be briefly stated. The tongue was clean and moist; the temperature normal; pulse 68 per minute, and of fairly good quality; respirations 18 per minute. Nothing abnormal was detected in the examination of the heart, lungs, liver, or spleen. The urine was acid, of amber colour, and had a specific gravity of 1012. It contained a trace of albumin, but neither blood nor sugar. The microscope showed the sediment to contain both epithelial and pus cells. This constitutes the original report of the case, which was diagnosed as one of locomotor ataxia. Inquiry into the previous history of the patient was carefully made, but the only fact elicited, which seemed of any importance, was that she was not always of temperate habits. The extent, however, to which she did indulge in alcohol was not ascertained. No history of syphilis could be obtained.

With regard to the progress of the patient during this, her first residence in the hospital, which extended over four months, there are a few facts which require to be recorded. Not only did she continue to suffer from the dysuria, but towards the end of November the urine was found to contain pus, epithelial cells, tube casts, the slight albuminuria noted above still persisting. The shooting-pains in the legs caused the patient at intervals much suffering. Sometimes she complained of pain in the lumbar region as well, frequently also of pain in the abdomen, with a feeling of nausea. These varieties of pain were much relieved by the administration of 10-gr. doses of antipyrin. The treatment otherwise consisted in a fair trial, at different periods, of iodide of potassium and nitrate of silver, along with the application of the galvanic current (10 milliampères) to the spine and legs. It was believed about the middle of October that there was some slight improvement, but this proved only temporary.

One important fact relative to the condition of the eyes should be referred to. On 13th December it was noted that the patient had for some days complained of the vision of the right eye being obscured. This was corroborated by the use of Jaeger's test types. With the right eye the patient could not see J. 20, while with the left she could see J. 6. On examination of the right eye by the ophthalmoscope, Dr. Rowan detected a large hæmorrhage in the macular region. There was no change with regard to the reflexes of the pupil. They remained as described above. On 30th December the patient was allowed to go home. The vision of the right eye had by this time much improved, but her general condition was no better, and the ataxic symptoms, if anything, were worse.

The day after the patient left the hospital she became very ill, complaining of pain "over the heart." This was accompanied by incessant vomiting, which persisted for four days. She was unable to retain even milk. The bowels were constipated, and required enemata to induce movement. There was also retention of urine for about two days. She was therefore readmitted to the hospital, on 5th January 1897. She was then extremely weak. Though she could stand, she could not walk without assistance. The pulse was weak, numbering 96 per minute. Respirations numbered 22 per minute. Temperature was normal. She was pale and restless, and still complained of pain over the heart, and, on inquiry, also of headache. The tongue was slightly coated and dry, but not tremulous. Vomiting persisted after admission, the matter ejected being coloured with bile. The pupils were contracted and equal; knee-jerks, ankle-clonus, and plantar reflexes were absent. Anæsthesia and analgesia were still present in the legs. Sensation was impaired also in the hands. The grasp of the right hand measured 70 by the dynamometer, the left 60. The heart, lungs, and abdomen were normal. The urine was muddy, strongly acid, having a specific gravity of 1017. Albumin was present to a slight extent. The microscope showed the sediment to contain pus cells, red blood cells, epithelial cells, and numerous tube casts, both granular and hyaline in character. Four days after admission (Jan. 9th) diarrhœa began, which was imperfectly controlled by opium. The motions were very liquid and very offensive. They contained no blood; vomiting still continued, but to a less extent.

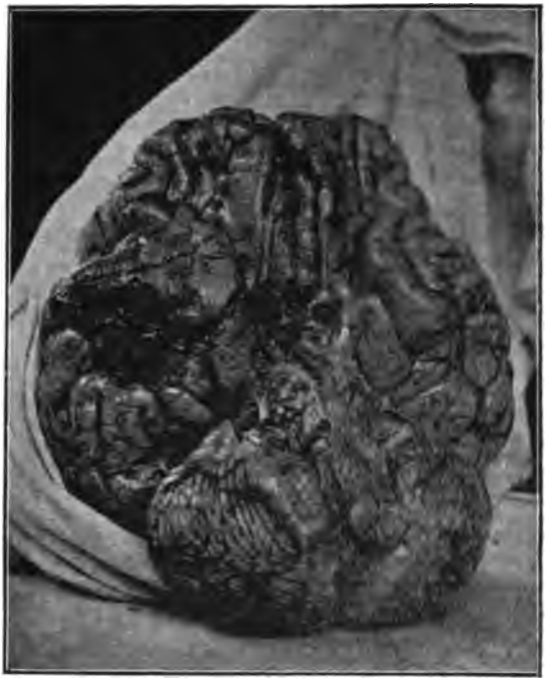
On 12th January the patient's temperature, which had previously been normal, rose to 101°·8 F., and on 13th January to 103° F. There were no rigors. On 14th January, swelling of the lower part of the face on both sides was noticed, and to a less extent of the neck immediately below the jaw. The swelling was hard to the touch, as if the tissues were infiltrated with inflammatory products. The skin itself was little reddened. By 16th January the swelling of the neck had somewhat diminished, but that of the cheeks had increased, while both parotid glands were greatly enlarged. The same evening the patient had a short rigor, and the temperature rose to 104° F. Soon the mucous membrane of the mouth was also found involved. By 22nd January the swelling of the face had increased, so as almost to close the eyes, while the swelling in the neck diminished. The temperature had steadily fallen, so that the maximum registered on the 22nd was only 99° F. The pulse remained very feeble. Subsequently the swelling of the face began to diminish, but the mucous membrane of the mouth became more and more involved, while the patient complained much of the pain in the throat. On the 24th a considerable amount of brown or black slough was removed from the inner surface of both cheeks, on a level with the molar teeth. On the 25th it was noted that while the swelling in front of the face continued to diminish, both temporal regions became swollen, giving the face a rather round, unnatural appearance. The posterior part of each cheek was red, and the capillaries distended, but there was no appearance of any slough forming externally. The diarrhœa continued, the evacuations, which were of a very offensive character, being passed in bed.

The treatment consisted chiefly in the free use of antiseptics to the mouth, and the administration of stimulants, in the form of port wine.

From this time onwards the cancrum progressed steadily within the mouth, speech becoming impossible. Nourishment could only be given in very small quantities. On the 26th, 27th, and 28th, pieces of slough were removed from the mouth. The patient's mental condition on the 28th became obscured. She gradually sank, and died on the 29th January.

A post-mortem examination was made by Dr. M'Crorie on 30th January, with the following result. The heart and lungs were normal. The abdominal contents were adherent, from old peritonitis. The liver was slightly enlarged and hyperæmic. The spleen was enlarged and very soft. Both kidneys presented the appearance of being affected with interstitial nephritis. In the right there was a large cyst. The bladder was greatly distended, but the mucous membrane was apparently normal.

On the removal of the brain it was found that there was a considerable necrosis of the under surface of the right temporo-sphenoidal lobe. This is well depicted in the figure reproduced from a photograph taken by Dr. James Dunlop. The meninges adjacent and the bone beneath were considerably discoloured. The same parts on the left side were also involved, but to a much less extent, the most apparent change to the naked eye being a greenish discoloration of the temporo-sphenoidal lobe with the adjacent membranes and bone. It seemed as if the mischief had only recently involved these parts.



On removal of the temporal bones, it was found that the whole of the soft tissues lying below formed a black or greenish black pulpy mass, so that the probe on both sides could be passed from above downwards through the necrosed tissues between the cheek and the molar teeth. On the right side the disease appears to have spread to the brain by the carotid canal, the contents of which, with the exception of the artery, were discoloured. On the left side extension took place along the course of a small vein, which, passing under the temporo-sphenoidal lobe, escaped from the skull through a small foramen in the temporal fossa.

The following report of the microscopical examination of the spinal cord has been kindly furnished by Dr. Charles Workman, pathologist to the Glasgow Royal Infirmary:—

"A portion of each region, cervical, dorsal, and lumbar, of the spinal cord was fixed in a 5 per cent. solution of formaldehyde, and rapidly embedded in celloidin. From these blocks sections were cut and stained by various methods, especially by carmine and by Mallory's stain, and also by Weigert's stain, after treating the sections with chromic acid solution, ½ per cent. The lumbar region shows the sclerosis most extensively, involving almost, if not the whole, of the column of Goll, and also the column of Burdach, except for a small area close to the posterior commissure on each side, and a small area close to the posterior roots where these pass out from the cord. In the dorsal and cervical regions the sclerosis is almost equally great in the columns of Goll, but there is a small part in the outer zone of these columns where some nerve-fibres are retained. In these regions the columns of Burdach show much more healthy nerve-tissue in the same parts as in the lumbar region, but the central zone of these columns is very completely degenerated. In the cervical and lumbar regions the anterior cornua appear perfectly healthy, and the posterior cornua fairly so, though the latter are perhaps a little involved in the degeneration. In the dorsal region, the anterior cornua appear much smaller than normal, and the multipolar nerve-cells are very few in number. In the posterior cornua of this region the posterior vesicular column is not so distinct as it should be. The rest of the white substance of the cord presents quite healthy characters, there being no evidence of degeneration of the direct cerebellar tracts nor of the pyramidal tracts."

This case presents a few interesting points, which, in conclusion, may be briefly enumerated:—

1. The existence of locomotor ataxy in the female. There is no doubt that this disease is not so common in women as in men, at least in this country. Most authorities agree upon this point. Gowers gives the proportion as being about 10 per cent.

2. While we had in this case all the important symptoms of locomotor ataxy, we had in addition, at one period of the disease, greatly exaggerated plantar reflexes, and occasionally involuntary spasmodic movements of the legs. These are unusual in this disease, but they have been acknowledged as possible signs in the early stage of the disease. Their presence, however, associated with slight tremor in the hands, and a very fine degree of nystagmus, did suggest the possibility of the cord being more widely involved than is the case in locomotor ataxia, but the post-mortem examination did not reveal anything of a definite character beyond the involvement of the posterior columns.

3. Burdach's columns are partially involved in their whole

length, corresponding with the clinical fact that the arms were involved as well as the legs. The columns of Goll were involved in their whole length, the result of an ascending degeneration. The anterior cornua, if we exclude a doubtful area in the dorsal region, were normal. This fact agrees with the one stated in the clinical report, that though the patient had fallen off somewhat in her general condition, there was no localised wasting of muscles.

4. The presence of interstitial nephritis explains the albuminuria, perhaps also the retinal hæmorrhage. Possibly the renal condition depended on alcoholism. Apart from this, however, there was a disturbance of the reflex nervous mechanism of the bladder, possibly associated with slight cystitis.

5. Death resulted from cancrum oris, with extension of the gangrenous process to the skull and the base of the brain. This is a very unusual complication in locomotor ataxia. I am aware that many cases, with lesions of the mouth and nose, probably the result of trophic changes in the region of the fifth nerve, have been recorded. For example, cases are recorded of perforating ulcer of nasal septum;[1] of perforating ulcer of hard palate, with loss of teeth and atrophy of the alveolar borders;[2] of rapid loss of teeth and atrophy of alveolar margin of upper jaw, with impairment of sensation in the region of the trigeminus;[3] and of affections of the teeth themselves.[4] Many such cases are reported, which cannot be ascribed to syphilis; they seem to be really trophic lesions. Is it possible that the gangrenous condition of the mouth, above described, may have supervened in an area already susceptible, through involvement of the fifth nerve? We know that perforating ulcers of the foot are not uncommon in tabetic patients. These are of trophic origin. But a case of actual necrosis of the terminal phalanges of the great toes, with periostitis of the terminal phalanges of the second toes, is reported by Russell as an unusual phenomenon in locomotor ataxia.[5] In this case we have no definite syphilitic history, and it may therefore serve as a connecting link between the slow, insidious, and very limited process of perforating ulcer, and the acute gangrenous process described in my case.

A more apt analogy, however, may be drawn between the complication described and the "acute decubitus" which sometimes occurs in nervous affections like acute myelitis.

I have to acknowledge my indebtedness to Dr. Geo. S. Middleton, for his permission to publish the case; also to Dr. John W. Jackson and Dr. Malcolm Watson, late house physicians in the Royal Infirmary, whose notes of the case I have made use of.

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[1] *Brit. Med. Journ.*, London, 1892, vol. i. p. 768.

[2] *Presse méd.*, Paris, 2nd April 1898; *Practitioner*, London, August 1898, p. 193.

[3] *Berl. klin. Wchnschr.*, 1885, No. 12, S. 178.

[4] *Med. Times and Gaz.*, London, 1882, vol. ii. p. 477.

[5] *Ibid.*, 1882, vol. ii. p. 210.

A CASE OF RENAL SARCOMA SUCCESSFULLY REMOVED BY LUMBAR NEPHRECTOMY.

By THOMAS JONES, B.S. (Lond.), F.R.C.S. (Eng.), *Professor of Surgery, Owens College, Manchester*; and T. N. KELYNACK, M.D. (Vict.), M.R.C.P., *Pathologist, Manchester Royal Infirmary.*

THE pathological researches of recent years have thrown considerable light on the varying structure assumed by the so-called "malignant" growths of the kidney, and wide interest has been awakened in the practical aspects of the condition by the records of a considerable number of cases in which nephrectomy has been performed, not only successfully as regards the operation, but in effectually removing a fatally-tending growth, without any recurrence.

These growths differ greatly in "malignancy," and there can be no doubt our knowledge will become more definite and precise in so far as we, to use Mr. Malcolm's words, "look for guidance in treatment to a careful study of clinical histories, and of the microscopical structure of the specimens removed."

For the present it seems most desirable that every renal growth removed should be submitted to careful pathological investigation, and the salient features of the case reported. We therefore make no apology for briefly placing the following case on record:—

CASE.—Thomas S., æt. 50, married, and a sawyer by trade, was admitted to the Manchester Royal Infirmary on 29th October 1898.

CLINICAL HISTORY.—Has always enjoyed good health, with the exception of an attack of "inflammation of the kidneys," eight years ago, which necessitated absence from work for two weeks. Has been somewhat intemperate. No history of "malignancy" in the family. Eighteen months ago "twisted" himself while carrying a plank, which led to considerable hæmaturia, blood continuing to be present in the urine for several days. For the last three months he has experienced much pain in the left side of the back, which was put down to "lumbago." At first the pain was of a dull aching character, unassociated with any sickness, and not continuous. Later, it became more severe and practically permanent, and blood made its appearance in the urine. The practitioner in attendance on the case, recently, on two occasions, found long thin clots of blood in the urine, looking as though they had been moulded in the ureter.

On admission to hospital the patient appeared to be a fairly nourished, robust-looking working man. The urine was neutral in reaction, sp. gr. 1012, and contained blood and albumin. Examination, under an anæsthetic, revealed enlargement of the left kidney, with a distinct nodular mass at its lower part. The kidney was exposed by the lumbar route, and found to be the seat of a distinct growth, which, however, seemed localised to the organ. The whole kidney was removed.

The renal vessels were first clamped, then tied, a drainage tube inserted, and gauze packed around. The patient made an uninterrupted recovery. During the first twenty-four hours, 10 oz. of urine were passed; on the second day, 20 oz.; on the third day, 32 oz.; on the fourth day, 34 oz.; from thence it continued to vary from 40 to 50 oz. daily.

Dr. Richmond, the patient's medical attendant, reports on 7th March, that "I have on repeated occasions examined his urine, but so far it has been quite free for any albumin or blood, and the quantity so far as I can gather is about normal."

PATHOLOGICAL CHARACTERS.—*Macroscopic.*—The organ weighed 7 oz. It was 4½ in. long and 2½ in. wide. The lower, 2¼ in., was involved by diffuse growth. The greater part was of a more or less solid nature, but with slight tendency to cystic softening in the centre. In colour it was pinkish yellow and yellowish white, but also with extensive reddish brown areas, due to extreme vascularity. The growth extended quite up to the capsule, which was elevated into irregular nodules, but the capsule itself did not appear to be penetrated. At its upper part the neoplasm gradually merged into the normal-looking renal tissue.

Microscopic.—Sections of the growth were made at its most active part, where it was invading the more or less normal kidney substance. The tumour consisted of embryonic connective tissue cells, separated and surrounded by newly formed fibrous tissue-like elements. The cells varied somewhat in size and form. Many were round or slightly oval, and staining deeply with hæmatin. Others were elongated and distinctly spindle-shaped. In some parts the cells were aggregated into dense masses, while in other areas they were separated by strands of fine embryonic connective tissue, which formed a delicate stroma in which the cells appeared in somewhat alveolated form. In extensive districts the supporting tissue had undergone myxomatous change, and in some parts even the cells seemed to be participating. These areas presented a curious honeycomb-like appearance, with the cells lying in many instances quite free in the spaces. The greater part of the growth stained readily. The vessels appeared as mere spaces lying in the tumour tissue, with no distinct walls. Some parts of the growth were extremely vascular, the blood spaces being large and irregular. The kidney tissue in the vicinity of the growth was very vascular, the epithelium swollen, and in a condition of early fatty degeneration. Between the tubules and around the glomeruli was much fibrous tissue.

REVIEWS OF
BRITISH AND FOREIGN LITERATURE.

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Traumatic Separation of the Epiphyses. By JOHN POLAND, F.R.C.S.
London: Smith, Elder, & Co. 1898.

THOUGH we owe a large proportion of our knowledge concerning the separation of the epiphyses to foreign observers, and more especially such facts as are derived from experimental work and the treatment of irreducible and complicated cases by operative measures, we may feel justly proud that it is to an English surgeon that we are indebted for the first attempt to gather together and collate the large amount of material that has been gradually accumulating on this subject. The result is a monograph that may be regarded as a complete compendium of our knowledge on all that appertains to the separation of the epiphyses. It cannot fail to be the standard work for many years to come, and to serve as the basis of any future research on similar lines. It is difficult in a short review to do full justice to the work of Mr. Poland; and some idea of the scope and contents of this volume is all that it will be possible to give.

The book commences with a history of the subject, which is traced from the time of the Amazons, who are reputed to have separated the epiphyses of their male children, to the present time.

The main feature of the succeeding chapter, on the anatomy of the epiphysis, is a series of nineteen most admirable skiagraphs of the hand and wrist, taken by Mr. William Webster. The importance of studying in this way the development of the bones is shown by the very interesting variations that children of the same age exhibit. For example, a thirteenth child, æt. 5, is seen to have its ossification much less advanced than a second child of the same age; in fact, development is no further advanced than is shown in another skiagraph of a child, æt. 3. The need of an entire revision of our knowledge of the ossification of the bones by means of skiagraphy is clearly indicated by this contribution of Mr. Poland's.

In two chapters, upon the pathological anatomy of separations, we have a résumé of the experimental work done to show where the line of separation occurs, and the evidence derived from the examination of specimens produced by natural means. Whilst considerable divergence of opinion has existed on the subject, the balance of evidence seems to point to the fact that separation occurs most often through the spongy layer of Broca; and to this fact, that the fracture does not disturb the epiphyseal line itself, is usually ascribed the comparative infrequency of shortening. But there is another view of the matter, and the author very rightly lays stress on it—that shortening does not depend so much on the line of separation as upon the proper want of adjustment of the fragments.

The subject of separation in general is admirably treated in the three chapters on symptoms, prognosis, and treatment, and this part of the work should be studied by all who wish for general information on the characters and results of these lesions. These chapters may certainly be regarded as the most interesting and perhaps the most instructive part of the volume. They comprise practically in themselves a résumé of the whole subject. The results of this form of injury, such as non-union, deformity, ankylosis of neighbouring joints, and the more remote contingencies, are all dealt with, and impress one with the necessity of a more careful study of these lesions and the methods of dealing with them. The general lines of treatment are carefully laid down. When reduction cannot be effected, the author recommends that the fragments should be cut down upon. "This would be imperatively demanded," he says, "if Ollier and other authors were correct in their belief that arrest of growth in the limb is usually due to the want of proper coaptation of the separated surfaces." His advocacy of operative measures certainly seems to be justified by the cases quoted, and if support were needed, it would be supplied by the success that has resulted of late years from similar measures resorted to for fractures.

Detailed accounts then follow, of the separations of the different epiphyses, one of the most admirable features here being the illustrations. In this respect, as in others, no pains or expense has been spared by author and publishers to make the book as complete and useful as possible. The space devoted to the three most common forms of separation is over a hundred pages apiece. Under the heading of "Separated Epiphyses of the Upper End of the Humerus," the author boldly lays down the law with regard to the treatment of separation complicated with dislocation. Waiting for union and then reducing the dislocation should never be resorted to. Except in rare cases, when reduction of the dislocation is possible, the complication should be treated by cutting down and reducing the head of the bone. This method was advocated by Dr. M'Burney in a case of fracture and dislocation in 1894, and carried out successfully by him on an adult patient.

In considering the lower epiphysis of the femur, the author quotes Dr. Harte, to the effect that nearly one-half of sixty cases terminated in amputation. This appears to be a startling proportion, and is doubtless far in excess of what would be the case at the present day. For, putting on one side such severe injuries to soft parts as would necessitate immediate amputation, modern antiseptic methods would doubtless lead to the saving of a considerable proportion of the cases of compound fracture; and cases of secondary hæmorrhage, gangrene, and other complications would be anticipated now by open operative measures. In this part of the work, many cases in which operation has been resorted to are recorded in considerable detail, and it is in these careful records that so much of the value of the work lies. Separation of the upper epiphysis of the femur accounts for thirty-five only of 693 cases. I think there can be no doubt that this fails to give anything like the real proportion of this injury, nor do I think the difficulties of diagnosis are so considerable as the author maintains.

Finally, the book is well indexed. Mr. Poland is to be congratulated on having brought to a successful termination a work involving so many years of labour. G. BELLINGHAM SMITH.

La Maladie de Carrion, ou la Verruga Péruvienne. Par ERNESTO ODRIOZOLA, Professeur à la Faculté de Médecine de Lima, etc. Paris: Georges Carré et Naud. 1898.

THE study of any disease strictly limited in its distribution to certain well-defined areas, which present features in common, is sure to be an interesting one. It seems possible, in such circumstances, to grasp the true cause, and, that effected, to devise means for its prevention, if not for its cure. Throughout special districts of the departments of Ancachs and Lima, in Peru, on the western slopes of the Andes, there prevails a curious endemic disease, known as verruga. This appears to occur nowhere else. As a rule, it lurks in deep, narrow, ill-ventilated valleys, where the heat is suffocating and vegetation luxuriant. It is not met with in all the defiles, even though their characteristics are similar, but is most severe in its incidence in those which run north and south, and are thus cut off from the purifying breezes blowing inwards from the sea. The streams which water these often overflow their banks, and on their subsidence leave thick deposits of alluvium and detritus, whence arise malarial fevers, almost inseparable from that essential to verruga. The verrugas, as the eruption is termed, are little, reddish, warty-like growths, which appear on the face and limbs, or form on the mucous surfaces, or within the muscular structures; or larger nodules or tumours. These take origin in the vasculo-connective tissue, either free or interstitial. As regards the skin, they are sub-epidermic, intradermic, or hypodermic. From their softness and vascularity they tend to bleed readily and copiously.

When in 1871 the Oroya Railway was constructed, a severe and fatal form of fever followed the track of the disturbance of the soil necessitated by its formation. This had never previously been so prevalent, and was called Oroya fever. Some classed it with malarial fever, while others traced a connection between it and the pyrexia often associated with verruga. The latter view gained ground; and on 27th August 1885, Daniel Carrion, a student of medicine, inoculated himself in several places with blood from a verrucous tumour. On the 17th September the first symptoms showed themselves, and he died on the thirty-ninth day after inoculation, in a state of profound anæmia, having passed through all the stages of the so-called Oroya fever. This quite conclusively proved the identity of the diseases. The sad termination of the experiment is the more to be regretted, as verruga is one to which domestic animals are liable in common with man, so that the observation might have been made on them with almost the same probative effect. In recognition of his self-sacrifice, his comrades desire it to be known as "Carrion's Disease."

In the work under review, Odriozola furnishes the first complete account of the complaint, based on the assumption that the fever of

Oroya is nothing else than the febrile stage of verruga. Commencing with the history, he gives a résumé of what previous authorities have written about it. Then he describes the geography, botany, and natural history of the situations in which it occurs, the localities to which it is limited being marked out in colour in a series of excellent maps. A full account of the malignant fever associated with it follows, illustrated by typical cases and charts; next the eruption is described, and its nature made fully intelligible by numerous coloured plates and woodcuts. Our interest centres in the etiology and mode of communication. Two suggestions are made. It may possibly be conveyed by drinking water, or it may be introduced into the system by emanations from the soil, for it is not contagious in the ordinary sense of the term. A very short sojourn in the infected areas is sufficient to acquire it, even sleeping one night seems ample. Since its inoculabilty has been proved, it is surprising that this source has not been alluded to by the author. It strikes us as most probable that the mosquito or some other dipterous insect is the real agent of communication. Mosquitoes abound in such steamy hot valleys as are the home of verruga, and the mosquito does not wander far from its birthplace. This view would explain the risk to strangers and new-comers, and the comparative immunity of those naturalised to the districts. The disease possesses no affinity to yaws or syphilis, though it can be passed on by a mother to her unborn infant; hence, neither mercury nor iodide have proved of value in treatment. Nor has quinine any effect, save to combat a complication with malaria. The author seems to have faith in Hayem's serum, but no definite proof of its value is forthcoming. The book is clearly and pleasantly written, and if at times there is a tendency to reiteration, this arises from a desire to emphasise his opinions. W. ALLAN JAMIESON.

Les Actualités Médicales.—(1) *La Diphtérie*, by Dr. H. BARBIER; (2) *Les Rayons de Röntgen et le Diagnostic de la Tuberculose*, by Dr. A. BÉCLÈRE; (3) *Le Tétanos*, by Drs. J. COURMONT et M. DOYON; (4) *Radiographie et la radioscopie cliniques*, by Dr. L. R. REGNIER; (5) *Les Glycosuries non diabétiques*, by Dr. GERMAIN ROQUE. Paris: J. B. Baillière et Fils. 1899.

THE series of neat little volumes, dealing briefly but practically with various topics, now in course of publication by the well-known firm of Baillière in Paris, forms an excellent reference library to the subjects discussed. Each volume is neatly bound in pale green, and is of equal length; indeed it must have occasioned exercise of much ingenuity on the part of the authors to so exactly fit their subjects to fill the ninety-five pages which three of the above occupy, or the ninety-two taken up by the other two. To render books like these of service a good index is indispensable; but even Baillière et Fils are unable to advance further than the usual French custom of only giving the headings of the different chapters or sections in its place. Perhaps the most interesting of the numbers under review is that placed second in order above; it is illustrated with several figures of apparatus and of the actual appearance shown by tubercular lungs. Dr. Roque treats of non-diabetic glycosuria under the types of—(1) Intermittent arthritis, (2) digestive, (3) nervous, and (4) puerperal.

Those which accompany arthritis are divided into a hereditary form in young subjects, in gouty adults, in old persons, and in the obese; and a nitrogenous form.

Those of nervous origin are classed with regard to organic nervous lesions, functional diseases, and traumata. Suggestions and hints as to diagnosis, prognosis, and treatment of the various types follow.

The volume upon tetanus deals largely with its bacterial aspect, the results and significance of its antitoxin treatment. The same applies to Barbier and Ulmann's description of diphtheria. Both are of value in spite of their lack of indices. A. Lockhart Gillespie.

A Text-Book upon the Pathogenic Bacteria for Students of Medicine and Physicians. By Joseph M'Farland, M.D., Professor of Pathology in the Medico-Chirurgical College, Philadelphia, etc. With 134 Illustrations. Second edition, revised and enlarged. London: Henry Kimpton. 1898.

It is little more than two years ago since the first edition of this excellent and compact text-book of bacteriology was published. It very soon established a reputation for simplicity, clearness, and accuracy, and became a general favourite among students of bacteriology. Since its first appearance other and powerful competitors for popular favour have appeared, but it is safe to say that this new edition, enlarged and extended, as it has been, to cope with the galloping advances of the science, will hold its own. Several new features have been introduced into the present edition. References have been added in the form of footnotes. It is doubtful if this is the best form of giving a bibliography, but the change is undoubtedly a good one, and is to be commended. Again, the author has added considerably to the technique, so as to make the manual more generally helpful in the laboratory. The bacteriology of whooping-cough, mumps, yellow fever, hog cholera, and swine plague has received careful notice in this edition. The organisms *Bacillus aërogenes capsulatus* and the *Proteus vulgaris* have been described. A chapter upon the determination of the thermal death point has been introduced. This includes the important subject of the methods of determining the antiseptic and germicidal value of reagents. Other notable changes and additions have been made, and, as is but natural, the present edition marks a distinct advance upon the last. Excellent though it is, however, it yet leaves something to be desired. The chapter upon susceptibility and immunity is hardly so thorough and complete as it ought to be. In measles, too, there is no notice taken of the protozoa described by Doehle, by L. Pfeiffer, and also by Behla. Further, it will be regretted by many that the author has not touched upon such important diseases as dysentery, malaria, and smallpox. It is true that, rigidly speaking, they should not be included, but their relationship and analogies strongly suggest the advisability of doing so. It is to be remembered, however, that the author had not unlimited space at his disposal, and that he has given us an excellent, reliable, and handy text-book of bacteriology. R. F. C. Leith.

The Diseases of Children. By James Frederick Goodhart. 6th Edition. London: J. & A. Churchill. 1899.

At the present time, when it is daily becoming a greater mystery how one small brain can carry all the medical student is expected to know, the demand for short manuals on the special courses now included in the curriculum is gradually replacing the more lengthy text-books on those subjects.

Goodhart's work on "Diseases of Children" has for many years enjoyed a well-deserved popularity, and the issue of a sixth edition will in no wise detract from its reputation.

With the co-operation of Dr. Still, the work has been critically revised, several new diseases have been described, the doses and remedies have been adjusted according to the British Pharmacopœia of 1898, and the volume, as a whole, brought well up to date. Written in a crisp, concise, and original style, the author frequently wanders from the beaten track traversed by other writers, and places before his readers, sometimes we think almost too dogmatically, the opinions which he has formed as the result of his wide experience. We are glad to notice the omission of many of those familiar statements, copied so religiously from text-book to text-book, but which have little more to recommend them than that they were the views of some prehistoric authority, whose opinions, like the laws of the Medes and Persians, cannot be altered.

The chapters on the respiratory system are hardly full enough, and are too sketchily written for a subject claiming such a lion's share of the pediatric physician's work, and the differences between the clinical aspects of those conditions in the child and adult are not sufficiently emphasised. Perhaps the best sections of the book are those devoted to treatment. The rules laid down for the feeding, upbringing, and general management of the infant are most excellent, and the indications for the treatment of the various diseases are full of resource and leave little to be desired.

The book may be confidently recommended as a safe guide, on the subject of which it treats, to the student and practitioner.

G. H. Melville Dunlop.

The Cold-Bath Treatment of Typhoid Fever. By J. A. Hare, M.D. London: Macmillan & Co.

The cold-bath treatment of typhoid fever has never yet been popularised in this country. It will be surprising, however, if Dr. Hare's book does not lead to a fair trial being given to this system, which has had such excellent results in America and on the Continent. For we may state at once that this monograph puts bathing in a much more attractive light than any other work we have read, and, further, that the completeness and excellence of its statistics puts it almost above criticism.

Dr. Hare reports two long series of cases treated in the Brisbane Hospital. The first series consists of patients treated expectantly up to the year 1886, and furnishes 1828 consecutive cases. The second series furnishes 1902 cases treated systematically by cold baths since the beginning of 1887. During the first period the death-rates of males and females were 14·14 and 16·02 per cent. respectively, while during the second the mortality of males was reduced to 8·7 and that of females to 5·6 per cent. It would appear, therefore, that females benefit more than males by this treatment, a fact which is to be explained by the greater liability of the latter to hæmorrhage and perforation, two conditions, the incidence of which is practically unaffected by cold baths, although certainly it is not increased. The change in the death-rate, therefore, would appear chiefly to refer to those cases which are liable to succumb to pyrexia and toxæmia. Whether the antipyretic action of the baths is the cause of the success of the treatment, as the author appears to believe, or whether it is not rather due to the increased elimination caused by the free diuresis which they undoubtedly promote, is, we think, still open to question. In any case the large number of cases quoted leaves in our opinion no possibility of doubting the success of this method, the technique of which is described in detail and illustrated by excellent photographs. We would point out, however, that the temperature of the baths is not so low as that recommended by Brand, and that in such a hot climate the objections of the patient are more easily overcome than they would be in this country.

Claude B. Ker.

Transactions of the Royal Academy of Medicine in Ireland. Vols. XV., XVI., 1897–1898. Edited by John B. Story, M.B., F.R.C.S. Dublin: Fannin & Co., Ltd.

Both volumes of these acceptable *Transactions* fully maintain the former standard of excellence characteristic of earlier issues. The first commences with an admirable lecture by Professor His on the "Development of the Brain," illustrated by delicate reproductions of his most artistic blackboard drawings, personally drawn for his lecture. It would be too arduous a duty to notice all the interesting papers included in the two volumes, and much too hazardous a task to particularise as to their merits, where so many are good, so many are concerned with such widely separated subjects. One suggestion might be made to the editor, namely, that a separate index to the first fifteen or sixteen volumes, giving the titles of the papers and the names of their authors, so that reference to the numerous valuable contributions throughout the series might be rendered less arduous, would be a boon. If such a general index is in existence for any earlier part of the series, we plead guilty to ignorance; should there be no such, its compilation would not be labour lost.

A. Lockhart Gillespie.

A Synopsis of Surgery. By R. F. Tobin, F.R.C.S.I. London: J. & A. Churchill. 1898.

The contents of this handy volume were originally distributed, in the form of leaflets, to the members of the author's clinical class, with the very laudable object of systematising clinical teaching, and of rendering attendance at theoretical lectures less necessary. The letterpress is interleaved with blank sheets, to allow of the addition of notes and commentaries. The work does not pretend to take the place of a text-book of surgery, nor is it a cram-book, in any sense of the term; it conveys very clearly and concisely the principles and main facts of surgery; and is very much the kind of book we should desire to see in the hands of students on commencing their hospital practice.

ALEXIS THOMSON.

On the Origin and Progress of Renal Surgery. By HENRY MORRIS. London: Cassell & Co. 1898.

MR. MORRIS' Hunterian lectures are so well known to the profession, through having appeared in the *Lancet*, that it is quite unnecessary to review them in their present published form. The lectures are mainly concerned with the subjects of stone in the kidney and ureter, the surgical treatment of calculous anuria, and of subparietal injuries of the ureter. Nearly one-half of the book is made up of tables of 267 cases of renal operations performed by the author, and of forty-nine collected cases of operations for calculous anuria.

The full and accurate record of Mr Morris's extensive experience in this special branch of surgery, as presented in this attractive volume, is of the greatest possible value, and is certain to meet with the recognition which it deserves.

ALEXIS THOMSON.

Transactions of the American Orthopædic Association. Vol. XI. Philadelphia. 1898.

THESE *Transactions* are always of great interest to the practising surgeon, and the present volume is no exception to the rule. It is larger than its predecessors and it contains a number of papers which will appeal to a wider circle of readers than those specially devoted to orthopædics, witness the article by Nichols of Boston on "Tuberculosis of Bones and Joints." It is impossible even to give a list of the titles of the other fifty-three papers; it must suffice to say that they are all characterised by originality and brevity, and are well worth reading. The excellent illustrations are again a special feature of the book.

ALEXIS THOMSON.

Notes on Surgery for Nurses. By JOSEPH BELL, F.R.C.S. Ed. Fifth Edition. Edinburgh: Oliver & Boyd. 1899.

IT is more difficult to teach or to write a book on surgery for nurses than for medical students. Mr. Bell has shown special gifts as a teacher and as an author, in relation to each of these sections of our common profession. That his "Notes on Surgery for Nurses" has run through four editions in little more than ten years, is sufficient evidence of its excellence and of its popularity. In the present one there is added an appendix dealing admirably with the relations of the trained nurse to the profession and to the public.

ALEXIS THOMSON.

RECENT ADVANCES IN MEDICAL SCIENCE.

MEDICINE.

UNDER THE CHARGE OF

A. LOCKHART GILLESPIE, M.D., F.R.C.P.Ed.,

MEDICAL REGISTRAR, EDINBURGH ROYAL INFIRMARY.

TUBERCULOSIS IN SANATORIA.

THE fourteenth Annual Report of the Adirondack Cottage Sanatorium (*Med. News*, Phila., 25th February 1899) notes that thirty-eight patients were apparently cured during 1898, in forty-four the disease was arrested, thirty-nine were improved, fourteen not relieved, and six died out of 141 (not taking note of three cases leaving in less than a week), or 4·2 per cent. Wilder (*New York Med. Journ.*, 4th March 1899) gives further statistics of this sanatorium.

Number of Patients treated in 1897–98, *and staying more than Three Months*, 203 ; *Average Time of Treatment, Nine Months.*

Condition of Patients when Admitted.		Apparently Cured.	Disease Arrested.	Improved.	Unimproved.	Died.
Incipient	75	55	16	2	2	0
Advanced	84	15	38	19	11	1
Far advanced . . .	44	0	7	19	13	5
Total	203	70	61	40	26	6

The report of the Loomis Sanatorium from 1st November 1897 to 1st November 1898 (*ibid.*, 7th January 1899) gives twenty-eight cases out of 204 as cured, sixty-seven as improved. In the first sanatorium, of thirty-four patients resident for less than three months six were cured, in seven arrest had occurred, thirteen improved, one died, and three became worse ; of 110 patients who stayed over three months (mean stay, nine months), twenty-seven out of thirty-six incipient, and nine out of forty-eight advanced, cases were apparently cured ; in seventy-eight and twenty-one cases respectively, and in three far advanced cases, the disease was arrested.

Exchaquat gives an account of the treatment of phthisical patients at the sanatorium at Leye. The results in 146 cases give thirty-three "cures," seventy-eight improved; worse, twelve; *in statu quo*, seventeen; died, seven. Forty-seven of the patients are put in the first or early stages, with twenty-six, or 55 per cent., "cures," and no deaths: seventy in the second, six "cures," or 9 per cent.; four deaths, or 6 per cent.: and twenty-nine in the third or advanced stage, with one "cure," or 3 per cent.; and three deaths, or 11 per cent.

The *Progrès méd.*, Paris, for 14th January 1899, reproduces a lecture by Lafitte upon "Sanatoria and Highland Stations," so full of facts of interest regarding them, from various points of view, that an abstract to be satisfactory would have to include most of it. Among the facts worth individual mention are the following:—Jordanet, in 1865, showed that in one of the towns of Mexico (200,000), at a high altitude, the deaths from phthisis were one to eighteen compared with the one to eight in London, and one to five in Paris. The advantages shown by lands of high altitude were, generally, decreased humidity, clearness and purity of the air, and the wind currents blowing more regular, quarters and periods, as a rule. As oxygen is present in smaller quantities in the air as one ascends, greater efforts are called for in respiratory movements; indeed, at Barèges, 1270 metres above sea level, the girth of the thorax has been found to have increased in a few months by 2 to 3 cms.; while Veraguth was able to inhale 2·7 litres of air at Zurich, and 3·5 litres at Saint Moritz, during a few days after his arrival, 3 litres subsequently. It has been shown that the blood has a greater power of absorbing oxygen at high altitudes, and contains a greater proportion of iron. A description of sanatoria follows. The essential rules which should be borne in mind when deciding upon the site and surroundings of a sanatorium are, in Laffitte's judgment, —(1) Sanitary conditions, (2) healthy climate, (3) no endemic or epidemic enteric fever, (4) not a trace of malaria.

These are indispensable. Further, a good and pure water supply, and the possibility of obtaining milk in abundance from non-tuberculous animals; absence of factories from the neighbourhood, pine forests near at hand, high ground, or, if a valley, one facing the south; never more than 100 patients, the front of the building placed to the south, or, if this exposes it to any very prevalent winds, with as slight a deviation from that direction as practicable. All these points should be remembered. Lafitte then takes Hohenhonnef as his model sanatorium,—Hohenhonnef, "the Nice of the Rhine," and describes it in detail. Falkenstein, Nordrach, Geerbersdorf (with three), Saint Blasim, and Ruppertshain (for necessitous patients), in Germany; Leysin, Davos, Aroza, in Switzerland; and Durtol (near Clermont-Ferrand), Vernet-les-Bains, Ormesson (for children), d'Hauteville (being constructed near Lyons for poor patients), Agincourt (also building for the poor), in France, are the better-known sanatoria.

What is the future history of the "cured" or "almost cured" cases after leaving sanatoria? Are the cases absolute or relative? No sufficiently categorical statistics have been given by any of the institutions, to allow of a definite conclusion.

von Ziemssen, last year (*München. med. Wchnschr.*, Jan. 4, 1898) summed up the benefits of high altitudes—(1) The air is purer and more free from dust, gases, and bacteria than the lower atmosphere; (2) the ozone is in greater proportion; (3) the atmospheric pressure is less, but what effect this has is not clear; (4) the warmth of the sun's rays in winter is greater, the air movements less marked, the air drier, mist rarer, bright sunshine more common; (5) the red blood corpuscles increase in number, even at Hohenhonnef—only 236 metres above sea level this gain is appreciable; (6) a greater resistance to atmospheric vicissitudes is exhibited. On the debit side of the ledger may be placed the frequent difficulty of acclimatisation experienced with many patients, especially those who are not in sanatoria, the usually long journey, leaving home and family, and the frequent deaths of others at the sanatorium or health resort,—these influence the mind, and psychical conditions are by no means the least important.

The *Deutsche med. Ztg.*, Berlin, for 2nd January 1899 contains an article "On the Question of Pulmonary Sanatoria," by "A." One of the chief points discussed is the short period of three months which has come to be regarded as the regulation length of stay to be recommended for patients. This may be taken as the official minimum, though often less time has been spent in a sanatorium when a "cure" is pronounced; while the mere fact of mention being made of a definite period causes many to leave before the "cure" is made absolute. Six months should be spent in trial of the method, by all individuals in the early stages of phthisis, although six months is often little enough. As one-seventh of mankind die of tubercular disease, where are all the necessary sanatoria to be erected, and who is to pay for them, if this method of treatment be made compulsory? "A." does not know.

Kusthy ("Ueber Lungenheilanstalten," Wien u. Leipzig, 1898) estimates the number of consumptives in Hungary at 500,000; in 1896, 74,000 died of phthisis. A Sanatorium Society has been recently formed to afford means for providing this form of treatment after the German model.

Freudenthal (*New York Med. Journ.*, 26th March and 9th April 1898) contributes a most instructive paper upon the climate of part of the South-Western States of North America. He deals principally with New Mexico and Arizona. Perhaps the most interesting statement is connected with laryngeal tuberculosis. Phthisis laryngis is not a favourite condition with sanatorial physicians; indeed, as a rule, most authorities agree that high altitudes are contra-indicated, should it be present; but Freudenthal opines that this conclusion is based upon error. He has observed, and been informed by actual observers, that cases of laryngeal phthisis may do very well in the highlands of South-Western America, especially in places where they are not required to speak much, because there is no one scarcely to speak to. If possible, local treatment should be instituted; if not, the open air and silence may suffice.

At Tucson in Arizona the annual mean humidity of the air in 1893 was only 42 per cent., at Denver not 44; Tucson is 2432 ft., Denver 3287 ft. above sea level; and in 1893 the maximal temperatures recorded were 107 and 96 respectively. During the month of June

1893, Tucson returns the extraordinary figure of 22 per cent. as the value of water vapours in its atmosphere. In short, the country described by Freudenthal appears itself to be a gigantic sanatorium, planned for the cure of the world's consumptives.

Netter and Beauvalon (*Gaz. hebd. de méd.*, Paris, 18th August 1898), in a communication given to the Congress upon Tuberculosis, held in Paris last summer, conclude that sanatoria for the poorer consumptive patients are desirable, as they afford a means for isolating tubercular cases who constitute a danger to the community; as they are equipped as fully as is possible for the most advantageous treatment of phthisis; and as sanatoria reserved for lung disease, and for none but those in early stages, yield by their successful results a gain to the State, pecuniary and social. The figures of life assurance offices bear witness to this.

The question as to whether it is advisable to give alcohol to sanatorium consumptive patients is discussed by Liebe (Tübingen, 1898, referred to in *Deutsche Med. Ztg.*, Leipzig, 2nd Feb. 1899), who fears that alcohol given to consumptives, though organically beneficial, proves to be mentally dangerous for them. Brehener, however, calls alcohol his *Heilmittel*, and the fact that it is abused by some ought not to exclude from our means one of the most valuable drugs known to us.

SURGERY.

UNDER THE CHARGE OF

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OPERATIVE INTERFERENCE IN TABETIC ARTHROPATHIES.

DR. ULLMANN has collected a series of cases in which tabetic arthropathies were made the subject of surgical interference. Excision of the joint was performed once in the shoulder and thrice in the hip with good results, five times in the ankle with good results in three, and nine times in the knee; in the last named joint a successful result was obtained in only one case, in four the operation was a failure, and in two it was followed by death. Amputation was performed in thirteen cases, primary in eight and secondary in five; thrice in the thigh and five times in the leg; the results were scarcely any better than the excisions of joints. Excision, according to the author, is only justifiable when, assuming the operation will be successful, the disease of the spinal cord will allow of the patient going about. In other cases, and in those where there is persistent suppuration, it is better to amputate if one is to interfere at all.

A supporting apparatus must be worn after a successful excision, in order to prevent a relapse of the disease, as complete consolidation is the exception.—*Centralbl. f. Chir.* Leipzig, 18th Feb. 1899.

ON SCIATICA AND ITS TREATMENT.

DR. HILTBRUNNER has written an inaugural dissertation on the above, based on a series of fifty cases in the clinique of Professor Kocher. Sciatica is often accompanied by a typical form of scoliosis, in which the trunk is inclined towards the unaffected side. This attitude is the result of the implication of the sensory nerves in the region of the erector spinæ, quadratus lumborum, glutei, etc. The patient avoids the contraction of those muscles because of pain, hence the body weight is chiefly displaced to the healthy side. Other forms of scoliosis are exceptional. As regards treatment, there were first a series of twenty-one cases subject to hydropathic methods; of these seven recovered, seven were improved, and seven were not influenced at all; of the seven recoveries, all relapsed except one. Massage was employed very successfully in four cases by itself, twice along with nerve-stretching (without operation), and once along with faradisation. In six cases, massage had no influence whatever. Electricity was employed in fourteen cases, excluding those in which other measures were also employed; there were two recoveries from the constant, and two improvements from the faradic current. Carbolic lotion (4 to 5 per cent.), osmic acid, and antipyrin were each tried in the form of subcutaneous injections without any decided benefit. Stretching of the nerve by manipulation of the limb, yielded improvement in three cases, gave no results in one case, and made a third case worse. Stretching of the nerve by open operation was carried out in thirteen cases; of these five were completely cured, although the recovery did not always immediately follow the operation. In two cases the recovery was not permanent, for a relapse took place, in one of them after an interval of three years. Two cases were only improved. Finally, the operation was followed in two cases by an exacerbation of the disease; one of these began to improve after a year, in the other the pains disappeared after three weeks.

The author concludes that the operative treatment of sciatica is *the* one to be recommended where other measures have failed.—Inaug. Diss., Bern, 1898.

ON THE PREVENTION AND CORRECTION OF SHORT LEG IN HIP DISEASE.

MR. ROBERT JONES of Liverpool refers to the frequency with which deformity results from hip disease and the difficulty of preventing it. The deformities which constitute short leg comprise all or most of the following conditions, namely, flexion, adduction, internal rotation, tilting of the pelvis, and arrest of growth. Excepting the last of these, Mr. Jones thinks they may be avoided where the disease is met with at an early stage. The shortening from arrest of growth can only be modified by deflecting the pelvis sufficiently, to insure apparent equality of the limbs. That the body weight is not the all-important factor in the production of the deformity, is shown by the fact that the whole series of elements which make for short leg may appear while the patient is lying in bed.

He recommends the Thomas splint as the best routine appliance for hip disease. To control deformity, it is best to add to it the "abduction body-wing," or to employ the author's special abduction splint. The new splint is so designed as to keep the diseased limb fixed in the abducted position, and it effectively prevents any elevation of the anterior-superior spine on the sound side.

The author agrees with the late Mr. Thomas and with Mr. Rushton Parker in rectifying existing hip deformities in cases where the disease is still in an active state, instead of waiting until it has subsided or cured. Having restored, as far as possible, the displacement of the hip and the length of the limb, the patient is placed in the abduction splint, and kept in it until that phase of the disease which is productive of deformity has passed. When the disease admits of it, he may be transferred to an ordinary walking splint, the abducted limb meanwhile being brought into its proper position. During the whole treatment, the patient must live as much as possible in the open air.

In cases which apply for the treatment of deformity after the cure of their hip disease, there may be shortening, varying from 2 to 6 or 7 in., due to femoral displacement and oblique pelvis. These cases can only be benefitted by operative measures. Where flexion is the chief element in the deformity, the author prefers Adams' osteotomy of the neck of the femur; where there is adduction with tilting of the pelvis, he makes the section obliquely through the great trochanter, and divides the adductors subcutaneously. Traction is then applied to the limb by means of pulleys; the leg, still fully extended, is placed in a position of abduction, which varies with the degree of the tilting of the pelvis. The greater the tilting, the greater should be the abduction. The pelvis on the sound side is fixed, the abduction splint is applied, and extension and abduction are maintained. Seven or eight weeks are allowed to elapse until bony union is complete, with the limb in abduction and at an angle with the body. The length of the limbs may then be equalised in one or two ways, or by a combination of both, by either lessening the practical length of the sound limb or increasing that of the short one. By osteotomy and extension the affected leg is actually lengthened. By the osseous union of the femur, in abduction with the pelvis, the elevation of the pelvis on the opposite side is effected. In this way several inches of shortening may be remedied.

The author refers also to the more difficult class of cases, in which cure of the disease has taken place with short, fibrous ankylosis. In these he divides the adductors, extends and forcibly abducts, but if the attempt at lateral movement threaten fracture, he performs an osteotomy as if for bony ankylosis. Such cases require to be kept much longer in a splint than do those where the fixation is bony. When the splint has been removed, the patient should be confined to bed for some time until the abducted limb is gradually brought into position, and exercises should be prescribed designed to elevate the pelvis on the sound side, and depress it on that of the diseased. Finally, with regard to the question of making a false joint, the author only employs it for the relief of double ankylosis.—*Trans. Am. Orth. Ass.*, Philadelphia, 1898, vol. xi.

CANCER OF THE LIP.

DR. T. **FRICKE** has analysed 127 cases of this from the clinique in Göttingen, and incorporated them with those recorded by Wörner, Koch, Maiweg, and Regalski, making a total of 1338 cases of cancer of the lip. The following are his conclusions:—The disease is nineteen times more frequent in the lower than in the upper lip; the lower lip is twelve times more commonly affected in men than in women. Of three cases of cancer of the upper lip, one was a female; 60 was the average age of the Göttingen cases; the majority occurred between the ages of 55 and 65; a large proportion of the men suffering from cancer of the lip had to do with the surface soil, or the care of animals; heredity, smoking, and the abuse of alcohol did not appear to be of any etiological importance; injuries of the lip seemed to have greater influence in the causation of the disease; the lymphatic glands may be infected without being felt to be enlarged, and before the primary growth is larger than a hazel nut; the jaw is usually affected later; it is fairly common after the disease has lasted nine months; it practically excludes any prospect of radical cure by operation. The favourite method of removing cancer of the lip is the wedge excision. The contents of the submaxillary triangles should always be explored, and if necessary cleared out; for aseptic reasons it is better to do this before dealing with the primary growth on the lip.—*Centralbl. f. Chir.*, Leipzig, No. 15, April 15, 1899.

THE RECENT OPERATION ON THE POPE.

HIS HOLINESS appears to have had a blood cyst in the left loin for more than twenty-five years. It never caused any inconvenience until quite recently, when it became inflamed and gave rise to considerable pain and fever. His own physician, Dr. Lapponi, along with Dr. Mazzoni, who was called in consultation, had some difficulty in persuading the august patient of the necessity of extirpating the suppurating cyst; he finally consented, however, to this being done, and the operation was carried out by Dr. Mazzoni on the 1st of March. The Pope being in his ninetieth year, it was not considered safe to put him under chloroform; local anæsthesia was effected by an injection of cocaine, sodium chloride, and morphine. The incision was 4¾ in. in length, and the cyst removed was the size of an orange; the wound was not sutured, in deference to the wishes of the patient. After the operation was completed, he insisted on seeing the specimen removed, and he congratulated Dr. Mazzoni on his courage in operating upon a man of his age. The wound healed without an untoward symptom.—*Gaz. méd. de Paris*, Nov. 3, 1899.

OBSTETRICS AND GYNÆCOLOGY.

UNDER THE CHARGE OF

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METHOD OF CREATING A VAGINA IN A CASE OF CONGENITAL ABSENCE.

ROBERT ABBE (*Med. Rec.*, N.Y., 10th Dec.) planned and carried out the following operation on a woman, æt. 21, who suffered from congenital absence of the vagina. A crescentic cut was made across the interlabial space with the concavity upward, thus getting a little shelf of mucous membrane below the urethra to divert escaping urine. By blunt dissection, a free cellular space to the depth of 5 in. was made between bladder and rectum, which was temporarily packed with sterile gauze to check oozing. Thiersch grafts were then cut from the thigh, sufficiently large to cover an ample plug, made thus:—A thin French rubber condom, such as can be obtained at drug stores, was sterilised and stuffed with long strips of iodoform gauze. Upon this the skin grafts were spread, with their wet sides outwards and edges freely overlapping, numerous small punctures having been previously made in the rubber, so as to allow the gauze to absorb any discharge. A piece of rubber tubing, wrapped loosely in iodoform gauze, was now inserted into the rectum, to facilitate the free exit of gas during the subsequent prolonged constipation. Finally, the graft-covered form was carefully passed into the new vaginal space, the walls of which were held apart by three deep retractors, which, on removal, allowed the fresh surfaces to come into closest contact with the wet surface of the grafts. The plug was held in position by two silkworm gut stitches passed across the vulva, transfixing the plug, and tied over iodoform plugs at either side. The urine was drawn off every eight hours for a week, and the bowels confined for ten days, when the plug was removed, the packing being taken out before the rubber. The grafts all had taken, and the new cavity measured 4½ in. deep. The patient was married ten weeks after operation, and keeps the vagina open by wearing a wax bougie part of every day.

REMARKS ON THE USE OF MAMMARY GLAND AND PAROTID GLAND DESICCATIONS IN GYNÆCOLOGY.

JOHN B. SHOBER (*Am. Journ. Obst.*, N.Y., February 1899) advocates the use of mammary gland and parotid gland preparations in the treatment of fibroid tumours of the uterus and in certain forms of ovarian disease respectively.

Since he began to use the mammary gland tissue, more than a year ago, he has only operated on one case of uterine fibroid where the pressure symptoms and hæmorrhage were too urgent to permit of palliative treatment. He has treated a number of cases, ranging from large multinodular tumours to small subserous nodules, with satisfactory results, the menorrhagia and metrorrhagia ceasing and the tumours diminishing in size. The gland is administered in the form of tablets, each containing an equivalent of 20 grs. of the fresh gland, three to six tablets being given daily.

He has also used the parotid gland in the treatment of ovaritis, enlarged, congested, and tender ovaries, ovarian neuralgia, and ovarian dysmenorrhœa, and has obtained better results than by any other form of treatment. Positive results cannot be expected, when there is associated disease of the Fallopian tubes, as hydrosalpinx, pyosalpinx, and pelvic inflammatory disease.

TREATMENT OF PLACENTA PRÆVIA BY REMOVAL OF THE UTERUS.

LAWSON TAIT (*Lancet*, London, 11th February) considers placenta prævia to be a much more fatal condition than is generally supposed, and believes that more than half the cases die. He advocates removal of the uterus as giving the best chance of saving both mother and child, and reports a case. The patient had a bad history of hæmorrhage. She had been twice curetted, and had nearly died of post-partum hæmorrhage after her second confinement, and had had several miscarriages. When he was called to see her she had been bleeding profusely for five hours, notwithstanding treatment. The pregnancy was at term, the cervix closed and rigid, and the liquor amnii drained off. He suggested hysterectomy, which was agreed to by the doctor, husband, and the patient herself. The operation was performed with the aid of the elastic ligature, by the "Tait-Porro" method. A healthy female child was born (which died a month after from bronchitis), and the mother made an excellent recovery.

ANEURYSM OF THE UTERINE ARTERY.

MUNDÉ (*Med. Rec.*, N.Y., December 1898) records a case of this rare condition. On vaginal examination, a soft, elastic, pulsating tumour, about the size of a hen's egg, was found in the left vaginal vault, lying close to the cervix and extending for a short distance down the left vaginal wall. As rupture seemed imminent, he opened the abdomen, and with some difficulty isolated the artery, which he ligatured, completely stopping all pulsation in the tumour. After the operation, a faint, apparently transmitted, pulsation could be felt in the aneurysm, which increased, and was thought to be due to the establishment of a collateral circulation, but the tumour itself was smaller and its wall hard and solid. To effect a more complete cure, he tried galvano-puncture of the aneurysm twice, at intervals of three days, passing a current of 20 milliampères through it for half an hour, which caused considerable further shrinkage of the tumour. Six weeks after operation the swelling was half its original size, hard and painless, and pulsated no more than a normal uterine artery.

BLOODLESS AND ASEPTIC VAGINAL HYSTERECTOMY.

JOHN BYRNE (*Am. Gynæc. and Obst. Journ.*, N.Y., January 1899), under the above heading, describes a series of cases of vaginal hysterectomy, performed by means of the galvano-cautery knife, without the use of any other cutting instrument throughout the operation. In his earlier cases he had to secure the uterine and ovarian arteries by means of Péan's compression forceps, left in the vagina for forty-eight hours, a procedure which he considers clumsy and unsurgical and very distressing to the patient. In November and December 1898 he performed two vaginal hysterectomies, for malignant disease of the uterus, in which neither ordinary forceps nor ligatures were employed. The forceps used were the electro-hæmostatic forceps, invented by Dr. A. J. C. Skene. The broad ligaments were secured by these forceps, and a suitable current of three minutes' duration served to cook and desiccate all the tissues within its grasp. Both patients made rapid and complete recoveries, the temperature never reaching 100° at any time after the operation.

THERAPEUTICS.

UNDER THE CHARGE OF

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The Nature of the Antagonism between Toxins and Antitoxins.

Martin and Cherry (*Intercolon. Q. J. M. and S.*, Melbourne, Dec. 20, 1898) contribute an experimental and critical paper on this subject. Behring, Ehrlich, and others maintain that the antagonism is of a chemical nature, and that the antitoxin neutralises the toxin much as an alkali does an acid, while Buchner, Roux, and others maintain that the antagonism is indirect and can only take place through the intervention of cells in the body.

Observations favouring the view that the action of antitoxin is indirect, have been made by Calmette. He found that cobra poison is not weakened by heating its solutions to 68° C. for ten minutes, while the antitoxin is completely destroyed by this treatment. Mixtures of cobra poison and antitoxin which produced no symptoms if injected into rabbits, killed similar animals, if, after being ten minutes in contact, they were heated to 68° C. before injection. He concluded that during the time they were in contact the poison and antitoxin remained quite separate, and that no chemical action took place between them. Wassermann experimented in a similar way with the toxin and antitoxin of the *Bacillus pyocyaneus*, and concluded that these bodies do not interact *in vitro*, but only *in corpore*, and that the action cannot be explained as a simple chemical operation between the two. Nikanarow discovered that cupric acetate solution precipitates the antitoxin, but not the toxin, from a mixed solution of the two. Marenghi made experiments with the toxin and antitoxin of diphtheria, which confirmed those of Calmette mentioned above. There are, however, many observations which favour the interpretation that the action of antitoxin is a direct chemical one exercised on the toxin. Kanthack showed that if cobra poison be mixed with antivenomous serum, it no longer prevented the coagulation of shed blood, and must therefore be changed in some way chemically by the antitoxin mixed with it. Ehrlich found that ricin no longer precipitates blood corpuscles outside the body, if mixed previously with serum from an animal immunised against ricin. Stephens and Meyers found that the hæmolytic action of cobra poison on blood *in vitro* was abolished by mixing it with the antivenomous serum.

Martin and Cherry think that the experiments of Calmette and others are vitiated by the fact that they take no account of time as a factor, and that they did not leave the two substances long enough in contact for a thorough chemical reaction to take place. They show that the experiments made by these authors can easily be repeated and the same results obtained, but that their conclusions are quite unjustified, for by modification of the factors—*time*, *temperature*, and *active masses*—exactly opposite results may also be obtained. They conducted their experiments with the toxin of diphtheria and one of the constituents of the poison of the Australian tiger snake, and their respective antitoxins. Diphtheria toxin can be filtered through a film of gelatine supported in the wall of a Pasteur-Chamberland filter, while the antitoxin does not pass through such a filter. A mixture of the two was placed in such a filter, after remaining in contact for two hours at 30° C., but neither passed through, the inference being that they must have combined in some way.

They repeated Calmette's experiments, but allowed the antitoxin and poison to remain longer in contact outside the body before heating to 68° C. The results were totally different from Calmette's, as the animals remained alive. The only conclusion to be drawn from this is, that during the time which elapsed between the mixture of the venom and the antivenomous serum, the latter had acted on the former, so that there was no longer a fatal dose of venom present. They conclude, therefore, that the action of antitoxins upon toxins is a direct chemical one, and does not depend on changes produced in the body cells.

The Tannic Acid of Hamamelis Virginica.

The tannin of hamamelis bark was isolated by Grüttner in 1898, and is by him regarded as its active principle. Like ordinary tannic acid it is a derivative of gallic acid, and when heated with a 3 per cent. solution of sulphuric acid readily breaks up into its constituents. It is crystalline and soluble only moderately in cold water, precipitates gelatine and albumin, and some alkaloids, from watery solution, and therefore closely resembles tannic acid in its chemical characteristics.

Observations regarding its action have been made by Straub (*Arch. f. exper. Path. u Pharmakol.*, Leipzig, 1899, Bd. xlii. S. 1) on rabbits and dogs. Very large doses (180 grs.) can be given without inducing symptoms of poisoning. It is excreted in the urine as gallic acid, and gives a bluish colour with perchloride of iron. Only when given intravenously could any tannin be detected in the urine. When given by the mouth no trace of it could ever be found in the blood, thus showing that the amount absorbed is small, and must be very rapidly excreted by the kidneys.

Treatment of Chronic Morphinism.

Hirt gave an address on this subject before the 1898 Congress of German Physicians, with especial reference to treatment in the patient's own home. It is necessary, first of all, in each case to determine the causes which have driven the person to opium, and to ascertain the method of taking it and the doses taken. As regards the latter, 90 per cent. of his cases used the hypodermic syringe, the remainder taking the drug in various forms by the mouth. Simple inspection will always reveal the marks of the needle on the skin. The dose taken varies from $\frac{1}{12}$ to 1 gr. from four to twelve times daily. If more than 4 grs. are consumed in the twenty-four hours, Hirt holds that the case should be treated in an institution, where constant attention can be given, as the collapsed condition into which the patient falls on withdrawal of the drug cannot be efficiently treated at home. When moderate doses are habitually taken, the author thinks that the best plan is to withdraw the morphine completely, but during this time the patient must never be left alone. After the first four days, the worst is over for the time being at least. Only a moderate amount of alcohol should be allowed. The patient should be kept asleep as much as possible, by means of large doses of chloral hydrate or trional. The food should be light and nourishing. Warm baths induce sleep and lessen the nervous restlessness. He subsequently treats the case by means of hypnotic suggestion. The patient is slightly hypnotised for rather less than an hour twice daily, and during this time the suggestion is made, and repeated perhaps ten times, that morphinism is a dangerous practice, and brings many evils in its train. Treatment lasted from twenty-one days to eight months in his cases. The immediate prognosis is good, but relapses sooner or later are very common.

Organotherapy.

Hillemand has published a short pamphlet (Paris, 1899) on this subject, and sums up his conclusions as follows:—(1) Treatment with thyroid preparations is the only method of this kind, the efficacy of which has been established with certainty. (2) The innumerable experiments with other glands or tissues have as yet led to no certain conclusions; nevertheless, the value of the thymus, pituitary body, suprarenal capsules, testicles, liver, and nervous tissues, has some serious testimony in its favour. (3) The theory of internal secretion has only been definitely proved in the case of the thyroid and liver. (4) It is, however, probably true also as regards the thymus, the pituitary body, the suprarenal capsules, and the spleen. (5) There is no proof of internal secretion in the case of the pancreas, the kidneys, the prostate, the mammæ, the testicles, and the ovaries. Such proof may be forthcoming on further investigation, and it is not improbable. (6) There is also no proof of internal secretion in the case of the lungs, bone-marrow, muscles, cartilage, nerve-tissue, etc., but it is also highly improbable and rests only on analogy. (7) Even if a certain therapeutic efficacy be proved for the testicles, bone-marrow, or nerve-tissue, it need not necessarily depend on an internal secretion. Their action may be antitoxic or simply tonic apart from their secretion.

MEDICAL JURISPRUDENCE.

UNDER THE CHARGE OF

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SYNCOPE DURING CONFINEMENT.

DR. PERRIN DE LA TOUCHE (*Ann. d'hyg.*, Paris, April 1899), at a recent meeting of the Medico-Legal Society of France, recorded the following case of syncope in a mother at the moment of confinement.

The facts are interesting, since in charges of infanticide, where the child is found dead without any marks of violence upon the body, a frequent plea put forward for the defence is, that the mother lost consciousness at the moment of delivery, and that the child succumbed to natural causes, in consequence of its being deprived of necessary assistance. This defence is not often admitted, but the following case shows that the plea should not be thrust aside without due consideration.

A primipara, who had not concealed her pregnant condition, was seized with the pains of labour while her neighbours were unfortunately absent from their houses. The fœtal head appeared at the vulva, and she had just sufficient strength to throw herself upon her bed. The delivery then made slow progress. After waiting for a time, which appeared interminable to her, she was seized with fear, and thought she was going to die, because the head of the infant could not pass through the vulva. She thereupon decided to enlarge the vulvar orifice. She took a pair of scissors and introduced one blade as deeply as she was able between the inferior portion of the vulva and the child's head, taking care to guide the blade with her finger so that no injury might happen to the child. She then closed the scissors, using both hands, and divided the perinæum. The pain caused her to lose consciousness. When she came to herself again, she found the child, together with the placenta, lying between her thighs. She hastened to put a ligature round the cord, and in afterwards wrapping the child in the bed-clothes she noticed that it was dead. The neighbours came in at this moment, and she told them what had happened. An examination of the perineum showed a clean incision stretching from the vulva to the anus, which did not, however, divide the sphincter or the rectum. Looking to the weak state of the mother at the time of her confinement, and the exceptional circumstances in which she found herself, her story bore the impress of truth. The child had died from accidental asphyxia while lying between the thighs of the mother.

UNUSUAL APPEARANCE IN A TORN UMBILICAL CORD.

TISSIER (*ibid.*, January 1899) records a case of precipitate birth, where the cord was found to have ruptured close to the umbilicus. The divided ends of the cord presented the peculiarity that they had the appearance of having been cut by a pair of scissors. There was no irregularity of the amniotic sheath or of Wharton's jelly, and the umbilical vessels did not protrude. A tear presenting such features is very exceptional, the usual form of the torn ends of the cord in cases of spontaneous rupture being irregular and oblique. If the birth had not taken place in the presence of several witnesses, one would have been inclined under the circumstances to suspect that a sharp instrument had been used.

Two Cases of Lightning-Stroke.

Dr. Bauer (*München. med. Wchnschr.*, No. 3, 1899) records two cases of lightning-stroke. A man and a boy took shelter from a storm, underneath a tree. A witness of the occurrence states that all at once he saw what he describes as a sea of flame, and both the man and the boy were suddenly thrown to the ground. The man lay stiff and still, but the boy began to kick with his feet, and after about five minutes came to himself again. The man was quite dead. The boy states that when he recovered consciousness he remembered nothing of what had happened; but when he saw the man lying dead, he thought that they had been assaulted, and at once attempted to run away, but stumbled and fell to the ground. He experienced a feeling of intense burning in both legs, his right arm below the elbow was powerless, and he had no feeling in it. In walking, he limped, and had to be assisted. On examination, his coat was found perforated in seven places, the holes being from 1 to 3 cms. in diameter, with well-marked singeing of the edges. The coat lining was not affected. On the right elbow there was a burn measuring about 3 in. in length and 1½ in. in breadth. There were also two small burns upon the fore and little fingers of the right hand. Of greatest interest, however, was the tree-like pattern developed upon the skin. This showed itself as a diffuse scarlet coloration of the skin of the calf of the left leg, and passed upwards as a sharply defined branch behind the thigh, round the pelvis, and ended on the anterior aspect of the abdomen in a network like the branching of a tree. Over the back and right buttock a similar appearance was presented. The discoloration gradually faded, and in three days had quite disappeared. The boy completely recovered from all symptoms in the course of a few days.

A post-mortem examination of the body of the man was held. His shirt was burnt near the neck band, and there was a hole in his hat, the edges of which were singed. With these exceptions his clothes showed no signs of injury.

On the right side of the head the hair was markedly singed. The skin of the right cheek was excoriated in several places. The skin of the chest between the nipple and sternum upon the right side was of a bright red colour and covered with numerous excoriations. A similar patch of redness was present in the left inguinal region. The pubic hair was singed. There were several discoloured patches of skin on the back and in the popliteal space of the right leg, together with numerous small burns or eschars of a blackish colour.

The chief internal appearances found consisted in marked softness and anæmia of the brain substance, with meningeal hæmorrhages in different parts.

The surface of the body did not show any of the characteristic tree-like markings.

THE PATHOLOGY OF GAS POISONING.

PROFESSOR LORRAIN SMITH (*Brit. Med. Journ.*, London, 1st April 1899) has recorded the results of an examination of five cases of poisoning by lighting-gas in Belfast. The lighting-gas supplied to Belfast is a mixture of coal-gas with about 40 per cent. of carburetted water-gas. This mixture contains about 16 per cent. of carbonic oxide.

The saturation of the blood in the five cases, two of which were children, was 87 per cent., 76·6 per cent., 69·5 per cent., 57 per cent., and 5 per cent. respectively. In the last case the carbonic oxide had disappeared from the blood, presumably in consequence of the application of artificial respiration. This case, Professor Smith says, is of importance, because it is the first on record in which it was proved that in three hours practically the whole of the carbonic oxide had been eliminated from the blood. The effect of the saturation of the hæmoglobin with carbonic oxide is equivalent to that of depriving a person of oxygen, by decreasing the amount in the air breathed. The patient poisoned by the gas suffers from a form of suffocation. When a person exposed to carbonic oxide is found while still breathing, and placed in normal air, the carbonic oxide is gradually eliminated from the blood, so that after some hours it has completely disappeared. The eliminating force is the affinity of the oxygen for the hæmoglobin, and the higher the percentage of oxygen present the quicker is the elimination. The affinity of carbonic oxide for hæmoglobin is stated to be three hundred times as great as that of oxygen, and the limit of saturation for the circulating blood is reached when its hæmoglobin is so highly saturated with carbonic oxide as to be unable to carry sufficient oxygen to support life.

POST-MORTEM DELIVERY.

LANGERHAUS, in the *Vrtljschr. f. gerichtl. Med.*, Berlin, 1899, Heft 1, describes a case of post-mortem delivery which came under his notice in the mortuary of the Moabit Hospital, Berlin.

The girl, æt. 24, died of phthisis, and the fact of her being pregnant was not noticed during life. When about to commence the post-mortem, the head of a fœtus was found to be protruding from the vulva. In subsequently endeavouring to remove the external and internal genital organs with the child *in situ*, the trunk escaped from the maternal passages. The fœtus, which was fresh but poorly developed, appeared to be about the eighth month. The placenta was detached and lay in the cervix; the uterus was firmly contracted; the os, however, was soft and widely dilated; the vagina was soft and dilated. There was no injury of any of the parts. The body showed no trace of decomposition, and the intestines, both large and small, were empty and not distended with gas.

The interest of the case lies in the absence of any sign of decomposition, to which post-mortem delivery is usually attributed. Langerhaus holds that in all cases of post-mortem delivery the passages have been dilated during life, and that the actual delivery may be caused post-mortem, in the case of an immature child, by such causes as the handling of the mother's body after death, or even through the weight of the child in certain positions of the corpse; if, however, the child be mature, and the head be of considerable size, then delivery can only take place in consequence of the pressure of gases in the abdomen as a result of decomposition.

In all cases of post-mortem delivery, Langerhaus is of opinion that the passages must have been dilated before death, and that decomposition alone is insufficient to produce delivery without such previous dilatation.

INJURIES TO THE VAGINA PRODUCED BY DIGITAL AND INSTRUMENTAL EXAMINATIONS.

CALMANN (*Vrtljschr. f. gerichtl. Med.*, Berlin, 1899, Heft 1) draws attention to the medico-legal importance of injuries of the vagina resulting from digital and instrumental examinations, owing to the liability of such injuries being attributed to criminal violence. He mentions cases from his own practice where the introduction of a speculum or the finger produced tears in the vaginal mucous membrane. The majority of such injuries are produced by the speculum, and Calmann is of opinion that they are much more frequent than is supposed, the seat of the injury varying with the kind of speculum used.

While injuries of the vagina, the result of the use of instruments in delivery, or for criminal purposes, are described in nearly all gynæcological books, he could find no reference to injuries produced in the manner described.

THE WILLIAMS MURDER.

IN the *Boston Med. and S. Journ.*, 10th Nov. 1898, Foster gives an interesting account of a murder in which a man Williams was convicted upon purely circumstantial evidence. On the floor of a hut which had been wholly destroyed by fire, there was found the remains of a human being, but so charred and so nearly consumed that it required a second examination to satisfy one's self that they were human remains. The head and neck were gone and both legs were entirely consumed, bones and all. The spinal column and the shrivelled and charred muscles of the back and some of the viscera, charred and contracted beyond recognition, alone were seen. All the medical evidence that could be furnished in the case was, that it was the body of a human being.

"At first sight it might have appeared that there was very little evidence in such a case of a fire in the night, which destroyed a house and its single occupant, but upon investigation it appeared—(1) That the body was consumed to a greater extent than we should expect from the burning of so small a building, the fleshy portions of the thighs and the large bones composing the hip-joints being entirely consumed; (2)

that the occupant of the house was not burned in his bed, as his remains were some feet away from where the bed-springs were to be seen; (3) that a kerosene oil-can was in the middle of the floor of the bedroom, and close beside the body; (4) that the victim of the burning was not clothed at the time of the burning, as the buckles and buttons of his clothes, and some coins and the clasp of a pocket-book, were found by the side of the bed-springs, and none were found near where the body lay; (5) that the body lay in the doorway leading out of the bedroom, with the head back in the room toward the bed, and not falling forward as a person would naturally fall, if trying to escape from a burning room or building, and it was shown by several witnesses that the fire was first started in the sleeping-room near the bed."

The evidence against the prisoner Williams rested chiefly upon two circumstances, namely, that while the day before the murder he had not money enough to buy a meal or pay a five-cent. fare upon the cars, on the following morning he spent several dollars, and was found with a considerable amount of money upon him when arrested; and, secondly, that in order to account for various injuries upon his body and blood-stains upon his clothes, he gave out a story of his having been "held up" on the night of the fire. It was shown that there were no signs of an encounter, no blood marks, and no unusual tracks upon the side-walk where Williams claimed he was assaulted and robbed.

The injuries found upon the accused were bruises upon the head, finger marks upon the throat; and one finger had a cut upon it which he said was caused by his assailant chewing it. There were a great many blood stains upon his clothing, especially upon the sleeves of his coat, and an effort had evidently been made to get rid of these by washing.

The principal arguments for the defence were—(1) That it could not be proved that John Gallo (the deceased) was really dead, or that the charred remains were not, or might not be, the remains of some other person; (2) that there was no evidence to show that Gallo was killed before the burning occurred, granting that the remains found were actually his; (3) that Williams was not located near the scene of the fire at any time during the night.

DISEASES OF THE EYE.

UNDER THE CHARGE OF

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OPHTHALMIC SURGEON, EDINBURGH ROYAL INFIRMARY;

AND

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SPONTANEOUS ABSORPTION OF CATARACT.

As an extraordinary rarity there appears from time to time a case of spontaneous disappearance of the opacities from a lens which has been quite certainly known to have cataract in a more or less advanced stage. Of course one does not under this heading include those cases in which a patient after a blow, or an illness such as influenza, may acquire a few spicules in the lens, which afterwards disappear, for that takes place not so very infrequently in diabetic patients when put under proper treatment; what one does refer to is the vastly rarer condition in which a cataract advances, ripens, and then disappears. It was not till 1885 that even the possibility of this occurrence taking place was admitted, but in that year Brettauer published three cases. There are, however, two possibilities, either the lens becomes absorbed and thus the cataract is removed, or its transparency is restored to the crystalline. Up to last year fifty cases had been recorded, though in some of them some other pathological condition, such as iritis, had been present at one time, so that, strictly speaking, the number of pure cases is fewer. Schmidt-Rimpler records two instances illustrating the two varieties—the first, in which a shrivelled and partially opaque remnant of lens remained behind, was that of a gentleman of 66, whose right eye had long been blind from cataract, with vision = fingers at 1 M. His left eye had begun to fail, which obliged him to seek advice. On dilatation of the right pupil, vision rose to = fingers at 4 M.; in the central area of the pupil lay a whitish opacity resembling an after-cataract, which towards the periphery faded away into a transparent membrane. There was but a small amount of lens substance enclosed within this capsule; after as complete an extraction as could be accomplished, vision was $\frac{2}{3}$. The author believes that the shrivelled papery lenses seen in some cases of congenital cataract are to be accounted for by a somewhat similar process having taken place. The second case, in which the whole lens was liquefied and carried away, concerned an old man of 81, whose left eye had been operated on for cataract eleven years previously. He was to have returned in six months to have the other eye relieved, but so long as he saw well with the left eye he neglected to come; the development of after-cataract, however, in that eye sent him to Schmidt-Rimpler, who found, to the old man's great astonishment, that with the (right) eye not operated upon, and which he had considered to be of no use, he obtained, by the aid of a lens of + 11 D., exactly twice as good vision as the left eye was capable of. The lower half of this pupil when dilated was found perfectly transparent; across the middle of it, the free lower edge of the lens capsule could be seen curving; in the sac all that could be seen to represent the lens substance were a few white chalky particles. More recently still, Hilbert records the case of a man of 65 in whom this singular self-cure had occurred. The pupil of the left eye, in which he was known to have had cataract, was clear except for a few small white capsular opacities, visible by focal illumination; these formed all that remained of the lens. In the right eye there was Morgagnian cataract, a condition which is believed by some to be the initial stage of spontaneous absorption of the lens.—*Berl. klin. Wchnschr.*, 31st October 1898; and *Die ophth. Klin.*, Stuttgart, 20th March 1899.

Reclination of Cataract.

Even before the introduction of antiseptic methods, the mode of dealing with cataract known as reclination had given place to extraction, and since we have learnt how to keep wounds sweet, so far as is possible inside a mucous sac, no surgeon performs it now, though it is still the favourite operation of the unlicensed practitioner of India and elsewhere. Valude of Paris, however, has employed reclination recently for a patient under peculiar circumstances—an old lady of 73, with ripe cataract in both eyes, but apparently healthy in other respects, underwent extraction of the lens of one eye. The operation, under cocaine, went off quite smoothly until Valude was about to apply the dressing, when all at once, without any straining, coughing, or movement on the part of the patient, and without pain, a hæmorrhage took place from the deeper parts of the eye, completely destroying it. The patient having thus lost one eye, as the apparently unavoidable result of an extraction operation, and the other eye having a ripe cataract, what was the wisest course to adopt? If extraction were performed, the very grave danger was run that what had happened before would happen again, and the second eye be lost. In order to avoid so sudden a reduction of tension, some surgeons in similar circumstances have advocated treating the case as if it were a congenital cataract, breaking it up with the needle and extracting piecemeal, but in a patient of her age, with a nucleus so large and hard, the risk of glaucoma was as bad as the other danger. Valude felt himself shut up to reclination, an operation liable to be followed by many evils, it is true, but how many of those may be avoided by careful antisepsis is unknown. He adopted a method which is said to have come down from ancient Arabian and Egyptian physicians, and made his puncture in the sclera rather than in the cornea. In performing the extraction in the first eye he had observed what a very large part of the lens was in the form of masses of semi-solid cortex, and, lest by misadventure he should tear the capsule of the lens and allow any of this to escape, he first made a minute incision with a small knife, and then performed the actual reclination of the lens with a *blunt* needle. The operation was easy and painless; the lens, after being held down in its new position for a few seconds, exhibited no tendency to re-ascend, and the pupil appeared quite black. There was no pain subsequently, and the patient left hospital in little over a week, able with a suitable glass to read the newspaper; six weeks later the eye was still well and quiet.—*Ann. d'ocul.*, Paris, Jan. 1899.

Hemicrania with Orbital Hæmorrhages.

Apart from cases of injury, hæmorrhages into the orbit are rare, but the peculiar interest of a case related by Brasch and Levinsohn lies not merely in the repeated recurrence of this, as in its association with hemicrania. A man of 23, in good health, suddenly experienced violent pain in the left eye, which swelled up rapidly, and he became squeamish and vomited. Soon the pain passed off, but three months later the same sequence of events was repeated. On examination, there was found to be slight exophthalmos, tension was slightly raised in both eyes, so that the mere touch of the finger was sufficient to produce pulsation in the retinal arteries; at the same time, in the fundus, there was distinct venous congestion; vision was not very good, but there was reason to think it had never been any better; there was some discoloration of the lower lid. A third and fourth attack were similar, but the fifth was more severe, the lids and conjunctivæ were full of blood and much swollen, the left pupil was dilated and inactive to direct light, and the lower half of the field of vision of the left eye was lost. In the patient's general condition nothing pathological was to be found; the attack came on when he was at stool; fortunately, recovery was both rapid and complete, even the lost part of the field being restored. The suggestion made by such a case is in favour of the vascular colic theory of the causation of hemicrania; spasm of vessels, paralysis of their walls, then hæmorrhage from them, is a probable sequence, though, of course, the lesion causing the actual hemicrania must be remote from the orbit. The author states that hæmorrhages in various situations through the body are frequent in hemicrania, but this is not in accordance with the usual experience of the disease.—*Berl. klin. Wchnschr.*, 26th Dec. 1898.

LOCALISATION OF INTRACRANIAL TUMOURS.

At a discussion of the Neurological Society of London on this topic, Gunn, in speaking of the great frequency of optic neuritis in tumour, pointed out that in order of such frequency tumours of the cerebellum come first, then those of the basal ganglia, of the occipital, frontal, and temporo-sphenoidal lobes. He considers that the circumstances which chiefly contribute to its origin and severity are rapid growth, increase of cerebro-spinal fluid, and basal meningitis, these accounting for its particular disposition to occur in cases of cerebellar tumour. He thinks, too, that it is quite possible, though on this subject one cannot be dogmatic, that the unilateral occurrence may sometimes help us to a localising diagnosis, for, so far as he has been able to observe, the farther forward a tumour is, so much the greater is the likelihood that the neuritis will be unilateral. Thus it is more probable—other things being equal—that a right-sided neuritis may be caused by a tumour of the right frontal lobe than by a neoplasm attacking the occipital lobe or *a fortiori* the cerebellum. As a quite different suggestion he indicates that it is also possible that its occurrence may be determined by minor anatomical differences in the orbits and their contents.—*Brain*, London, Autumn, 1898.

ENUCLEATION OF THE EYE.

It is desirable, when the disagreeable necessity of removing the eye entirely does arise, that everything possible should be done to minimise the evil to the appearance of the patient, first by so performing the operation as to do the smallest amount of damage to tissues, and, secondly, by providing the best substitute. To take the second point first, Snellen of Utrecht has made some useful suggestions as to the manufacture of artificial eyes, indicating that in a certain class of cases he finds an eye shaped somewhat like a half-collapsed indiarubber ball, concavo-convex, and with smooth rounded edges, to be the most advantageous. In regard to the first point, there is some difference of opinion among surgeons as to whether or not it is wise to suture the conjunctiva, some believing that by so doing one obtains a better stump, others asserting the exact contrary. Two years ago or so, Schmidt recommended that a slit should be made in the conjunctiva overlying each rectus, and the muscles thus sutured to the conjunctiva, this giving, in his experience, a better, more mobile, and less sunken stump. Priestley Smith attains the same result by the simpler means of picking up each muscle in turn with a needle and thread passed through the conjunctiva, before dividing it. This can hardly be said to complicate the operation at all, and with the result in our own hands we have been well pleased.—*Ophth. Rev.*, London, December 1898 and May 1899.

Learning to see in Adult Life.

Dr. Minor, in the *Transactions of the American Ophthalmological Society*, had the unusual experience of operating for cataract upon an adult who had never been able to see, but who then acquired approximately full vision, after he had been "taught to see." He was a man of 40, shrewd and knowing, though uneducated, who had only perception of light with each eye, but who had made his living as a farm-labourer, besides being an illicit distiller, a horse-dealer, and a successful hunter. He is *said* to have been able to tell the colour and age of a horse by the sense of touch, and his "homing" instinct was extraordinary. After extraction, his impressions were tested in various ways; for example, a square box and a round ball were shown to him separately, but he had at first no idea of their relative shapes; nor had he any idea as to size until allowed to assist sight by touch. He was found unable to designate in numbers also; he could tell whether there was one or more, but could not even, after a good deal of teaching, count beyond five without using his hands. He was able, however, to recognise colours quite readily. When the opaque lens had been removed from the second eye he even obtained binocular vision, apparently without any difficulty.—*Trans. Am. Ophth. Soc.*, 1898.

REPORTS OF THE MEDICAL SOCIETIES.

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Edinburgh Medico-Chirurgical Society.—*3rd May* 1899.—Sir **John Batty Tuke**, President, in the chair.—Dr. **R. A. Fleming** showed specimens of—(1) Typhoid ulceration of intestine from a breast-fed child of ten months; (2) gumma of the brain; (3) waxy degeneration of kidney and liver.—Dr. **Harvey Littlejohn** and Dr. **Claude B. Ker** read a paper on "The History and Clinical Features of an Outbreak of Typhus Fever," part of which appears in this number.

17th May 1899.—Sir J. **Batty Tuke**, President, in the chair.—Mr. **Shaw M'Laren** showed—(1) A patient after operation for congenital torticollis; (2) a patient after operation for ruptured bladder; (3) a patient after operation for tumour of the bladder.—Mr. **Caird** showed—(1) A patient from whom he had removed a carcinoma of the pylorus three years ago; (2) a patient on whom he had performed gastro-enterostomy for supposed carcinoma of the pylorus, with secondary deposit in the lymphatic glands. Two years later the patient was operated upon for ventral hernia, and it was discovered that all the induration and glandular enlargement had disappeared. The case may have been simply an inflammatory deposit, or a true carcinoma, which disappeared as these tumours occasionally do.—Dr. **Affleck** showed—(1) A case of acute diabetes mellitus in a boy æt. 13; (2) a case of diabetes mellitus in an adult who also has peripheral neuritis and optic atrophy; (3) a case of diabetes insipidus in a boy of 16.—Dr. **M'Gillivray** showed two patients on whom he had operated for ruptured gastric ulcer.—Dr. **James** showed—(1) A case of cirrhosis of the liver, with abdominal and thoracic effusions communicating; (2) a case of adult rickets in a man æt. 23.—Dr. **Allan Jamieson** showed a patient in the early stage of tropho-neurotic leprosy.—Dr. **Russell** showed—(1) Two specimens illustrating one of the unusual positions of the vermiform appendix; (2) specimens from a case of perityphlitis due to fæcal ulceration of the cæcum.—Dr. **Fleming** and Dr. **Welsh** exhibited specimens showing tubercular disease in various organs.—Mr. **Caird** showed—(1) A nodule of scirrhus removed from the breast nine years after the first operation; (2) drawing from a case of mesenteric cysts; (3) cystic tumour of testicle; (4) uterine fibroid which had undergone malignant degeneration, and which was removed by Kelly's method.—Mr. **Cathcart** showed a stomach with three ulcers on its walls; one had perforated and was repaired by operation, but the patient died a few hours after of hæmorrhage from a second ulcer.

Edinburgh Obstetrical Society.—10*th May* 1899.—President, Dr. **Halliday Croom**, in the chair.—**Specimens** were shown by the **President**, Professor **Simpson**, Dr. J. W. **Ballantyne**, Dr. **Lackie**, and Dr. **Fordyce**. **Communications**.—Dr. **Lucas**, Dalkeith, read a note of "A Fatal Case of Œdema Glottidis during Pregnancy." The patient suddenly developed pain in the larynx, without any apparent cause, and speedily breathing became difficult and stridulous. She became extremely ill, and tracheotomy was strongly indicated, but this operation both the patient and her husband absolutely refused. She died, and no post-mortem examination was allowed.—Dr. **Lamond Lackie** read a paper on "Puerperal Hyperpyrexia," which appears in this number of the Journal.—Dr. **Haig Ferguson** read a paper entitled "Observations on Labour complicated by a Minor Degree of Pelvic Contraction in Primiparæ and Multiparæ." He recorded in this connection a case of a patient who with a conjugate of 3½ in. had with her first two children easy labours, but with the subsequent three, forceps were necessary, and the labour was terminated only with great difficulty. He accounted for this by the fact that, as the muscular tone of the patient was good, during her first two pregnancies the head was kept low down and developed really through the brim, but during her subsequent pregnancies the head remained above the brim, and had to be brought through this by instrumental aid.—Dr. **Fordyce** read "Notes on Two Cases of Peritoneal Effusion—(*a*) Tubercular, (*b*) Malignant, with Microscopic Preparations." In the first case the condition was undoubtedly tubercular, but in the second, although clinically the appearances were malignant, the microscopic specimens and the after-history of the case threw doubt on the question. The patient had improved very much, and was still alive, and Dr. Fordyce had failed to get a pathologist to say definitely that the specimen removed was undoubtedly malignant.

Glasgow Pathological and Clinical Society, 8*th May* 1899.—Dr. **Barr**, Vice-president, in the chair.—Dr. J. **Lindsay Steven** showed specimens from three cases of mitral stenosis, illustrating different phenomena and complications of that lesion. In all a well-marked stenotic condition was present at the mitral orifice, with dilatation and hypertrophy of the left auricle, right ventricle, and right auricle. In the third case (that of a boy who died from cerebral embolism) vegetations were noticed around the valve. After detailing the clinical history of each case, the following points were specially emphasised:—(1) The relationship of pregnancy to this condition. Two of the cases were women; in one the mitral lesion was possibly present from childhood, the other developing subsequent to an attack of rheumatic fever in adult life. In both, hæmoptysis was present during pregnancy; in both, abortion had occurred. (2) Hæmoptysis.—In all three this was present, being specially severe in the case of the boy, where, owing to the degree of the obstruction, greater tension existed in the lung blood vessels. (3) The occurrence of embolism and thrombosis.—In one of the cases there was great œdema of the right upper limb, due to thrombosis of the right subclavian and innominate veins; in another, embolism of the right middle cerebral artery. (4)

The murmur present. In two of the cases a pronounced presystolic murmur was very evident, in the other it was never definite. (5) Causation of the murmur. Dr. Steven believes the murmur to be due to contraction of the left auricle and to nothing else.—Dr. ALEC ROBERTSON showed a man suffering from a spasmodic disorder, chiefly affecting the muscles of the right upper limb and the right side of the neck. The patient is a wright, and attributes his illness to overwork. The muscles of the right upper limb were first affected, almost a year elapsing before the muscles of the neck became involved. There is no spasm when at rest. Dr. Robertson regarded the condition as one of functional disorder of the cells of the anterior cornua, and chiefly affecting the fifth, sixth, and seventh cervical nerves.—Mr. PARRY showed—(*a*) The bones of a foot after a Mikulicz's operation. The patient died some time afterwards from tubercular meningitis. (*b*) A large renal calculus, removed from a patient on whom nephro-lithotomy was performed on the opposite side ten years ago.

FORFARSHIRE MEDICAL ASSOCIATION, 3*d May* 1899.—Dr. MACKIE WHYTE, Vice-president, in the chair.—Professor KYNOCH showed a uterus removed by vaginal hysterectomy, for cancer of the cervix.—Dr. BUIST showed drawings of a liver from a case of acute yellow atrophy, and of microscopic sections of it, and the disease was then considered from the histological side and also the clinical side. The multiplication of bile ducts, he thought, was often only an apparent increase. He quoted a case he had under his care which simulated acute yellow atrophy of the liver, but proved to be toxæmia from internal obstruction.—Professor STALKER communicated a case of severe anæmia, in which there were convulsions and exaggeration of the deep reflexes in the upper and lower extremities, bilateral sacral bedsore, double optic neuritis. In less than a week improvement commenced, the exaggeration of the deep reflexes improved first in the lower extremities. The hallucinations which existed disappeared. The optic neuritis disappeared, and the patient was steadily improving.—Dr. MACKIE WHYTE read a paper on the cases of chronic tuberculous peritonitis that had been under his care in the Dundee Royal Infirmary during the last five and a half years. They were eleven in number; nine were dead; one was in a hopeless condition, and one had recovered completely. The single case in which laparotomy had been performed had died, and though most of the cases seemed unsuitable for operation, when admitted, still it was thought it might be more frequently tried. Most of the cases lived four and a half months on an average after the worst of symptoms, two for one and a half or three years. Family tuberculosis was present in seven. In three cases there were marked pleuritic symptoms, and in three old tuberculosis of the lungs. The principal signs and symptoms were reviewed, and various points in the diagnosis and treatment were discussed.—Mr. GREIG then continued his communication on "Two years' work in the surgical outpatient room of the Infirmary."

ANALYTICAL REPORTS.

HODGKINSON'S ELECTRO-NEUROTONE.

THE Electro-Neurotone is an invention of considerable interest and of great possible value. Practically it is an apparatus fitted for performing electrical massage, using the faradic current. The usual method, by means of sponges or other electrodes, which require to be damped with water before their application, is often attended by discomfort to the patient from spilling of the water, apart from the local painful sensations caused by the alternations of the current. The Electro-Neurotone consists of a faradic coil and alternator, and two electrodes in one instrument, furnished with a handle springing from the top of the body of the instrument, that is, from the back of the electrodes. The electrode or under surface varies in shape in the different types of the instrument, but essentially is in the form of two broad smooth metal plates of equal size, and separated by an insulating strip from one another, through which the current is conveyed to the skin. The plates must be kept dry and clean, only requiring to be warmed by hand friction before actual application. The electricity is produced by a dry cell, and conducted to the neurotone by insulated wires. After the cell has been connected, all that is necessary is to grasp the neurotone by the handle, apply the metal plate to the skin, either keeping it stationary or moving it over the skin surface as desired. The current passes from plate to plate, and is so generated as to cause no pain on application.

The apparatus certainly appears to possess the great advantages of being simple, easy to work, light in weight, and of small bulk, while its use has proved successful in many suitable cases without causing any disagreeable sensations. A practical instrument of this kind must appeal to the busy practitioner, some of whose greatest trials have often arisen from the difficulties attendant upon the satisfactory carrying out of electrical treatment in patients' houses, if not in his own.

TASTELESS PILLS.

(Cox and Co., Brighton.)

WE have had occasion in previous numbers to commend the Tasteless Pills manufactured by Arthur Cox and Co., on account of their elegant appearance, and the skilful combinations offered by them, as well as the absence of any tendency to harden or become stale on keeping. It is now forty-five years since the founder of the firm of Cox and Co. was granted a patent for his process of coating pills, so as to render them tasteless; the patent expired some years ago, but the firm still confines itself entirely to pill-making; no other products are attempted. The details of manufacture are very different to one's usual conception of the process of making a pill. Machinery of a size almost ludicrously out of proportion to the bulk of a single pill, grinds, mixes, presses, cuts, and rolls the requisite materials. One large press can turn out a hundred gross of pills each time. The tasteless coatings are put on by members of the firm only.

Since the new edition of the British Pharmacopœia was issued last year, Messrs Cox and Co. have introduced a number of new formulæ, necessitated by the official alterations in the composition of many pills. The fact that there must needs be a large number of stock pills fashioned according to the old pharmacopœia in existence, and differing from those of the new edition, should be remembered when prescribing pills such as are manufactured in wholesale. One is more apt to forget this fac- in such a connection than when writing out an extemporaneous prescription. The addition of the 1898 formulæ to Cox's list should be noted.

Among the individual pills which have come under our notice we especially commend Pil. Aperiens, Pil. Extracti Ergotæ, and Pil. Phosphori Composita.

MATHER'S PLAISTERS.

(William Mather Limited, Manchester.)

A NUMBER of samples of these first-class preparations have been sent us. We are not acquainted with any more satisfactory and elegant *emplastra* in the market. The belladonna plaisters, made from extract of the root, with a guaranteed alkaloidal strength of 0·5 per cent., as directed in the British Pharmacopœia of 1898, are very commendable, especially that which is spread upon swansdown; while the variety in which the basis is combined with indiarubber, and requires neither the application of heat nor of moisture before application, is useful.

The rubber adhesive simple plaister possesses a most tenacious power of grip, and can be used without preliminary preparation for the purpose of affording support, or for strapping, without fear of any loosening or detachment occurring. Other classes of plaisters supplied by the firm are Emp. Calefaciens, Roborans, Adhæsivum, Optimum, Capsici, and strengthening plaisters of various kinds; all of them are of the best manufacture and finish, and as perfect as could be wished for.

WHEATLEY'S HOP BITTERS.

(Wheatley and Bates, Ltd., Sheffield.)

WHEATLEY'S Hop Bitters taste uncommonly like Bass's beer, though perhaps the absence of all but a mere trace of alcohol allows the genuine bitter flavour of hops to be better appreciated. Perhaps we may corroborate the manufacturers' statement, and agree that it is the only non-alcoholic substitute for alcoholic liquors we have as yet met with which does not prove mawkish or cloying to the palate when it is taken on several occasions. The Hop Bitters effervesces strongly on removal of the cork, is pale yellow in colour, like beer; when shaken up in the bottle, the presence of hops in it is shown unmistakably. The reaction is acid both before and after expulsion of the carbonic acid by boiling, the specific gravity 1012 after ebullition, while a considerable proportion of native proteid can be detected in it. There seems to be not a trace of alcohol, no other bitter than hops, and a moderate amount of dextrose, Fehling's solution being readily reduced.

To sum up, in Hop Bitters we have an excellent beverage, equal in flavour and body to the best beers, but free from intoxicating properties, from which the tonic and stimulating actions of hops on the body and on gastric digestion can be made use of without fear of evil result.

OBITUARY.

JOHN MOIR, M.D., F.R.C.P.Ed.

ALMOST every stage of the career of Dr. JOHN MOIR presents features of picturesque interest. He first saw the light in a French prison, for his father—a naval surgeon—had been captured during the Napoleonic wars, and his wife going out to share his captivity, their eldest son was born at Verdun in 1808. He had a vague remembrance of seeing the Emperor and Empress pass through the camp, and he knew that his mother headed a deputation to Josephine, praying that the British prisoners should be exchanged or allowed greater liberty. When liberty came, Dr. Moir returned home and established himself in practice in Edinburgh. His little boy entered the High School, and afterwards passed on to the University.

In those days the regulations of the University were less rigid than they are now, and so he was able to take his M.D. when he was 20; and after a visit to the United States he joined his father in practice, and became assistant to Professor Hamilton. This no doubt contributed largely to success. He often lectured for the Professor, and discharged the whole duties of the Chair during the period that elapsed between the death of Hamilton and the appointment of Sir James Simpson. He served the Maternity Hospital faithfully in his day, and was for long Consulting Physician.

While specially great in the department of obstetrics, he was a sensible and sound practitioner in surgery and in medicine. He made one of the early successes in ovariotomy, and could use the knife with no little skill. I remember that when he was past 85 he learned to determine the amount of urea discharged daily, when he found that it was deemed important in regard to the management of the case of one of his patients. He remained capable of taking in new ideas to the last, in a way that I have scarcely ever known anyone do at his time of life.

His character was a noble one, simple, manly, and straightforward; but those who knew him well could see how with years it became more mellowed, refined, and generous. He was interested in all that is good, and was one of the original members of the Edinburgh Medical Missionary Society when it was founded by Abercrombie, Chalmers, and Alison. In politics he was always a sturdy and somewhat advanced Liberal. He cast in his lot with Chalmers when the separation from the State occurred at the Disruption, and he was an elder in Free St. Andrew's Church for a good deal more than fifty years.

If he had enemies he must have long outlived them all. His magnificent constitution and astonishing endurance are rarely equalled; and even when he was past 90, and had long been the father of the profession in Edinburgh, there remained much of the stately manliness which no doubt must have helped him in his earlier days.

T. G. S.

MONTHLY NOTES
ON METEOROLOGY AND VITAL STATISTICS.

By A. LOCKHART GILLESPIE, M.D., F.R.S.E., F.R.C.P.Ed.,
MEMBER SCOTTISH METEOROLOGICAL SOCIETY.

METEOROLOGICAL MEANS FOR THE EAST OF SCOTLAND DISTRICT, FOR THE FOUR WEEKS ENDING APRIL 29, 1899; AND FOR THE YEAR FROM JANUARY 1.

	TEMPERATURE.			RAINFALL IN INCHES.		SUNSHINE. HOURS.		Mean Daily Variability of Temperature.
	Mean.	Accumulated Day-Degrees.		Actual.	Accumulated from Jan. 1.	Actual.	Accumulated from Jan. 1.	
		Above 42°.	Below 42°.					
Mean	43°·6	*[1] 111° [2] 244°	88° 619°	1·8	9·2	125	334	2°·76
Observed	42°·4	[1] 103° [2] 252°	86° 606°	3·2	11·6	125	365	2°·8
Difference	−1°·2	[1] −8° [2] +8°	− 2° −13°	+1·4	+2·4	± 0	+31	+0°·04

VITAL STATISTICS.

MORTALITY IN FOUR LARGE TOWNS IN THE EAST OF SCOTLAND, DURING APRIL, AND SINCE JANUARY 1. Actual Numbers and Rate per 10,000 per annum.

	TOTAL.		CONSTITUTIONAL DISEASES.		CIRCULATORY SYSTEM.		NERVOUS SYSTEM.		RESPIRATORY SYSTEM.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	[1] 1,137 [2] 4,838	199·5 211·6	220 873	38·6 38·2	127 465	22·3 20·3	119 525	20·9 22·3	215 1085	37·7 47·4
Actual Deaths and Rate per 10,000	[1] 1,088 [2] 4,937	190·8 215·9	225 942	39·4 41·2	99 441	17·3 19·3	122 518	21·3 22·6	209 1035	36·7 45·2
Difference from Mean	[1] − 49 [2] + 99	− 8·7 + 4·3	+ 5 + 69	+ 0·8 + 3·0	− 28 − 24	− 5·0 − 1·0	+ 3 − 7	+ 0·4 − 0·3	− 6 − 50	− 1·0 − 2·2

	DIGESTIVE SYSTEM.		URINARY SYSTEM.		PNEUMONIA.		PHTHISIS.		CANCER.	
	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.	No.	Rate.
Deaths expected and Mean Rate for 1891–1897	[1] 63 [2] 269	11·0 11·8	28 116	5·0 5·1	92 387	16·2 17·0	112 447	19·8 19·5	43 185	7·6 8·1
Actual Deaths and Rate per 10,000	[1] 64 [2] 291	11·1 12·7	31 120	5·4 5·2	96 451	16·8 19·7	107 473	19·0 20·6	55 215	9·7 9·4
Difference from Mean	[1] + 1 [2] + 22	+ 0·1 + 0·9	+ 3 + 4	+ 0·4 + 0·1	+ 4 + 64	+ 0·6 + 2·7	− 5 + 26	− 0·8 + 1·1	+ 12 + 30	+ 2·1 + 1·3

* NOTE.—The figures [1] denotes the data for April, [2] for year to date.

The month of April was cold and very wet. The mean temperature was as much as 1°·2 below the mean for the East of Scotland, the excess of rainfall 77 per cent. of the average total. There were no great extremes of either heat or cold, but the first and fourth weeks were warmest, the second and especially the third (6° below the mean) colder than the average. The number of hours of bright sunshine was normal, as was the daily variability of temperature. East and north-east winds were more prevalent than usual.

The temperature figures for the year to date are almost equal to the average, but the rainfall is 2·4 in. above that expected, or nearly 26 per cent., although the hours of bright sunshine amount to nearly 10 per cent. over the mean.

The total deaths registered during April are below the average, especially those due to respiratory and circulatory causes; none of the other groups show very marked increase except cancer, with almost 28 per cent. excess. Influenza caused twenty-five deaths.

For the first four months of 1899 the total number of deaths is above the mean, especially from constitutional and digestive diseases, and from pneumonia, phthisis, and cancer, the latter with 16 per cent. Respiratory cases show the greatest defect.

April, therefore, characterised by cold, raw, and wet weather, shows a diminished mortality, especially from circulatory diseases. The year up to date, with more rain than usual, but no deficiency of heat, gives a higher mortality than that expected. The deaths from cancer in April were very numerous, for the first four months of the year well above the average.

The deaths from influenza for these months have been four, forty-two, forty-five, and twenty-five respectively.

INDEX.

PAGE
ABDOMEN, Chylous Cyst of the . 305
Abdominal Aneurysm, A Case of, by C. E. BEEVOR . 42
,, Tumours, Mistaken Diagnosis of . . 522
Abortion, The Curette in . . 190
Abscess, Subdiaphragmatic, consequent upon Perforating Appendicitis 305
Acromegaly, Lesions associated with 402
Actualités médicales. *Review* . 607
Acute Mania following Simple Ovariotomy, by J. HALLIDAY CROOM 470
Adamkiewicz, A., Die Functionsstörungen des Grosshirnes. *Rev.* 178
Addison's Disease in Children . 92
Adult Life, Learning to See in . 632
Albumosuria 404
Alkaptonuria 294
Amœba, How to Obtain a Pure Culture of 84

Analytical Reports—

Fry's Pure Concentrated Cocoa, 100; Fry's Malted Cocoa, 100; Protene Foods, 100; Tannigen (Triacetyl of Tannin), 102; Tannopine, 102; Klosterle Mineral Water, 212; Eucryl, 212; Hommel's Hæmatogen, 213; Tannalbin, 213; Turtle Soup, 214; Möller's Hydroxyl-Free Cod-Liver Oil, 323; Chlorobrom, 323; Heroin, 324; Phenalgin, 324; Royal Leamington Spa Mineral Waters, 325; Antidiphtheritic and Antistreptococcic Serums, 437; Sparklets, 438; Tabloid Krameria and Cocaine, 438; Johannis Lithia Water, 540; Elixir Phosphori, 540; L'Eau d'Orezza, 541; Nuclein Solution, 541; Tabloid Fluorescin, 541; Eucasin and Guajacetin, 542; Hodgkinson's Electro-Neurotone, 636; Mather's Plaisters, 637; Wheatley's Hop Bitters, 637; Cox's Tasteless Pills 636

PAGE
Antitoxins 525
Antrum of Highmore . . . 188
Appendicitis, Deceptive Remission of Symptoms in . 518
,, Subdiaphragmatic Abscess consequent upon Perforating . 305
Arsenic, Action of, on the Bone-Marrow and Blood . . . 193
Asch Operation 200
ASHBY, HENRY, On the Use and Abuse of Chlorate of Potash in the Treatment of Disease . 25
,, On the Value of Milk Whey in Infant Feeding . . . 389
Asthenic Paralysis . . . 404
Asthma, Clinical Notes on Treatment of . . . 415
,, Sexualis and its occurrence in the Insane, by F. G. CROOKSHANK . . . 588
Athetosis, Double, from Infantile Cerebral Palsy 93

BACTERIOLOGY in connection with Eye Disease 314
BALFOUR, A., and PORTER, C., A Research into the Bacteriology of Typhus Fever: Preliminary Notice 141
BALFOUR, A., Masters of Medicine. *Review* 508
BEEVOR, C. E., A Case of Abdominal Aneurysm bursting into the Psoas Muscle, and subsequently into the Left Pleural Cavity . . . 42
,, Diseases of the Nervous System. *Rev.* . 172
Bell, Joseph, Notes on Surgery for Nurses. *Review* . . . 611
BERRY, G. A., On the Treatment of some of the More Common Eye Affections . . . 333

PAGE

Berry, G. A., Atlas of External Diseases of the Eye, by A. Maitland Ramsay. *Review* . . 504
Birth, The Cord and the Period of Survival after 196
Bladder, Extroversion of the, Final Results of Operations for . 74
Blastomyces 86
Bollinger, O., Pathological Anatomy. *Review* 67
Bone-Marrow and Blood, Action of Arsenic on 193
Bramwell, Byrom, The Genesis and Dissolution of the Faculty of Speech, by J. Collins. *Rev.* . 392
Bramwell, J. M., Hypnotism and its Application to Practical Medicine, by O. G. Wetterstrand. *Review* . . . 176
Broadbent, Sir W. H., Lectures on Giddiness and on Hysteria in the Male, by Sir T. G. Stewart. *Review* 59
Brockbank, E. M., Causation and Rhythm of the Crescendo Murmur of Mitral Stenosis . 236, 341
Brocq, L., Traitement des dermatoses. *Review* 509
Bronchoscopy 201

Calculi in the Region of the Prostate 297
Campbell, H., Respiratory Exercises in the Treatment of Disease. *Review* 286
Camphor Poisoning . . . 81
Cancer of the Lip . . . 618
,, Patients, Bone-Marrow of 306
,, Vaginal Hysterectomy for, by J. Halliday Croom . . . 249
Carbolic Acid Gangrene . . 82
Castration for Rudimentary Uterus 190
Cataract, Reclination of . . 630
,, Spontaneous Absorption of 628
Caton, Richard, On the Prevention of Valvular Disease of the Heart 329
Central Nervous System, Peculiar Bodies found in the . . . 307
Chlorate of Potash in the Treatment of Disease, On the Use and Abuse of, by H. Ashby . . 25
Chloroform Sickness, by T. A. Ross 591
Chorea, Clinical Considerations in, by J. Purves Stewart . . 257
Chronic Splenic Enlargement with Recurring Gastro-Intestinal Hæmorrhages, by William Osler 441

Chylous Cyst of the Abdomen . 305
Cirrhosis, A Minute Micro-Organism in Cases of Progressive Portal 307
Clinch, T. Aldous, *see* Jones . . 363
Collins, J., The Genesis and Dissolution of the Faculty of Speech. *Review* 392
Colon, Hypertrophic Tuberculosis of the 304
Consumptives, Treatment of, in Sanatoria 414
Cord, The, and the Period of Survival after Birth . . . 196
Corpulence, Treatment of, with Thyroid Preparations . . 413
Cosaprin and Phesin . . . 414
Coxa Vara 408
Crookshank, F. G., Asthma Sexualis and its Occurrence in the Insane 588
Croom, J. H., Vaginal Hysterectomy for Cancer . 249
,, Transactions of Glasgow Obstetrical & Gynæcological Society, vol. i. *Review* . . 288
,, On Acute Mania following Simple Ovariotomy . 470
Cystic Adenoma of the Pancreas . 295
Cystitis, Instillations in the Treat-of, by A. A. Warden . . 50

Davidson, A., Ueber Malaria, by Hans Ziemann. *Rev.* 65
,, Lectures on the Malarial Fevers, by W. S. Thayer. *Review* . 282
Decidual Cell, The Function of the, by W. E. Fothergill . . 265
Delbet, P., Leçons de clinique chirurgicale. *Review* . . 401
Dermatitis from Boric Acid and Borax 303
Diabetes, Ocular Troubles in . 313
Digitalis Leaves, The Active Constituents of 194
Diphtheria Bacilli and Diphtheria in Scarlet Fever Cases 206
,, Pathogenesis and Prevention of the Serum Exanthemata observed in the Antitoxin Treatment of . . . 88
Duckworth, Sir Dyce, Gout, by A. P. Luff. *Review* . . . 61
Dunlop, G. H. M., Traité de l'Allaitement et de l'Alimentation des enfants, by A. B. Marfan. *Review* . 287
,, Diseases of Children, by J. F. Goodhart. *Rev.* 609

PAGE

Duodenum, Perforating Ulcer of the 296

EARL, ALFRED, The Living Organism. *Review* 173
Ectopic Pregnancy repeated in the same Patient, by J. H. FERGUSON 145
Embolism following Placenta Prævia 80
Enteric Fever, Perforation in, with Notes of ten Cases, by G. P. YULE 358
Enterostomy, The Treatment of the Intestinal Paralysis of Peritonitis by 409
Epilepsy, Pathology of. . . . 417
,, Resection of the Cervical Sympathetic in . 405
Erasion *versus* Excision in Childhood, by A. E. MAYLARD . . 577
Ergot in Chronic Malaria . . . 524
Exanthemata, Serum 88
Exophthalmic Goitre, Resection of the Cervical Sympathetic in . 405
Eye Affections, On the Treatment of some of the More Common, by G. A. BERRY . 333
,, Disease, Bacteriology in connection with . . . 314
,, Enucleation of the . . . 63

FALLOPIAN TUBES, Tuberculosis of the Uterus and 81
FERGUSON, J. H., Repeated Ectopic Pregnancy in the same Patient . 145
Flagella, Staining of 85
FLEMING, R. A., Diseases of the Nervous System, by C. E. Beevor. *Review* 172
Food Preparations 179
Formaldehyde, Practical Disinfection by 203
Formalin in Sweating of the Feet . 303
FOTHERGILL, W. E., The Function of the Decidual Cell 265
Fractures, Treatment of, by Weight-Extension 76

GALL STONES, Results of 360 Laparotomies for 520
Gas Poisoning, Pathology of. . . 626
Gastro-Enterostomy, Later Results of 517
,, with Jejuno-Jejunostomy 76
Gastrostomy, The After Treatment of Cases of, by R. H. LUCY . 46
GILLESPIE, A. L., Respiratory Exercises, by H. Campbell. *Review* . 286
,, Practical Uranalysis and Urinary Diagnosis, by C. W. Purdy. *Review* 401

PAGE

GILLESPIE, A. L., A Visit to a German Institution for the Treatment of Pulmonary Disease . 484
,, Transactions of the Royal Academy of Medicine in Ireland, vols. xv. and xvi. *Review* . . . 610
,, Les Actualités médicales. *Review* . . . 607
Glaucoma, Resection of the Cervical Sympathetic in 405
Goitre, Six Hundred Operations for 188
GOODHART, J. F., Diseases of Children. *Review* 607
Gowers, Sir W. R., Diseases of the Nervous System, vol. i. *Review* 396
GREENFIELD, W. S., On a Case of Typhus, and some Lessons to be drawn from it . . . 378, 492
Guyon, J. C. F., Die Krankheiten der Harnwege. *Review* . . 289
Gynæcology, Remarks on the Use of Mammary-Gland, and Parotid-Gland Desiccations in . . 619

HÆMOPHILIA, Joint Lesions in . 186
Halliburton, W. D., Essentials of Chemical Physiology. *Review* . 510
Hamamelis Virginica, Tannic Acid of 622
Hare, J. A., The Cold Bath Treatment of Typhoid Fever. *Review* 609
HAULTAIN, F. W. N., A Retrospect of a first series of one hundred and ten Abdominal Sections for Pelvo-Abdominal Disease . . . 105
,, Diseases of Women, by G. E. Herman. *Rev.* 284
,, Operative Gynecology, by H. A. Kelly. *Rev.* 397
,, The Science and Practice of Midwifery, by W. S. Playfair. *Review* 399
Hay Fever 199
Heart Disease, A Case showing Heredity in, by G. C. THOMAS . . . 56
,, On the Prevention of Valvular Disease of the, by RICHARD CATON . . . 329
Hemianæsthesia 292
Hemicrania, Treatment of . . 194
Herman, G. E., Diseases of Women. *Review* 284
Highmore, Antrum of . . . 188
Hip Disease, On the Prevention and Correction of Short Leg in . 616

PAGE

Hofmann, E. von, Atlas of Legal Medicine. *Review* . . . 510
Holden's Human Osteology. *Rev.* 281
Holmes, Timothy, Sir Benjamin Brodie. *Review* 506
Hospital Ship "Bay State" in the Spanish-American War . . 534
Hydatid Cysts, Operative Treatment of 519
Hydatidiform Mole, Two Cases of, by James Oliver . . . 502
Hyperleucocytosis and Leucocytosis 83

Idiopathic Atrophy of the Skin 293
Inebriates Act of 1898, by G. R. Wilson 254
Infant Feeding, On the Value of Milk Whey in, by Henry Ashby 389
Infantile Cerebral Palsy, Double Athetosis from 93
Intestine, Rupture of, in the Newly Born 198
Intracranial Complications, Occurrence of, in Acute Suppurative Otitis Media . . . 529
,, Tumours, Localisation of 631
Involution of Life, by R. Maguire 1, 119
Iodine, A New Reagent for Spermatic Fluid 197

Jamieson, W. Allan, Maladie Carrion, by E. Odriozola. *Review* . 606
,, Pellagra, by C. Lombroso. *Review* . 175
,, Traitement des dermatoses, by L. Brocq. *Review* . . . 509
Jejuno-Jejunostomy with Posterior Gastro-Enterostomy . . . 76
Jones, R. L., and Clinch, T. A., Suppurative Cholangitis . . 363
Jones, T., and Kelynack, T. N., Case of Renal Sarcoma successfully removed by Lumbar Nephrectomy 602

Kelly, H. A., Operative Gynecology. *Review* 397
Kelynack, T. N., *see* Jones . 602
Ker, C., Twentieth Century Practice, vols. xiii., xiv. *Review* . . . 290
,, *see* Littlejohn . . 555
,, Cold Bath Treatment of Typhoid Fever, by J. A. Hare. *Review* . 609
Kidney, Catheterisation of the Ureters in relation to the Surgery of the 298

PAGE

Kirkes' Handbook of Physiology, by W. D. Halliburton. *Review* 512

Lackie, J. L., Puerperal Hyperpyrexia, with an Illustrative Case 585
Lane, W. A., Clinical Lectures on Cleft Palate, etc. *Review* . 400
Laryngeal Stridor, Congenital . 428
Lateral Sinus Thrombosis, Some Points in the Diagnosis of . 532
Leaf, C. H., Surgical Anatomy of the Lymphatic Glands. *Review* 70
Leith, R. F. C., Text-book of Pathogenic Bacteria, by J. M'Farland. *Review* . . . 608
,, Transactions of the Association of American Physicians. *Review* 512
Leprosy, Notes on 89
,, Ocular Manifestations of 313
Leucocytosis 83
Lightning-stroke 625
Littlejohn, Harvey, Medico-Legal Cases . . 473
,, Atlas of Legal Medicine, by E. von Hofmann. *Review* . . . 510
,, and Ker, Claude B., An Outbreak of Typhus Fever . . 555
Locomotor Ataxia, Case of, with Cancrum Oris as a Fatal Complication, by R. Barclay Ness 595
Lombroso, C., Der Pellagra. *Rev.* 175
Lucy, R. H., The After-Treatment of Cases of Gastrostomy . . 46
Luff, A. P., Gout: Its Pathology and Treatment. *Review* . . 61
Lumbar Puncture in Children . 429
Lysol Poisoning 79

M'Bride, P., The Treatment of Ozæna, with special reference to Cupric Electrolysis . . 217
M'Farland, J., Text-book of Pathogenic Bacteria. *Review* . . 608
Mackenzie, W. Leslie, Handbook of Hygiene and Sanitary Science, by G. Wilson. *Review* 285
,, The Hygienics of Milk 372, 563
M'Keown, W. A., Unripe Cataract. *Review* 511
Maguire, R., The Involution of Life—A Scheme of Pathology 1, 119
Malaria, Ergot in Chronic . . 524
Malarial Hæmoglobinuria, Quinine in 302
Mammary Gland Desiccations . 619
,, Histological Changes in, during the Secretion of Milk . . . 309

PAGE

Marfan, A. B., Traité de l'Allaitement et de l'Alimentation des enfants. *Review* 287
MAYLARD, A. E., Erasion *versus* Excision in Childhood; or, the Remote Effects of Excising or Erasing Tuberculous Joints in Children 577
Measles, A New Diagnostic Sign of 91
Meat Extracts 182
,, Peptone, Liebig's 181
,, Powders 182
Medical News 326, 539
,, Officers for School Boards 535
Medico-Legal Cases, by HARVEY LITTLEJOHN 473
Meningitis, Bacteriology of the Simple Posterior Basic of Infants 308
Menstrual Molimina, Absence of 190
Metabolism, Action of Alcohol and of Water on 525
Micro-chemical Reactions 312
Micrococcus Tetragenus 306
Milk, Hygienics of, by W. LESLIE MACKENZIE 372, 563
,, Preparations, Rose's 181
,, Removal of the Sediment from 357
Mills, C. K., The Nervous System and its Diseases. *Review* 170
MILROY, T. H., Essentials of Chemical Physiology, by W. D. Halliburton. *Review* 510
,, General Physiology, by Verworn and Lee. *Review* 505
,, Kirkes' Handbook of Physiology, by W. D. Halliburton. *Rev.* 512
,, Stöhr's Text-book of Histology. *Review* 68
Mitral Stenosis, The Causation and Rhythm of the Crescendo Murmur of, by E. M. BROCKBANK 236, 341
Monthly Notes on Meteorology and Vital Statistics, by A. LOCKHART GILLESPIE 103, 215, 327, 439, 543, 639
MORISON, ALEX., On the Relation of the Nervous System to Disease and Disorder of the Viscera 28, 151, 224
Morphinism 623
Morris, Henry, On the Origin and Progress of Renal Surgery. *Review* 611
,, Treatise on Human Anatomy. *Review* 169
MOTT, F. W., The Nervous System and its Diseases, by C. K. Mills. *Review* 170

PAGE

Moullin, C. Mansell, Inflammation of the Bladder and Urinary Fever. *Review* 69
Movable Kidney in Children 425
Multiple Myelomata 404
Murphy's Button in Czerny's Clinique 77

NEPHROTOMY, Nephrostomy, and Nephrectomy 77
Nervous System, On the Relation of, to Disease and Disorder of the Viscera, by ALEX. MORISON 28, 151, 224
NESS, R. BARCLAY, Case of Locomotor Ataxia, with Cancrum Oris as a Fatal Complication 595

Obituary—
George James Allman, 98; Sir William Jenner, Bart., 98; A. A. Kanthack, 210; Sir James Mouat, 211; James Cappie, 211; Joseph Tillie, 211; Henry Alleyne Nicholson, 320; Joseph Coats, 321; W. H. Murray, 322; Thomas Cooke, 322; William Rutherford, 322, 434; Sir John Struthers, 433; Sir William Roberts, 539; John Fenton Evans, 539; John Moir 638
Odriozola, E., La maladie de Carrion. *Review* 606
Old Age (Senilitas Præcox), The Prevention of 413
Oliver, James, Two Cases of Hydatidiform Mole 520
Orbital Hæmorrhages, Hemicrania with 630
Organotherapy 623
OSLER, WILLIAM, Chronic Splenic Enlargement with Gastro-Intestinal Hæmorrhages 441
Otitis Media, Occurrence of Intracranial Complications in Acute Suppurative 529
Ovum, Case of Retained 192
Ozæna, The Treatment of, with special reference to Cupric Electrolysis, by P. M'BRIDE 217

PAGET, STEPHEN, John Hunter. *Review.* 506
Pancreas, Removal of Cystic Adenoma of the 295
Pancreatic Cyst, Retroperitoneal Cyst simulating 304
Paralysis Agitans 293
Paravaginal Space 410
Parotid Gland Desiccations 619
Partial Hæmatomata 410
,, Hemihypertrophy 292

PAGE

Pelvic Abscess, Disadvantages of Vaginal Drainage for 191
,, Tumour, Unusual Course of the Ureter in a Case of 302
Pelvo-Abdominal Disease, Retrospect of a first series of one hundred and ten Abdominal Sections for, by F. W. N. HAULTAIN . 105
Peptone 182
Pericarditis in Children . . 428
Peritonitis, The Treatment of Intestinal Paralysis of, by Enterostomy 409
Phthisis Pulmonum . . . 515
Placenta Prævia, Embolism following . . . 80
,, ,, Treatment of . 620
Plague Toxin 84
PLAYFAIR, W. S., The Science and Practice of Midwifery. *Rev.* . 399
Pneumonia in Children . . 427
Poland, John, Traumatic Separation of the Epiphyses. *Review.* 604
Pope, The Recent Operation on the 618
PORTER, C., *see* Balfour . . 141
Post-mortem Delivery . . . 626
POWER, D'ARCY, William Harvey. *Review* 506
Pregnancy, Combined Extra- and Intra-uterine 522
Procreation? Are Complete Castrates capable of . . . 520
Prostate, Calculi in the region of the 297
Proteosoma, Cultivation of, in Grey Mosquitoes . . . 528
Pseudo-Lupus Vulgaris caused by a Blastomyces . . 86
,, Tuberculosis . . . 308
Puerperal Hyperpyrexia, with an Illustrative Case, by J. LAMOND LACKIE 535
Pulmonary Disease, A Visit to a German Institution for the Treatment of, by A. LOCKHART GILLESPIE 484
Purdy, C. W., Practical Uranalysis and Urinary Diagnosis. *Review.* 401

QUININE in Malarial Hæmoglobinuria 320

RAMSAY, Maitland, Atlas of External Diseases of the Eye. *Rev.* 504
RANSOM, W. B., The Vis Medicatrix Naturæ . . 12, 129

Recent Advances in Medical Science—
Dermatology, by W. ALLAN JAMIESON 86, 421
Diseases of Children, by G. H. MELVILLE DUNLOP 91, 425

PAGE

Recent Advances in Medical Science—
Diseases of the Ear, Nose, and Throat, by P. M'BRIDE and A. LOGAN TURNER 199, 529
,, the Eye, by GEORGE A. BERRY and W. G. SYM . . 313, 628
Insanity, by Sir JOHN BATTY TUKE and JOHN MACPHERSON 417
Medical Jurisprudence, by HARVEY LITTLEJOHN . . 195, 624
Medicine, by A. LOCKHART GILLESPIE 71, 179, 292, 402, 514, 612
Obstetrics and Gynæcology, by F. W. N. HAULTAIN . 79, 190, 300, 411, 521, 617
Pathology and Bacteriology, by R. F. C. LEITH . 83, 304, 525
Physiology, by T. H. MILROY . 309
Public Health, by CHARLES HUNTER STEWART . 203, 534
Surgery, by ALEXIS THOMSON 74, 184, 295, 405, 517, 615
Therapeutics, by RALPH STOCKMAN . 81, 193, 302, 413, 524, 621
Rectum, Gonorrhœal Stricture of the 299
Renal Dropsy, The Treatment of, by NESTOR TIRARD . 545
,, Sarcoma, Case of, successfully removed by Lumbar Nephrectomy, by T. JONES and T. N. KELYNACK . . . 602

Reports of Societies—
Edinburgh Medico-Chirurgical Society, 95, 207, 317, 430, 633
,, Obstetrical Society 95, 208, 317, 634
Forfarshire Medical Association 98, 319, 430, 538, 635
Glasgow Pathological and Clinical Society 96, 209, 319, 432, 634
Royal Medical Society . 97, 209, 318, 431
Retroperitoneal Cyst simulating Pancreatic Cyst . . . 304
Rickets, Phosphorus in . . 524
Rose's Milk Preparations . . 181
Ross, T. A., Chloroform Sickness. 591

SANATORIA, Tuberculosis treated in 612
Saprophytic Organisms, Production of Pathogenic Properties in . 205
Scarlet Fever, Diphtheria Bacilli and Diphtheria in . . . 206
Sciatica and its Treatment . . 616
Senilitas Præcox 314

PAGE

Septum, Asch Operation for Deviation of the 200
Serum Exanthemata . . . 88
Skin, Idiopathic Atrophy of the . 293
SMITH, G. BELLINGHAM, Traumatic Separation of the Epiphyses, by JOHN POLAND. *Rev.* 604
Soap, Disinfecting Power of . 204
Spermatic Fluid, Iodine a new Reagent for 197
Spine, The Forcible Correction of Angular Curvature of the . 407
Stedman, T., Twentieth Century Practice, vols. 13-14. *Review* . 290
Stenosis, New Operation for Enlarging the Uterine Cervical Canal in Cases of . . . 190
STEWART, PURVES, Clinical Considerations in Chorea . . 257
Stewart, Sir T. G., Lectures on Giddiness and on Hysteria in the Male. *Review* . . . 59
„ Contributions to Clinical Medicine, by T. M'Call Anderson. *Review* . 280
STILES, H. J., Surgical Anatomy of the Lymphatic Glands, by C. H. Leaf. *Review* . . . 70
Stöhr's Text-book of Histology. *Review* 68
STOKES, Sir WM., William Stokes. *Review* 506
Suppurative Cholangitis, by R. L. JONES and T. A. CLINCH . . 363
Suprarenal Gland for the Treatment of Hay Fever 199
SYM, W. G., Unripe Cataract, by William A. M'Keown. *Review* 511
Syncope during Confinement . 624
Syphilis, The Inheritance of . . 421
„ Spinal Localisations of Hereditary . . 93

TABETIC ARTHROPATHIES, Operative Interference in . . . 615
Talbot, E. S., Degeneracy. *Review* 173
Testicle, Tumours of the . . 408
Tetanus Puerperalis . . . 300
„ Treatment of . . 82, 524
Tetany 71
„ following Partial Excision of the Thyroid Gland . 404
Thayer, W. S., Lectures on the Malarial Fevers. *Review* . . 282
THOMAS, G. C., A Case showing Heredity in Heart Disease . 56
THOMSON, ALEXIS, Inflammation of the Bladder, by C. Mansell Moullin. *Rev.* 69
„ Clinical Lectures on Cleft Palate, etc., by W. A. Lane. *Review* . 400

PAGE

THOMSON, ALEXIS, Leçons de clinique chirurgicale, by P. Delbet. *Review* . . . 49
„ Notes on Surgery for Nurses, by Joseph Bell. *Review* . . 611
„ On the Origin and Progress of Renal Surgery, by H. Morris. *Review* . . . 611
„ Synopsis of Surgery, by R. F. Tobin. *Review* 610
„ Transactions of the American Orthopædic Association, vol. xi. *Review* . . . 611
THOMSON, ARTHUR, Holden's Human Osteology. *Rev.* 281
„ Treatise on Human Anatomy, edited by H. Morris. *Review* . 169
THOMSON, J. ARTHUR, Degeneracy, by E. S. Talbot. *Rev.* 173
„ The Living Organism, by Alfred Earl. *Review* 173
Thoracic Duct, Injuries of the Cervical Portion of the . . 75
Thoracotomy for Foreign Body in the Bronchus 187
Thymus Gland, Enlargement of the, as a Cause of Sudden Death 195
Thyroid Gland, Tetany following Partial Excision of the . . 404
TIRARD, NESTOR, The Treatment of Renal Dropsy . . . 545
Tobin, R. F., Synopsis of Surgery. *Review* 610
Toxins 525
„ and Antitoxins, Antagonism between . . . 621
Toxoids 525
Transactions of the American Orthopædic Association, vol. xi. *Review* . . 611
„ Association of American Physicians, vol. xiii. *Review* . . . 512
„ Glasgow Obstetrical and Gynæcological Society, vol. i. *Review* . 288
„ Royal Academy of Medicine in Ireland, vols. xv. and xvi. *Review* 610
Tuberculosis and its Prevention, by G. SIMS WOODHEAD. 453
„ in Sanatoria . . . 612
„ Transmission of, by means of the Placenta 81
TURNER, W. ALDREN, Die Functionsstörungen des Grosshirnes, by A. Adamkiewicz. *Rev.* 178

PAGE

Turner, W. Aldren, Diseases of the Nervous System, by Sir W. R. Gowers, vol. i. *Review* . 396
Typhoid Fever in a Child Two Months Old 92
Typhus, Clinical Lecture on a Case of, and some Lessons to be drawn from it, by W. S. Greenfield . 378, 492
,, Fever, A Research into the Bacteriology of, by A. Balfour and C. Porter . . 141
,, An Outbreak of, by Harvey Littlejohn and Claude B. Ker 555

Umbilical Cord, Unusual Appearance in a Torn 624
Ureter, Unusual Course of, in a Case of Pelvic Tumour . . 302
Ureters, Catheterisation of the, in relation to the Surgery of the Kidney 298
Urinary Antisepsis, by A. A. Warden . . . 273
,, Bladder, Septic Infection of the . . 184
Uterine Artery, Aneurysm of the . 620
,, Douching, Lysol Poisoning from . . . 79
Uterus, Castration for Rudimentary 190
,, New Operation for Retroposition of the . 411
,, Simple Method of Plugging the Puerperal . 412
,, Torsion of the . . . 300
,, Treatment of Chronic Puerperal Inversion of the 301
,, Tuberculosis of the, and Fallopian Tubes . . 81

Vaccinal Immunity 536
Vagina, Injuries to the, produced by Digital and Instrumental Examinations 627

PAGE

Vagina, Method of Creating a, in Congenital Absence . 619
,, Rupture of, during Parturition . . . 523
Vaginal Hysterectomy, Bloodless and Aseptic . . 620
,, Secretion of the Pregnant Woman, The Cause of the Conflicting Statements concerning the Bacterial Contents of the . 411
Verruga Peruviana . . . 87
Verworn and Lee, General Physiology. *Review* 505
Vesico-Vaginal Fistula treated by Freund's Method, Four Cases of 521
Vis Medicatrix Naturæ, by W. B. Ransom 12, 129

Wallace, David, Krankheiten der Harnwege, by J. C. Guyon. *Review* 289
Warden, A. A., Instillations and their Value in the Treatment of Cystitis 50
,, Urinary Antisepsis . 273
Weight Extension, Treatment of Fractures by 76
Wetterstrand, O. G., Hypnotism and its Application to Practical Medicine. *Review* . . . 176
Williams (The), Murder . . 627
Wilson, G., Handbook of Hygiene. *Review* . . . 285
,, The Inebriates Act of 1898 254
Woodhead, G. S., Pathological Anatomy, by O. Bollinger. *Review* . 67
,, Tuberculosis and its Prevention . . 453

Yule, G. P., Perforation in Enteric Fever 358

Ziemann, Hans, Ueber Malaria. *Review* 65

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3 2044 103 055 224